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*OTHER DEPARTMENTS*

The American Economic Review

OLUME XLIX JUNE 1959 NUMBER THREE

# THEORIES OF DECISION-MAKING IN ECONOMICS AND BEHAVIORAL SCIENCE

*By* HERBERT A. SIMON*

[*Editor's note:* This is the first of eight survey articles on recent developments in economics scheduled for appearance in the *Review* over the next few years. Financial support of the series has been generously provided by the Rockefeller Foundation. The managing editor is particularly grateful for the personal interest which the late Dr. Norman S. Buchanan, Director for the Social Sciences at the Foundation, took in the planning of the project.]

Recent years have seen important new explorations along the boundries between economics and psychology. For the economist, the imnediate question about these developments is whether they include iew advances in psychology that can fruitfully be applied to economics. But the psychologist will also raise the converse question—whether here are developments in economic theory and observation that have nplications for the central core of psychology. If economics is able to nd verifiable and verified generalizations about human economic beavior, then these generalizations must have a place in the more genral theories of human behavior to which psychology and sociology spire. Influence will run both ways.[1]

## I. *How Much Psychology Does Economics Need?*

How have psychology and economics gotten along with little relaion in the past? The explanation rests on an understanding of the oals toward which economics, viewed as a science and a discipline, ias usually aimed.

Broadly speaking, economics can be defined as the science that

* The author is professor of administration at the Carnegie Institute of Technology. This aper draws heavily upon earlier investigations with his colleagues in the Graduate chool of Industrial Administration, carried out in library, field, and laboratory, under everal grants from the Ford Foundation for research on organizations. He is especially ndebted to Julian Feldman, whose wide-ranging exploration of the so-called binary hoice experiment [25] has provided an insightful set of examples of alternative approaches o a specific problem of choice.

[1] The influence of economics upon recent work in the psychology of higher mental ocesses is well illustrated by Bruner, Goodnow and Austin [14, Ch. 3 and 4]. In this ork, game theory is used to throw light on the processes of concept formation.

describes and predicts the behavior of several kinds of economic r notably the consumer and the entrepreneur. While perhaps literall rect, this definition does not reflect the principal focus in the lite of economics. We usually classify work in economics along two d sions: (a) whether it is concerned with industries and the whole omy (macroeconomics) or with individual economic actors (r economics); and (b) whether it strives to describe and explair nomic behavior (descriptive economics), or to guide decisions ( at the level of public policy (normative macroeconomics) or a level of the individual consumer or businessman (normative n economics).

The profession and literature of economics have been largely occupied with normative macroeconomics. Although descriptive m economics provides the scientific base for policy prescription, rese emphases have been determined in large part by relevance to p (e.g., business cycle theory). Normative microeconomics, carried ward under such labels as "management science," "engineering nomics," and "operations research," is now a flourishing area of \ having an uneasy and ill-defined relation with the profession of nomics, traditionally defined. Much of the work is being done by ma maticians, statisticians, engineers, and physical scientists (althc many mathematical economists have also been active in it).[2]

This new area, like the old, is normative in orientation. Econom have been relatively uninterested in descriptive microeconomics— derstanding the behavior of individual economic agents—except as is necessary to provide a foundation for macroeconomics. The nor tive microeconomist "obviously" doesn't need a theory of human havior: he wants to know how people *ought* to behave, not how tl *do* behave. On the other hand, the macroeconomist's lack of conc with individual behavior stems from different considerations. Fi he assumes that the economic actor is rational, and hence he mal strong predictions about human behavior without performing the ha work of observing people. Second, he often assumes competition, wh carries with it the implication that only the rational survive. Thus, t classical economic theory of markets with perfect competition a rational agents is deductive theory that requires almost no conta with empirical data once its assumptions are accepted.[3]

Undoubtedly there is an area of human behavior that fits these a sumptions to a reasonable approximation, where the classical theo

[2] The models of rational decision-making employed in operations research are surve in Churchman, Ackoff, and Arnoff [16]; Bowman and Fetter [11]; and Vazsonyi [6

[3] As an example of what passes for empirical "evidence" in this literature, I cite pp. 23 of Friedman's *Essays in Positive Economics* [27], which will amaze anyone broug up in the empirical tradition of psychology and sociology, although it has apparently cited little adverse comment among economists.

with its assumptions of rationality is a powerful and useful tool. With-
out denying the existence of this area, or its importance, I may observe
hat it fails to include some of the central problems of conflict and dy-
amics with which economics has become more and more concerned.
A metaphor will help to show the reason for this failure.

Suppose we were pouring some viscous liquid—molasses—into a
owl of very irregular shape. What would we need in order to make a
heory of the form the molasses would take in the bowl? How much
vould we have to know about the properties of molasses to predict its
ehavior under the circumstances? If the bowl were held motionless,
nd if we wanted only to predict behavior in equilibrium, we would
ave to know little, indeed, about molasses. The single essential as-
umption would be that the molasses, under the force of gravity, would
ninimize the height of its center of gravity. With this assumption,
vhich would apply as well to any other liquid, and a complete knowl-
dge of the environment—in this case the shape of the bowl—the equi-
ibrium is completely determined. Just so, the equilibrium behavior of a
erfectly adapting organism depends only on its goal and its environ-
nent; it is otherwise completely independent of the internal properties
f the organism.

If the bowl into which we were pouring the molasses were jiggled
apidly, or if we wanted to know about the behavior before equilibrium
vas reached, prediction would require much more information. It would
equire, in particular, more information about the properties of mo-
asses: its viscosity, the rapidity with which it "adapted" itself to the
ontaining vessel and moved towards its "goal" of lowering its center
f gravity. Likewise, to predict the short-run behavior of an adaptive
rganism, or its behavior in a complex and rapidly changing environ-
nent, it is not enough to know its goals. We must know also a great
eal about its internal structure and particularly its mechanisms of
daptation.

If, to carry the metaphor a step farther, new forces, in addition to
ravitational force, were brought to bear on the liquid, we would have
o know still more about it even to predict behavior in equilibrium. Now
ts tendency to lower its center of gravity might be countered by a force
o minimize an electrical or magnetic potential operating in some lateral
irection. We would have to know its relative susceptibility to gravita-
onal and electrical or magnetic force to determine its equilibrium posi-
on. Similarly, in an organism having a multiplicity of goals, or afflicted
ith some kind of internal goal conflict, behavior could be predicted
nly from information about the relative strengths of the several goals
nd the ways in which the adaptive processes responded to them.

Economics has been moving steadily into new areas where the power
f the classical equilibrium model has never been demonstrated, and

where its adequacy must be considered anew. Labor economics is such an area, oligopoly or imperfect competition theory another, decision-making under uncertainty a third, and the theory of economic development a fourth. In all of these areas the complexity and instability of his environment becomes a central feature of the choices that economic man faces. To explain his behavior in the face of this complexity, the theory must describe him as something more than a featureless, adaptive organism; it must incorporate at least some description of the processes and mechanisms through which the adaptation takes place. Let us list a little more concretely some specific problems of this kind:

(a) The classical theory postulates that the consumer maximizes utility. Recent advances in the theory of rational consumer choice have shown that the existence of a utility function, and its characteristics, if it exists, can be studied empirically.

(b) The growing separation between ownership and management has directed attention to the motivations of managers and the adequacy of the profit-maximization assumption for business firms. So-called human relations research has raised a variety of issues about the motivation of both executives and employees.

(c) When, in extending the classical theory, the assumptions of perfect competition were removed, even the definition of rationality became ambiguous. New definitions had to be constructed, by no means as "obvious" intuitively as simple maximization, to extend the theory of rational behavior to bilateral monopoly and to other bargaining and outguessing situations.

(d) When the assumptions of perfect foresight were removed, to handle uncertainty about the environment, the definition of rationality had to be extended in another direction to take into account prediction and the formation of expectations.

(e) Broadening the definition of rationality to encompass goal conflict and uncertainty made it hard to ignore the distinction between the objective environment in which the economic actor "really" lives and the subjective environment that he perceives and to which he responds. When this distinction is made, we can no longer predict his behavior—even if he behaves rationally—from the characteristics of the objective environment; we also need to know something about his perceptual and cognitive processes.

We shall use these five problem areas as a basis for sorting out some recent explorations in theory, model building, and empirical testing. In Section II, we will examine developments in the theory of utility and consumer choice. In Section III, we will consider somewhat parallel issues relating to the motivation of managers. In Section IV, we will deal with conflict of goals and the phenomena of bargaining. In Section V

we will survey some of the work that has been done on uncertainty and the formation of expectations. In Section VI, we will explore recent developments in the theory of human problem-solving and other higher mental processes, and see what implications these have for economic decision-making.

## II. *The Utility Function*

The story of the re-establishment of cardinal utility, as a consequence of the introduction of uncertainty into the theory of choice, is well known.[4] When Pareto and Slutsky had shown that the theory of consumer demand could be derived from the properties of indifference curves, without postulating a cardinal utility function underlying these curves, it became fashionable to regard utility as an ordinal measure—a ranking of alternatives by preference. Indeed, it could be shown that only ordinal utility had operational status—that the experiments that had been proposed, and even tried in a couple of instances, to measure an individual's utilities by asking him to choose among alternatives could never distinguish between two cardinal utility functions that were ordinally equivalent—that differed only by stretchings and contractions of the unit of measurement.

It was shown by von Neumann and Morgenstern, as a byproduct of their development of the theory of games, that if the choice situation were extended to include choices among uncertain prospects—among lottery tickets, say—cardinal utilities could be assigned to the outcomes in an unequivocal way.[5] Under these conditions, if the subject's behavior was consistent, it was possible to measure cardinally the utilities that different outcomes had for him.

A person who behaved in a manner consistent with the axioms of choice of von Neumann and Morgenstern would act so as to maximize the expected value—the average, weighted by the probabilities of the alternative outcomes of a choice—of his utility. The theory could be tested empirically, however, only on the assumption that the probabilities assigned to the alternatives by the subject were identical with the "objective" probabilities of these events as known to the experimenter. For example, if a subject believed in the gamblers' fallacy, that after a run of heads an unbiased coin would be more likely to fall tails, his choices might appear inconsistent with his utility function, while the real difficulty would lie in his method of assigning probabilities. This

[4] Ward Edwards [23] provides an account of these developments from the psychologist's point of view; Chapter 2 of Luce and Raiffa [43] is an excellent introduction to the "new" utility theory. Arrow [5] contains a nonmathematical survey of this and related topics.

[5] The second edition of von Neumann and Morgenstern [50] contains the first rigorous axiomatic demonstration of this point.

difficulty of "subjective" versus "objective" probability soon came to light when attempts were made to test experimentally whether people behaved in accordance with the predictions of the new utility theory. At the same time, it was discovered that the problem had been raised and solved thirty years earlier by the English philosopher and mathematician Frank Ramsey.[6] Ramsey had shown that, by an appropriate series of experiments, the utilities and subjective probabilities assigned by a subject to a set of uncertain alternatives could be measured simultaneously.

*Empirical Studies*

The new axiomatic foundations of the theory of utility, which show that it is possible at least in principle to determine empirically whether people "have" utility functions of the appropriate kind, have led to a rash of choice experiments. An experimenter who wants to measure utilities, not merely in principle but in fact, faces innumerable difficulties. Because of these difficulties, most experiments have been limited to confronting the subjects with alternative lottery tickets, at various odds, for small amounts of money. The weight of evidence is that, under these conditions, most persons choose in a way that is reasonably consistent with the axioms of the theory—they behave as though they were maximizing the expected value of utility and as though the utilities of the several alternatives can be measured.[7]

When these experiments are extended to more "realistic" choices—choices that are more obviously relevant to real-life situations—difficulties multiply. In the few extensions that have been made, it is not at all clear that the subjects behave in accordance with the utility axioms. There is some indication that when the situation is very simple and transparent, so that the subject can easily see and remember when he is being consistent, he behaves like a utility maximizer. But as the choices become a little more complicated—choices, for example, among phonograph records instead of sums of money—he becomes much less consistent [21, Ch. 3] [47].[8]

We can interpret these results in either of two ways. We can say that consumers "want" to maximize utility, and that if we present

[6] Ramsey's important essay [57] was sufficiently obscure that it was overlooked until the ideas were rediscovered independently by de Finetti [26]. Valuable notes on the history of the topic together with a thorough formal treatment will be found in the first five chapters of Savage [58].

[7] Some of the empirical evidence is reviewed in [23]. A series of more recent empirical studies is reported in Davidson and Suppes [21].

[8] Some more recent experiments [57a], show a relatively high degree of transitivity. A. G. Papandreou, in a publication I have not yet seen (University of California Publications in Economics) also reports a high degree of transitivity.

them with clear and simple choices that they understand they will do so. Or we can say that the real world is so complicated that the theory of utility maximization has little relevance to real choices. The former interpretation has generally appeared more attractive to economists trained in classical utility theory and to management scientists seeking rules of behavior for normative microeconomics; the latter to behavioral scientists interested in the description of behavior.

*Normative Applications*

The new utility theory has provided the formal framework for much recent work in mathematical statistics—i.e., statistical decision theory.[9] Similarly (it would be accurate to say "synonymously"), this framework provides the basis for most of the normative models of management science and operations research designed for actual application to the decision-making problems of the firm.[10] Except for some very recent developments, linear programming has been limited to decision-making under certainty, but there have been far-reaching developments of dynamic programming dealing with the maximization of expected values of outcomes (usually monetary outcomes) in situations where future events can be predicted only in terms of probability distributions.[11]

Again, there are at least two distinct interpretations that can be placed on these developments. On the one hand, it can be argued: "Firms would like to maximize profits if they could. They have been limited in doing so by the conceptual and computational difficulties of finding the optimal courses of action. By providing powerful new mathematical tools and computing machines, we now enable them to behave in the manner predicted by Alfred Marshall, even if they haven't been able to in the past." Nature will imitate art and economic man will become as real (and as artificial) as radios and atomic piles.

The alternative interpretation rests on the observation that, even with the powerful new tools and machines, most real-life choices still lie beyond the reach of maximizing techniques—unless the situations are heroically simplified by drastic approximations. If man, according to this interpretation, makes decisions and choices that have some ap-

[9] The systematic development of statistics as decision theory is due largely to A. Wald [70] on the basis of the earlier work of J. Neyman and E. Pearson. Savage [58] carries the development further, erecting the foundations of statistics solidly on utility and probability theory.

[10] This work relates, of course, to profit maximization and cost minimization rather than utility maximization, but it is convenient to mention it at this point. See [11] [16] [69].

[11] Arrow, Harris and Marschak [3] were among the first to treat inventory decisions dynamically. A general treatment of the theory of dynamic programming will be found in Bellman [9].

pearance of rationality, rationality in real life must involve somet simpler than maximization of utility or profit. In Section VI, we wil where this alternative interpretation leads.

*The Binary Choice Experiment*

Much recent discussion about utility has centered around a p ticularly simple choice experiment. This experiment, in numer variants, has been used by both economists and psychologists to t the most diverse kinds of hypotheses. We will describe it so that we c use it as a common standard of comparison for a whole range of theor and empirical studies.[12]

We will call the situation we are about to describe the *binary cho* experiment. It is better known to most game theorists—particula those located not far from Nevada—as a two-armed bandit; and most psychologists as a partial reinforcement experiment. The subje is required, in each of a series of trials, to choose one or the other o two symbols—say, plus or minus. When he has chosen, he is tol whether his choice was "right" or "wrong," and he may also receive reward (in psychologist's language, a reinforcement) for "right choices. The experimenter can arrange the schedule of correct response in a variety of ways. There may be a definite pattern, or they may b randomized. It is not essential that one and only one response be correct on a given trial: the experimenter may determine that both or neither will be correct. In the latter case the subject may or may not be informed whether the response he did not choose would have been correct.

How would a utility-maximizing subject behave in the binary choice experiment? Suppose that the experimenter rewarded "plus" on one-third of the trials, determined at random, and "minus" on the remaining two-thirds. Then a subject, provided that he believed the sequence was random and observed that minus was rewarded twice as often as plus, should always, rationally, choose minus. He would find the correct answer two-thirds of the time, and more often than with any other strategy.

Unfortunately for the classical theory of utility in its simplest form, few subjects behave in this way. The most commonly observed behavior is what is called *event matching*.[13] The subject chooses the two alternatives (not necessarily at random) with relative frequencies roughly proportional to the relative frequencies with which they are rewarded.

[12] My understanding of the implications of the binary choice experiment owes much to conversations with Julian Feldman, and to his unpublished work on the experiment. See also, Bush and Mosteller [15] particularly Chapter 13.

[13] An example of data consistent with event-matching behavior is given on page 283 of [15].

Thus, in the example given, two-thirds of the time he would choose minus, and as a result would make a correct response, on the average, in 5 trials out of 9 (on two-thirds of the trials in which he chooses minus, and one-third of those in which he chooses plus).[14]

All sorts of explanations have been offered for the event-matching behavior. The simplest is that the subject just doesn't understand what strategy would maximize his expected utility; but with adult subjects in a situation as transparent as this one, this explanation seems far-fetched. The alternative explanations imply either that the subject regards himself as being engaged in a competitive game with the experimenter (or with "nature" if he accepts the experimenter's explanation that the stimulus is random), or that his responses are the outcome of certain kinds of learning processes. We will examine these two types of explanation further in Sections IV and V respectively. The important conclusion at this point is that even in an extremely simple situation, subjects do not behave in the way predicted by a straightforward application of utility theory.

*Probabilistic Preferences*

Before we leave the subject of utility, we should mention one recent important development. In the formalizations mentioned up to this point, probabilities enter only into the estimation of the consequences that will follow one alternative or another. Given any two alternatives, the first is definitely preferable to the second (in terms of expected utility), or the second to the first, or they are strictly indifferent. If the same pair of alternatives is presented to the subject more than once, he should always prefer the same member of the pair.

One might think this requirement too strict—that, particularly if the utility attached to one alternative were only slightly greater or less than that attached to the other, the subject might vacillate in his choice. An empirical precedent for such vacillation comes not only from casual observation of indecision but from analogous phenomena in the psychophysical laboratory. When subjects are asked to decide which of two weights is heavier, the objectively heavier one is chosen more often than the lighter one, but the relative frequency of choosing the heavier approaches one-half as the two weights approach equality. The probability that a subject will choose the objectively heavier weight depends, in general, on the ratio of the two weights.

Following several earlier attempts, a rigorous and complete axiom system for a utility theory incorporating probabilistic preferences has been constructed recently by Duncan Luce [cf. 43, App. 1]. Although

[14] Subjects tend to choose the more highly rewarded alternative slightly more frequently than is called for by event matching. Hence, the actual behavior tends to be some kind of average between event matching and the optimal behavior. See [15, Ch. 13].

the theory weakens the requirements of consistency in preference, it is empirically testable, at least in principle. Conceptually, it provides a more plausible interpretation of the notion of "indifference" than does the classical theory.

## III. *The Goals of Firms*

Just as the central assumption in the theory of consumption is that the consumer strives to maximize his utility, so the crucial assumption in the theory of the firm is that the entrepreneur strives to maximize his residual share—his profit. Attacks on this hypothesis have been frequent.[15] We may classify the most important of these as follows:

(a) The theory leaves ambiguous whether it is short-run or long-run profit that is to be maximized.

(b) The entrepreneur may obtain all kinds of "psychic income" from the firm, quite apart from monetary rewards. If he is to maximize his utility, then he will sometimes balance a loss of profits against an increase in psychic income. But if we allow "psychic income," the criterion of profit maximization loses all of its definiteness.

(c) The entrepreneur may not care to maximize, but may simply want to earn a return that he regards as satisfactory. By sophistry and and adept use of the concept of psychic income, the notion of seeking a satisfactory return can be translated into utility maximizing but not in any operational way. We shall see in a moment that "satisfactory profits" is a concept more meaningfully related to the psychological notion of aspiration levels than to maximization.

(d) It is often observed that under modern conditions the equity owners and the active managers of an enterprise are separate and distinct groups of people, so that the latter may not be motivated to maximize profits.

(e) Where there is imperfect competition among firms, maximizing is an ambiguous goal, for what action is optimal for one firm depends on the actions of the other firms.

In the present section we shall deal only with the third of these five issues. The fifth will be treated in the following section; the first, second, and fourth are purely empirical questions that have been discussed at length in the literature; they will be considered here only for their bearing on the question of satisfactory profits.

### *Satisficing versus Maximizing*

The notion of satiation plays no role in classical economic theory, while it enters rather prominently into the treatment of motivation in psychology. In most psychological theories the motive to act stems from

[15] For a survey of recent discussions see Papandreou [55].

*drives*, and action terminates when the drive is satisfied. Moreover, the conditions for satisfying a drive are not necessarily fixed, but may be specified by an aspiration level that itself adjusts upward or downward on the basis of experience.

If we seek to explain business behavior in the terms of this theory, we must expect the firm's goals to be not maximizing profit, but attaining a certain level or rate of profit, holding a certain share of the market or a certain level of sales. Firms would try to "satisfice" rather than to maximize.[16]

It has sometimes been argued that the distinction between satisficing and maximizing is not important to economic theory. For in the first place, the psychological evidence on individual behavior shows that aspirations tend to adjust to the attainable. Hence in the long run, the argument runs, the level of aspiration and the attainable maximum will be very close together. Second, even if some firms satisficed, they would gradually lose out to the maximizing firms, which would make larger profits and grow more rapidly than the others.

These are, of course, precisely the arguments of our molasses metaphor, and we may answer them in the same way that we answered them earlier. The economic environment of the firm is complex, and it changes rapidly; there is no a priori reason to assume the attainment of long-run equilibrium. Indeed, the empirical evidence on the distribution of firms by size suggests that the observed regularities in size distribution stem from the statistical equilibrium of a population of adaptive systems rather than the static equilibrium of a population of maximizers.[17]

Models of satisficing behavior are richer than models of maximizing behavior, because they treat not only of equilibrium but of the method of reaching it as well. Psychological studies of the formation and change of aspiration levels support propositions of the following kinds.[18] (a) When performance falls short of the level of aspiration, search behavior (particularly search for new alternatives of action) is induced. (b) At the same time, the level of aspiration begins to adjust itself downward until goals reach levels that are practically attainable. (c) If the two mechanisms just listed operate too slowly to adapt aspirations to performance, emotional behavior—apathy or aggression, for example—will replace rational adaptive behavior.

[16] A comparison of satisficing with maximizing models of decision-making can be found in [64, Ch. 14]. Katona [40] has independently made similar comparisons of economic and psychological theories of decision.

[17] Simon and Bonini [66] have constructed a stochastic model that explains the observed data on the size distributions of business firms.

[18] A standard psychological reference on aspiration levels is [42]. For applications to economics, see [61] and [45] (in the latter, consult the index under "aspiration levels").

The aspiration level defines a natural zero point in the scale of utility—whereas in most classical theories the zero point is arbitrary. When the firm has alternatives open to it that are at or above its aspiration level, the theory predicts that it will choose the best of those known to be available. When none of the available alternatives satisfies current aspirations, the theory predicts qualitatively different behavior: in the short run, search behavior and the revision of targets; in the longer run, what we have called above emotional behavior, and what the psychologist would be inclined to call neurosis.[19]

*Studies of Business Behavior*

There is some empirical evidence that business goals are, in fact, stated in satisficing terms.[20] First, there is the series of studies stemming from the pioneering work of Hall and Hitch that indicates that businessmen often set prices by applying a standard markup to costs. Some economists have sought to refute this fact, others to reconcile it—if it is a fact—with marginalist principles. The study of Earley [22a, pp. 44-70] belongs to the former category, but its evidence is suspect because the questions asked of businessmen are leading ones—no one likes to admit that he would accept less profit if he could have more. Earley did not ask his respondents how they determined marginal cost and marginal revenue, how, for example, they estimated demand elasticities.

Another series of studies derived from the debate over the Keynesian doctrine that the amount of investment was insensitive to changes in the rate of interest. The general finding in these studies has been that the rate of interest is not an important factor in investment decisions [24] [39, Ch. 11] [71].

More recently, my colleagues Cyert and March, have attempted to test the satisficing model in a more direct way [19]. They found in one industry some evidence that firms with a declining share of market strove more vigorously to increase their sales than firms whose shares of the market were steady or increasing.

*Aspirations in the Binary Choice Experiment*

Although to my knowledge this has not been done, it would be easy to look for aspiration-level phenomena in the binary choice experiment.

[19] Lest this last term appear fanciful I should like to call attention to the phenomena of panic and broken morale, which are well known to observers of the stock market and of organizations but which have no reasonable interpretation in classical utility theory. I may also mention that psychologists use the theory described here in a straightforward way to produce experimental neurosis in animal and human subjects.

[20] A comprehensive bibliography of empirical work prior to 1950 will be found in [37]. Some of the more recent work is [19] [24] [39, Ch. 11].

By changing the probabilities of reward in different ways for different groups of subjects, we could measure the effects of these changes on search behavior—where amount of search would be measured by changes in the pattern of responses.

### *Economic Implications*

It has sometimes been argued that, however realistic the classical theory of the firm as a profit maximizer, it is an adequate theory for purposes of normative macroeconomics. Mason, for example, in commenting on Papandreou's essay on "Problems in the Theory of the Firm" [55, pp. 183-222] says, "The writer of this critique must confess a lack of confidence in the marked superiority, *for purposes of economic analysis*, of this newer concept of the firm over the older conception of the entrepreneur." The italics are Mason's.

The theory of the firm is important for welfare economics—e.g., for determining under what circumstances the behavior of the firm will lead to efficient allocation of resources. The satisficing model vitiates all the conclusions about resource allocation that are derivable from the maximizing model when perfect competition is assumed. Similarly, a dynamic theory of firm sizes, like that mentioned above, has quite different implications for public policies dealing with concentration than a theory that assumes firms to be in static equilibrium. Hence, welfare economists are justified in adhering to the classical theory only if: (a) the theory is empirically correct as a description of the decision-making process; or (b) it is safe to assume that the system operates in the neighborhood of the static equilibrium. What evidence we have mostly contradicts both assumptions.

## IV. *Conflict of Interest*

Leaving aside the problem of the motivations of hired managers, conflict of interest among economic actors creates no difficulty for classical economic theory—indeed, it lies at the very core of the theory—so long as each actor treats the other actors as parts of his "given" environment, and doesn't try to predict their behavior and anticipate it. But when this restriction is removed, when it is assumed that a seller takes into account the reactions of buyers to his actions, or that each manufacturer predicts the behaviors of his competitors—all the familiar difficulties of imperfect competition and oligopoly arise.[21]

The very assumptions of omniscient rationality that provide the basis for deductive prediction in economics when competition is present lead

[21] There is by now a voluminous literature on the problem. The difficulties in defining rationality in competitive situations are well stated in the first chapter of von Neumann and Morgenstern [50].

to ambiguity when they are applied to competition among the few. The central difficulty is that rationality requires one to outguess one's opponents, but not to be outguessed by them, and this is clearly not a consistent requirement if applied to all the actors.

*Game Theory*

Modern game theory is a vigorous and extensive exploration of ways of extending the concept of rational behavior to situations involving struggle, outguessing, and bargaining. Since Luce and Raiffa [43] have recently provided us with an excellent survey and evaluation of game theory, I shall not cover the same ground here.[22] I concur in their general evaluation that, while game theory has greatly clarified the issues involved, it has not provided satisfactory solutions. Not only does it leave the definition of rational conduct ambiguous in all cases save the zero-sum two-person game, but it requires of economic man even more fantastic reasoning powers than does classical economic theory.[23]

*Power and Bargaining*

A number of exploratory proposals have been put forth as alternatives to game theory—among them Galbraith's notion of countervailing power [30] and Schelling's bargaining theory [59] [60]. These analyses draw at least as heavily upon theories of power and bargaining developed initially to explain political phenomena as upon economic theory. They do not lead to any more specific predictions of behavior than do game-theoretic approaches, but place a greater emphasis upon description and actual observation, and are modest in their attempt to derive predictions by deductive reasoning from a few "plausible" premises about human behavior.

At least four important areas of social science and social policy, two of them in economics and two more closely related to political science, have as their central concern the phenomena of power and the processes of bargaining: the theory of political parties, labor-management relations, international politics, and oligopoly theory. Any progress in the basic theory applicable to one of these is certain to be of almost equal importance to the others. A growing recognition of their common concern is evidenced by the initiation of a new cross-disciplinary journal, *Journal of Conflict Resolution.*

[22] Chapters 5 and 6 of [43] provide an excellent survey of the attempts that have been made to extend the theory of games to the kinds of situations most relevant to economics.

[23] In a forthcoming volume on *Strategy and Market Structure,* Martin Shubik approaches the topics of imperfect competition and oligopoly from the standpoint of the theory of games.

*ımes against Nature*

While the binary choice experiment is basically a one-person game, is possible to interpret it as a "game against nature," and hence to y to explain it in game-theoretic terms. According to game theory, the bject, if he believes in a malevolent nature that manipulates the dice ainst him, should minimax his expected utility instead of maximizing That is, he should adopt the course of action that will maximize his pected utility under the assumption that nature will do her worst to m.

Minimaxing expected utility would lead the subject to call plus or inus at random and with equal probability, regardless of what the story of rewards has been. This is something that subjects demon-:ably do not do.

However, it has been suggested by Savage [58] and others that ople are not as interested in maximizing utility as they are in mini-izing regret. "Regret" means the difference between the reward ac-ally obtained and the reward that could have been obtained with rfect foresight (actually, with perfect hindsight!). It turns out that inimaxing regret in the binary choice experiment leads to event-atching behavior [64, Ch. 16]. Hence, the empirical evidence is at ıst crudely consistent with the hypothesis that people play against .ture by minimaxing regret. We shall see, however, that event-match-g is also consistent with a number of other rules of behavior that em more plausible on their face; hence we need not take the present planation too seriously—at least I am not inclined to do so.

## V. *The Formation of Expectations*

While the future cannot enter into the determination of the present, pectations about the future can and do. In trying to gain an under-ınding of the saving, spending, and investment behavior of both con-mers and firms, and to make short-term predictions of this behavior r purposes of policy-making, economists have done substantial em-rical work as well as theorizing on the formation of expectations.

*npirical Studies*

A considerable body of data has been accumulated on consumers' ans and expectations from the Survey of Consumer Finances, con-ıcted for the Board of Governors of the Federal Reserve System by e Survey Research Center of the University of Michigan [39, Ch. 5]. ıese data, and similar data obtained by others, begin to give us some formation on the expectations of consumers about their own incomes, ıd the predictive value of their expenditure plans for their actual sub-

sequent behavior. Some large-scale attempts have been made, notably by Modigliani and Brumberg [48, pp. 388-436] and, a little later, by Friedman [28] to relate these empirical findings to classical utility theory. The current empirical research on businessmen's expectations is of two main kinds:

1. Surveys of businessmen's own forecasts of business and business conditions in the economy and in their own industries [24, pp. 165-88] [29, pp. 189-98]. These are obtained by straightforward questionnaire methods that assume, implicitly, that businessmen can and do make such forecasts. In some uses to which the data are put, it is also assumed that the forecasts are used as one basis for businessmen's actions.

2. Studies of business decisions and the role of expectations in these decisions—particularly investment and pricing decisions. We have already referred to studies of business decisions in our discussion of the goals of the firm.[24]

*Expectations and Probability*

The classical way to incorporate expectations into economic theory is to assume that the decision-maker estimates the joint probability distribution of future events.[25] He can then act so as to maximize the expected value of utility or profit, as the case may be. However satisfying this approach may be conceptually, it poses awkward problems when we ask how the decision-maker actually estimates the parameters of the joint probability distribution. Common sense tells us that people don't make such estimates, nor can we find evidence that they do by examining actual business forecasting methods. The surveys of businessmen's expectations have never attempted to secure such estimates, but have contented themselves with asking for point predictions—which, at best, might be interpreted as predictions of the means of the distributions.

It has been shown that under certain special circumstances the mean of the probability distribution is the only parameter that is relevant for decision—that even if the variance and higher moments were known to the rational decision-maker, he would have no use for them.[26] In these cases, the arithmetic mean is actually a certainty equivalent, the optimal decision turns out to be the same as if the future were known with certainty. But the situations where the mean is a certainty equival-

[24] See the references cited [12, p. 160].

[25] A general survey of approaches to decision-making under uncertainty will be found in [2] and in [43, Ch. 13].

[26] The special case in which mean expectations constitute a certainty equivalent is treated in [62]. An alternative derivation, and fuller discussion is given by Theil [67, Ch. 8, sect. 6].

ent are, as we have said, very special ones, and there is no indication that businessmen ever ask whether the necessary conditions for this equivalence are actually met in practice. They somehow make forecasts in the form of point predictions and act upon them in one way or another.

The "somehow" poses questions that are important for business cycle theory, and perhaps for other problems in economics. The way in which expectations are formed may affect the dynamic stability of the economy, and the extent to which cycles will be amplified or damped. Some light, both empirical and theoretical, has recently been cast on these questions. On the empirical side, attempts have been made: (a) to compare businessmen's forecasts with various "naïve" models that assume the future will be some simple function of the recent past, and (b) to use such naïve models themselves as forecasting devices.

The simplest naïve model is one that assumes the next period will be exactly like the present. Another assumes that the change from present to next period will equal the change from last period to present; a third, somewhat more general, assumes that the next period will be a weighted average of recent past periods. The term "naïve model" has been applied loosely to various forecasting formulae of these general kinds. There is some affirmative evidence that business forecasts fit such models. There is also evidence that elaboration of the models beyond the first few steps of refinement does not much improve prediction; see, for example, [20]. Arrow and his colleagues [4] have explored some of the conditions under which forecasting formulae will, and will not, introduce dynamic instability into an economic system that is otherwise stable. They have shown, for example, that if a system of multiple markets is stable under static expectations, it is stable when expectations are based on a moving average of past values.

The work on the formation of expectations represents a significant extension of classical theory. For, instead of taking the environment as a "given," known to the economic decision-maker, it incorporates in the theory the processes of acquiring knowledge about that environment. In doing so, it forces us to include in our model of economic man some of his properties as a learning, estimating, searching, information-processing organism [65].

*The Cost of Information*

There is one way in which the formation of expectations might be reincorporated in the body of economic theory: by treating information-gathering as one of the processes of production, so to speak, and applying to it the usual rules of marginal analysis. Information, says price theory, should be gathered up to the point where the incremental

cost of additional information is equal to the incremental profit that can be earned by having it. Such an approach can lead to propositions about optimal amounts of information-gathering activity and about the relative merits of alternative information-gathering and estimating schemes.[27]

This line of investigation has, in fact, been followed in statistical decision theory. In sampling theory we are concerned with the optimal size of sample (and in the special and ingenious case of sequential sampling theory, with knowing when to stop sampling), and we wish to evaluate the efficiencies of alternative sampling procedures. The latter problem is the simpler, since it is possible to compare the relative costs of alternative schemes that have the same sampling error, and hence to avoid estimating the value of the information.[28] However, some progress has been made also toward estimating the value of improved forecast accuracy in situations where the forecasts are to be used in applying formal decision rules to choice situations.[29]

The theory of teams developed by Marschak and Radner is concerned with the same problem (see, e.g., [46]) It considers situations involving decentralized and interdependent decision-making by two or more persons who share a common goal and who, at a cost, can transmit information to each other about their own actions or about the parts of the environment with which they are in contact. The problem then is to discover the optimal communication strategy under specified assumptions about communication costs and payoffs.

The cost of communication in the theory of teams, like the cost of observations in sampling theory, is a parameter that characterizes the economic actor, or the relation of the actor to his environment. Hence, while these theories retain, in one sense, a classical picture of economic man as a maximizer, they clearly require considerable information about the characteristics of the actor, and not merely about his environment. They take a long stride toward bridging the gap between the traditional concerns of economics and the concerns of psychology.

*Expections in the Binary Choice Experiment*

I should like to return again to the binary choice experiment, to see what light it casts on the formation of expectations. If the subject is told by the experimenter that the rewards are assigned at random, if he

[27] Fundamental and applied research are examples of economically significant information-gathering activities. Griliches [34] has recently made an attempt to estimate the economic return from research on hybrid corn.

[28] Modern treatments of sampling theory, like Cochran [17] are based on the idea of minimizing the cost of obtaining a fixed amount of information.

[29] For the theory and an application to macroeconomics, see Theil [67, Ch. 8, sects. 5 and 6].

is told what the odds are for each alternative, ***and if he believes the experimenter,*** the situation poses no forecasting problem. We have seen, however, that the behavior of most subjects is not consistent with these assumptions.

How would sequential sampling theory handle the problem? Each choice the subject makes now has two consequences: the immediate reward he obtains from it, and the increment of information it provides for predicting the future rewards. If he thinks only of the latter consequences, he is faced with the classical problem of induction: to estimate the probability that an event will occur in the future on the basis of its frequency of occurrence in the past. Almost any rule of induction would require a rational (maximizing) subject to behave in the following general manner: to sample the two alternatives in some proportion to estimate the probability of reward associated with each; after the error of estimate had been reduced below some bound, always to choose the alternative with the higher probability of reward. Unfortunately, this does not appear to be what most subjects do.

If we give up the idea of maximization, we can make the weaker assumption that the subject is adaptive—or learns—but not necessarily in any optimal fashion. What do we mean by adaptation or learning? We mean, gradually and on the basis of experience responding more frequently with the choice that, in the past, has been most frequently rewarded. There is a whole host of rules of behavior possessing this characteristic. Postulate, for example, that at each trial the subject has a certain probability of responding "plus," and the complementary probability of responding "minus." Postulate further that when he makes a particular response the probability of making the same response on the next trial is increased if the response is rewarded and decreased if the response is not rewarded. The amount of increment in the response probability is a parameter characterizing the learning rate of the particular subject. Almost all schemes of this kind produce asymptotic behaviors, as the number of trials increases, that are approximately event-matching in character.

Stochastic learning models, as the processes just described are usually called, were introduced into psychology in the early 1950's by W. K. Estes and Bush and Mosteller [15] and have been investigated extensively since that time. The models fit some of the gross features of the observed behaviors—most strikingly the asymptotic probabilities—but do not explain very satisfactorily the fine structure of the observations.

Observation of subjects in the binary choice experiment reveals that usually they not only refuse to believe that (or even to act as if) the reward series were random, but in fact persist over many trials in

searching for systematic patterns in the series. To account for such behavior, we might again postulate a learning model, but in this case a model in which the subject does not react probabilistically to his environment, but forms and tests definite hypotheses about systematic patterns in it. Man, in this view, is not only a learning animal; he is a pattern-finding and concept-forming animal. Julian Feldman [25] has constructed theories of this kind to explain the behavior of subjects in the binary choice experiment, and while the tests of the theories are not yet completed, his findings look exceedingly promising.

As we move from maximizing theories, through simple stochastic learning theories, to theories involving pattern recognition our model of the expectation-forming processes and the organism that performs it increases in complexity. If we follow this route, we reach a point where a theory of behavior requires a rather elaborate and detailed picture of the rational actor's cognitive processes.

## VI. *Human Cognition and Economics*

All the developments we have examined in the preceding four sections have a common theme: they all involve important modifications in the concept of economic man and, for the reasons we have stated, modifications in the direction of providing a fuller description of his characteristics. The classical theory is a theory of a man choosing among fixed and known alternatives, to each of which is attached known consequences. But when perception and cognition intervene between the decision-maker and his objective environment, this model no longer proves adequate. We need a description of the choice process that recognizes that alternatives are not given but must be sought; and a description that takes into account the arduous task of determining what consequences will follow on each alternative [63, Ch. 5] [64, Part 4] [14].

The decision-maker's information about his environment is much less than an approximation to the real environment. The term "approximation" implies that the subjective world of the decision-maker resembles the external environment closely, but lacks, perhaps, some fineness of detail. In actual fact the perceived world is fantastically different from the "real" world. The differences involve both omissions and distortions, and arise in both perception and inference. The sins of omission in perception are more important than the sins of commission. The decision-maker's model of the world encompasses only a minute fraction of all the relevant characteristics of the real environment, and his inferences extract only a minute fraction of all the information that is present even in his model.

Perception is sometimes referred to as a "filter." This term is as

misleading as "approximation," and for the same reason: it implies that what comes through into the central nervous system is really quite a bit like what is "out there." In fact, the filtering is not merely a passive selection of some part of a presented whole, but an active process involving attention to a very small part of the whole and exclusion, from the outset, of almost all that is not within the scope of attention.

Every human organism lives in an environment that generates millions of bits of new information each second, but the bottleneck of the perceptual apparatus certainly does not admit more than 1,000 bits per second, and probably much less. Equally significant omissions occur in the processing that takes place when information reaches the brain. As every mathematician knows, it is one thing to have a set of differential equations, and another thing to have their solutions. Yet the solutions are logically implied by the equations—they are "all there," if we only knew how to get to them! By the same token, there are hosts of inferences that *might* be drawn from the information stored in the brain that are not in fact drawn. The consequences implied by information in the memory become known only through active information-processing, and hence through active selection of particular problem-solving paths from the myriad that might have been followed.

In this section we shall examine some theories of decision-making that take the limitations of the decision-maker and the complexity of the environment as central concerns. These theories incorporate some mechanisms we have already discussed—for example, aspiration levels and forecasting processes—but go beyond them in providing a detailed picture of the choice process.

A real-life decision involves some goals or values, some facts about the environment, and some inferences drawn from the values and facts. The goals and values may be simple or complex, consistent or contradictory; the facts may be real or supposed, based on observation or the reports of others; the inferences may be valid or spurious. The whole process may be viewed, metaphorically, as a process of "reasoning," where the values and facts serve as premises, and the decision that is finally reached is inferred from these premises [63]. The resemblance of decision-making to logical reasoning is only metaphorical, because there are quite different rules in the two cases to determine what constitute "valid" premises and admissible modes of inference. The metaphor is useful because it leads us to take the individual *decision premise* as the unit of description, hence to deal with the whole interwoven fabric of influences that bear on a single decision—but without being bound by the assumptions of rationality that limit the classical theory of choice.

### *Rational Behavior and Role Theory*

We can find common ground to relate the economist's theory of decision-making with that of the social psychologist. The latter is particularly interested, of course, in social influences on choice, which determine the *role* of the actor. In our present terms, a role is a social prescription of some, but not all, of the premises that enter into an individual's choices of behavior. Any particular concrete behavior is the resultant of a large number of premises, only some of which are prescribed by the role. In addition to role premises there will be premises about the state of the environment based directly on perception, premises representing beliefs and knowledge, and idiosyncratic premises that characterize the personality. Within this framework we can accommodate both the rational elements in choice, so much emphasized by economics, and the nonrational elements to which psychologists and sociologists often prefer to call attention.

### *Decision Premises and Computer Programs*

The analysis of choice in terms of decision premises gives us a conceptual framework for describing and explaining the process of deciding. But so complex is the process that our explanations of it would have remained schematic and hypothetical for a long time to come had not the modern digital computer appeared on the scene. The notion of decision premise can be translated into computer terminology, and when this translation has been accomplished, the digital computer provides us with an instrument for simulating human decision processes—even very complex ones—and hence for testing empirically our explanations of those processes [53].

A fanciful (but only slightly fanciful) example will illustrate how this might be done. Some actual examples will be cited presently. Suppose we were to construct a robot incorporating a modern digital computer, and to program (i.e., to instruct) the robot to take the role of a business executive in a specified company. What would the program look like? Since no one has yet done this, we cannot say with certainty, but several points are fairly clear. The program would not consist of a list of prescribed and proscribed behaviors, since what an executive does is highly contingent on information about a wide variety of circumstances. Instead, the program would consist of a large number of *criteria* to be applied to possible and proposed courses of action, of routines for *generating* possible courses of action, of computational procedures for *assessing* the state of the environment and its implications for action, and the like. Hence, the program—in fact, a role prescription—would interact with information to produce concrete behavior adapted to the situation. The elements of such a program take

the form of what we have called decision premises, and what the computer specialists would call instructions.

The promise of constructing actual detailed descriptions of concrete roles and decision processes is no longer, with the computer, a mere prospectus to be realized at some undefined future date. We can already provide actual examples, some of them in the area of economics.

1. *Management Science*. In the paragraphs on normative applications in Section II, we have already referred to the use of such mathematical techniques as linear programming and dynamic programming to construct formal decision processes for actual situations. The relevance of these decision models to the present discussion is that they are not merely abstract "theories" of the firm, but actual decision-making devices. We can think of any such device as a simulation of the corresponding human decision-maker, in which the equations and other assumptions that enter into the formal decision-making procedure correspond to the decision premises—including the role prescription—of the decision-maker.

The actual application of such models to concrete business situations brings to light the information-processing tasks that are concealed in the assumptions of the more abstract classical models [65, pp. 51-52]:

(1) The models must be formulated so as to require for their application only data that are obtainable. If one of the penalties, for example, of holding too small inventories is the loss of sales, a decision model that proposes to determine optimal inventory levels must incorporate a procedure for putting a dollar value on this loss.

(2) The models must call only for practicable computations. For example, several proposals for applying linear programming to certain factory scheduling problems have been shown to be impracticable because, even with computers, the computation time is too great. The task of decision theory (whether normative or descriptive) is to find alternative techniques—probably only approximate—that demand much less computation.

(3) The models must not demand unobtainable forecast information. A procedure that would require a sales department to estimate the third moment of next month's sales distribution would not have wide application, as either description or prescription, to business decision-making.

These models, then, provide us with concrete examples of roles for a decision-maker described in terms of the premises he is expected to apply to the decision—the data and the rules of computation.

2. *Engineering Design*. Computers have been used for some years to carry out some of the analytic computations required in engineering design—computing the stresses, for example, in a proposed bridge

design. Within the past two years, ways have been found to program computers to carry out synthesis as well as analysis—to evolve the design itself.[30] A number of companies in the electrical industry now use computers to design electric motors, transformers, and generators, going from customer specifications to factory design without human intervention. The significance of this for our purpose here is that the synthesis programs appear to simulate rather closely the processes that had previously been used by college-trained engineers in the same design work. It has proved possible to write down the engineers' decision premises and inference processes in sufficient detail to produce workable computer programs.

3. *Human Problem Solving*. The management science and engineering design programs already provide examples of simulation of human decision-making by computer. It may be thought that, since in both instances the processes are highly arithmetical, these examples are relevant to only a very narrow range of human problem-solving activity. We generally think of a digital computer as a device which, if instructed in painful detail by its operator, can be induced to perform rather complicated and tedious arithmetical operations. More recent developments require us to revise these conceptions of the computer, for they enable it to carry out tasks that, if performed by humans, we would certainly call "thinking" and "learning."

Discovering the proof of a theorem of Euclid—a task we all remember from our high school geometry course—requires thinking and usually insight and imagination. A computer is now being programmed to perform this task (in a manner closely simulating the human geometer), and another computer has been successfully performing a highly similar task in symbolic logic for the past two years.[31] The latter computer is programmed to learn—that is to improve its performance on the basis of successful problem-solving experience—to use something akin to imagery or metaphor in planning its proofs, and to transfer some of its skills to other tasks—for example, solving trigonometric identities—involving completely distinct subject matter. These programs, it should be observed, do not involve the computer in rapid arithmetic—or any arithmetic for that matter. They are basically nonnumerical, involving the manipulation of all kinds of symbolic material, including words.

Still other computer programs have been written to enable a computer to play chess.[32] Not all of these programs, or those previously

[30] A nontechnical description of such a program will be found in [33].

[31] The program for proving theorems in logic is discussed in [51] and [52], Gelernter and Rochester's geometry program in [31].

[32] A survey of computer chess programs can be found in [54].

mentioned, are close simulations of the processes humans use. However, in some direct attempts to investigate the human processes by thinking-aloud techniques and to reproduce in computer programs the processes observed in human subjects, several striking simulations have been achieved.[33] These experiments have been described elsewhere and can't be reviewed here in detail.

4. *Business Games*. Business games, like those developed by the American Management Association, International Business Machines Corporation, and several universities, represent a parallel development.[34] In the business game, the decisions of the business firms are still made by the human players, but the economic environment of these firms, including their markets, are represented by computer programs that calculate the environment's responses to the actions of the players. As the games develop in detail and realism, their programs will represent more and more concrete descriptions of the decision processes of various economic actors—for example, consumers.

The games that have been developed so far are restricted to numerical magnitudes like prices and quantities of goods, and hence resemble the management science and engineering design programs more closely than they do those we have described under the heading of human problem solving. There is no reason, however, to expect this restriction to remain very long.

*Implications for Economics*

Apart from normative applications (e.g., substituting computers for humans in certain decision-making tasks) we are not interested so much in the detailed descriptions of roles as in broader questions: (1) What general characteristics do the roles of economic actors have? (2) How do roles come to be structured in the particular ways they do? (3) What bearing does this version of role theory have for macroeconomics and other large-scale social phenomena?

*Characterizing Role Structure*. Here we are concerned with generalizations about thought processes, particularly those generalizations that are relatively independent of the substantive content of the role. A classical example is Dewey's description of stages in the problem-solving process. Another example, of particular interest to economics, is the hypothesis we have already discussed at length: that economic man is a *satisficing* animal whose problem solving is based on search activity to meet certain aspiration levels rather than a *maximizing* animal whose problem solving involves finding the best alternatives in terms of specified criteria [64]. A third hypothesis is that operative goals (those

[33] Much of this work is still unpublished, but see [53] and [54].

[34] Two business games are described by Andlinger [1].

associated with an observable criterion of success, and relatively definite means of attainment) play a much larger part in governing choice than nonoperative goals (those lacking a concrete measure of success or a program for attainment) [45, p. 156].

*Understanding How Roles Emerge.* Within almost any single business firm, certain characteristic types of roles will be represented: selling roles, production roles, accounting roles, and so on [22]. Partly, this consistency may be explained in functional terms—that a model that views the firm as producing a product, selling it, and accounting for its assets and liabilities is an effective simplification of the real world, and provides the members of the organization with a workable frame of reference. Imitation within the culture provides an alternative explanation. It is exceedingly difficult to test hypotheses as to the origins and causal conditions for roles as universal in the society as these, but the underlying mechanisms could probably be explored effectively by the study of less common roles—safety director, quality control inspector, or the like—that are to be found in some firms, but not in all.

With our present definition of role, we can also speak meaningfully of the role of an entire business firm—of decision premises that underlie its basic policies. In a particular industry we find some firms that specialize in adapting the product to individual customer's specifications; others that specialize in product innovation. The common interest of economics and psychology includes not only the study of individual roles, but also the explanation of organizational roles of these sorts.

*Tracing the Implications for Macroeconomics.* If basic professional goals remain as they are, the interest of the psychologist and the economist in role theory will stem from somewhat different ultimate aims. The former will use various economic and organizational phenomena as data for the study of the structure and determinants of roles; the latter will be primarily interested in the implications of role theory for the model of economic man, and indirectly, for macroeconomics.

The first applications will be to those topics in economics where the assumption of static equilibrium is least tenable. Innovation, technological change, and economic development are examples of areas to which a good empirically tested theory of the processes of human adaptation and problem solving could make a major contribution. For instance, we know very little at present about how the rate of innovation depends on the amounts of resources allocated to various kinds of research and development activity [34]. Nor do we understand very well the nature of "know how," the costs of transferring technology from one firm or economy to another, or the effects of various kinds and amounts of education upon national product. These are diffi-

cult questions to answer from aggregative data and gross observation, with the result that our views have been formed more by arm-chair theorizing than by testing hypotheses with solid facts.

## VII. *Conclusion*

In exploring the areas in which economics has common interests with the other behavioral sciences, we have been guided by the metaphor we elaborated in Section I. In simple, slow-moving situations, where the actor has a single, operational goal, the assumption of maximization relieves us of any need to construct a detailed picture of economic man or his processes of adaptation. As the complexity of the environment increases, or its speed of change, we need to know more and more about the mechanisms and processes that economic man uses to relate himself to that environment and achieve his goals.

How closely we wish to interweave economics with psychology depends, then, both on the range of questions we wish to answer and on our assessment of how far we may trust the assumptions of static equilibrium as approximations. In considerable part, the demand for a fuller picture of economic man has been coming from the profession of economics itself, as new areas of theory and application have emerged in which complexity and change are central facts. The revived interest in the theory of utility, and its application to choice under uncertainty, and to consumer saving and spending is one such area. The needs of normative macroeconomics and management science for a fuller theory of the firm have led to a number of attempts to understand the actual processes of making business decisions. In both these areas, notions of adaptive and satisficing behavior, drawn largely from psychology, are challenging sharply the classical picture of the maximizing entrepreneur.

The area of imperfect competition and oligopoly has been equally active, although the activity has thus far perhaps raised more problems than it has solved. On the positive side, it has revealed a community of interest among a variety of social scientists concerned with bargaining as a part of political and economic processes. Prediction of the future is another element common to many decision processes, and particularly important to explaining business cycle phenomena. Psychologists and economists have been applying a wide variety of approaches, empirical and theoretical, to the study of the formation of expectations. Surveys of consumer and business behavior, theories of statistical induction, stochastic learning theories, and theories of concept formation have all been converging on this problem area.

The very complexity that has made a theory of the decision-making process essential has made its construction exceedingly difficult. Most

approaches have been piecemeal—now focused on the criteria of choice, now on conflict of interest, now on the formation of expectations. It seemed almost utopian to suppose that we could put together a model of adaptive man that would compare in completeness with the simple model of classical economic man. The sketchiness and incompleteness of the newer proposals has been urged as a compelling reason for clinging to the older theories, however inadequate they are admitted to be.

The modern digital computer has changed the situation radically. It provides us with a tool of research—for formulating and testing theories—whose power is commensurate with the complexity of the phenomena we seek to understand. Although the use of computers to build theories of human behavior is very recent, it has already led to concrete results in the simulation of higher mental processes. As economics finds it more and more necessary to understand and explain disequilibrium as well as equilibrium, it will find an increasing use for this new tool and for communication with its sister sciences of psycology and sociology.

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# INDIA AND CHINA: CONTRASTS IN DEVELOPMENT PERFORMANCE

By WILFRED MALENBAUM*

A decade has passed since the problems of economic growth in the ›oorer nations became a major foreign policy concern of the wealthy .nd powerful nations of the world. In the underdeveloped lands, the en years reveal extensive planning activity as well as unprecedented ıflows of technical and capital assistance on government account. Also uring this period the imagination and efforts of many economists and ther social scientists all over the world have turned to the task of unovering the secret to the critical transition—the process by which tagnation may become growth, or progress at a slow rate may be acelerated. Yet very few countries have succeeded in making this transition during the decade. In Asia, where live a large part of the world's opulation and an even larger part of its poor people, India and mainınd China alone offer some prospect of such achievement in the near ıture.

The relative progress in the development of these two countries is of :eat significance. There were strong parallels in their preplan structıre and strong contrasts between China's totalitarian and India's :mocratic programs [18, pp. 1-24]. Their performance relative to ıe another may influence the programs adopted by other, now less adınced, countries. It will certainly bear upon Soviet and United States ›reign policies. Furthermore, the record of the course of development these two lands provides a unique opportunity for examining the ·ocess of development as such. What are the essential economic inedients? Can they be used with equal effectiveness in democratic and ›mmunist societies?

## I. *Comparative Performance*

India is now in the last half of its second five year plan. China :tiated its second plan on January 1, 1958. Records can actually be

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compared over the nine-year period ending in early 1959. Of course, records are available "more or less." India has continued to publish extensively on its economy and its problems. Since mid-1955, there has been a great expansion in the information available for mainland China in official communist publications and in an expanding flow of press and visiting mission reports.

In some major respects, comparison of the development record is hampered more by lack of data from "open" India than from "closed" China. It is more difficult to estimate national savings or investment when a private sector plays an important role in these basic economic activities than when they are more nearly the prerogative of government. Furthermore, officialdom in India has not yet adapted its statistical services to the requirements of a national development program. Thus, even the official record of the first five year plan does not present investment data for each of the years 1951-1956. There is no explanation as to how the rough figures for total investment over the five years were obtained, nor any indication of their sectoral allocation. It is not clear just how such basic magnitudes as total investment in sectors of the economy play their role in planning or in postauditing the plans. In contrast the Chinese, less encumbered, it is true, with the traditional statistics of normal times, focus pointedly on "accumulation," or on "capital constructions." They leave no doubt that these are key magnitudes for their own needs, in planning and in actual development operations. In these circumstances, nonofficial and even personal estimates play a significant role in the Indian data, as well as in the Chinese. On the other hand, it is possible to discuss such estimates with Indian officials, to study basic underlying data, and to observe. For China, much of the data, even official data, must remain simply numbers to most students. The data can be "tested" only through checks of internal consistency, or logical relationship to some past figures.

A. *Gross National Product and Gross Investment*

The aggregate figures used in the present analysis are given in Table 1.

1. *Relative rates of change.* India achieved an annual rate of growth of real income of almost 3.5 per cent in the period from April 1, 1950 through March 31, 1959. Over essentially the same period, the Chinese growth rate was at least three times as great. China recorded impressive gains in the preplan years when expanding meant primarily reactivating and rebuilding; large increases in output were the results of greater use of existing plant rather than of new investment. It was not until 1952/53 that China regained past levels of aggregate real

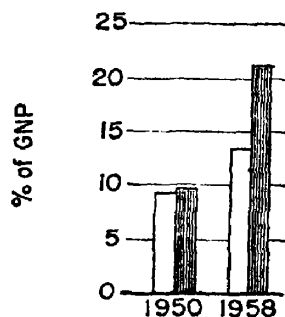
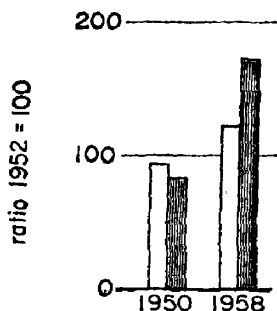
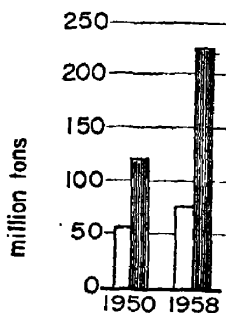
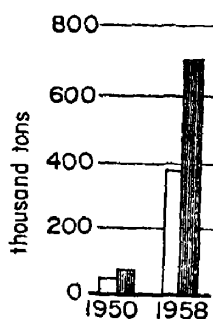
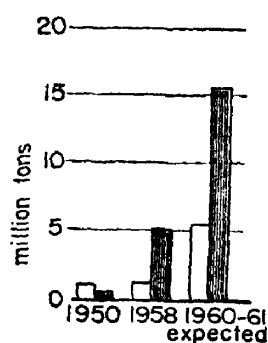
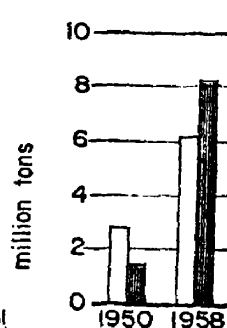
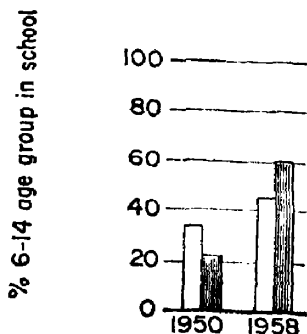
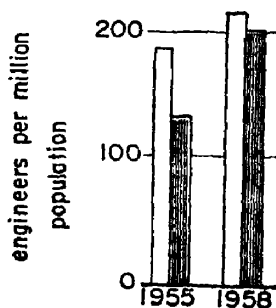
COMPARATIVE PERFORMANCE · 1950-1958
INDIA
CHINA
GROSS NATIONAL PRODUCT
Constant market prices
ratio 1952 = 100
200
100
0
1950 1958
GROSS INVESTMENT
% of GNP
25
20
15
10
5
0
1950 1958
FOOD GRAINS OUTPUT
million tons
250
200
150
100
50
0
1950 1958
FERTILIZER OUTPUT
thousand tons
800
600
400
200
0
1950 1958
STEEL PRODUCTION
million tons
20
15
10
5
0
1950 1958 1960-61 expected
CEMENT PRODUCTION
million tons
10
8
6
4
2
0
1950 1958
PRIMARY EDUCATION
% 6-14 age group in school
100
80
60
40
20
0
1950 1958
PROFESSIONAL TRAINING
engineers per million population
200
100
0
1955 1958


FIGURE 1

TABLE 1—GROSS NATIONAL PRODUCT AND GROSS INVESTMENT
(At Constant Market Prices)
A. India

| Year[a] | Gross National Product | | Gross Investment | | |
|---|---|---|---|---|---|
| | Billions of Rupees (1952 Prices) | Ratios (1952=100) | Billions of Rupees (1952 Prices) | Ratios (1952=100) | Per Cent of GNP |
| 1950 | 101 | 93.4 | 9.4 | 88.7 | 9.3 |
| 1951 | 103.7 | 95.9 | 9.9 | 93.4 | 9.5 |
| 1952 | 108.1 | 100 | 10.6 | 100 | 9.8 |
| 1953 | 114.4 | 105.8 | 11.1 | 104.7 | 9.7 |
| 1954 | 117.8 | 109 | 13.4 | 126.4 | 11.4 |
| 1955 | 120.1 | 111.1 | 14.5 | 136.8 | 12.1 |
| 1956 | 126.1 | 116.7 | 16.7 | 157.5 | 13.2 |
| 1957[b] | 126.8 | 117.3 | 17.2 | 162.3 | 13.6 |
| 1958[c] | 130 | 120.3 | 17.5 | 165.1 | 13.5 |

B. China

| Year[a] | Gross National Product | | Gross Investment | | |
|---|---|---|---|---|---|
| | Billions of Yuan (1952 Prices) | Ratios (1952=100) | Billions of Yuan (1952 Prices) | Ratios (1952=100) | Per Cent of GNP |
| 1950 | 55.02 | 81.1 | 5.31 | 52.6 | 9.7 |
| 1951 | 62.85 | 92.6 | 7.39 | 73.2 | 11.8 |
| 1952 | 67.86 | 100 | 10.09 | 100 | 14.9 |
| 1953 | 77.06 | 113.6 | 12.94 | 128.2 | 16.8 |
| 1954 | 81.92 | 120.7 | 15.65 | 155.1 | 19.1 |
| 1955 | 85.41 | 125.9 | 15.38 | 152.4 | 18 |
| 1956 | 97.21 | 143.2 | 18.62 | 184.5 | 19.2 |
| 1957 | 102.42 | 150.9 | 24.47 | 242.5 | 23.9 |
| 1958[c] | 117.50 | 173.2 | 25.85 | 256.2 | 22 |

[a] For India, accounting year begins April 1; for China, January 1.
[b] Estimates constructed from preliminary official component data.
[c] Orders of magnitude based upon general reports from the two countries. For China, the 1958 figures are conservative; it now appears certain that later estimates will be higher—by amounts which cannot yet be indicated.

*Sources and Nature of Estimates:*

1. All data for India are my personal estimates based upon official materials available through 1956/57 and to a lesser extent for 1957/58 and 1958/59. The government of India (GOI) does prepare estimates of national income both at factor cost and market price level; these are published in an annual "white paper" in March [6]. Government has not yet released official annual estimates of capital formation. Where the GOI does make use of investment figures these are given on a net basis (generally for groups of years together), for monetized investment only, and exclusive of changes in stocks [13, pp. 9-10], [14, pp. 8-11]. Finally, GOI estimates in constant prices are based on the year 1948/49.

Where a large proportion of national product is generated without benefit of financial transactions, as is true for much of agriculture and rural activity in India (and China) nonmonetized investment is of great importance. Indeed, in the very nature of the rural economies, it becomes difficult if not impossible to categorize any meaningful component

*(Continued on page 288)*

*Footnote to Table 1—Continued*

of private and sometimes, even public rural investment as the monetized component. Similarly, the accounting devices available when most output is produced by entrepreneurs who are generally illiterate raise doubts about the depreciation adjustment for the private sector, especially in small enterprise. Moreover, "replacement" for depreciated capital takes on a net aspect where the new equipment is modern and the existing machinery long outdated—the frequent situation in these economies. Official data provide estimates for depreciation on government account only. However, the GOI has released "unofficial" estimates on a gross basis and inclusive of transactions made on a barter basis [8, pp. 154-59]. These data and subsequent materials from the same source were used in deriving the estimates for Table 1.

Preliminary unofficial estimates for changes in working stock suggest that these expanded by only 1.4 billion rupees over the first plan. The year-to-year pattern of changes indicates wide annual variations rather than any persistent movement. In view of the partial nature of these data, they could not be incorporated into Table 1. Finally, all data available only in current prices, or in constant 1948/49 prices, were shifted to a 1952/53 price basis by the use of official series, with separate price indexes applied for the consumption and investment components of the gross product. (Thus the data of Table 1 do not reflect the post-1952 tendency for the unit costs of investment to increase relative to other costs.)

All these adjustments serve to raise the general level of both the national product and investment series for India in the table above those usually presented; year-to-year fluctuations are less affected, especially for the national product series.

2. Data, 1950-57, are from W. W. Hollister [5, pp. 132-33]. These were constructed from budget and retail sales data, and augmented by direct estimates for farm consumption, private investment, various consumer services, and inventory changes [5, pp. xxii-iv]. Except for the inclusion of changes in working stocks, the data are analytically identical with those presented for India. I have added the 1958 figures. They reflect about as large an increase in real product as any actually achieved in the period since 1950. The investment ratio for 1958 (22 per cent) has some basis in official source materials. (Actual developments of 1958 assure that gross real product will be appreciably above that in the table.)

The gross national product figures of Table 1 are generally higher than those derived directly from official Chinese sources; also they increase at a somewhat slower rate than do the more official estimates. On the other hand, Table 1 estimates for gross investment tend to be below official estimates. (The most convenient source for the official data is in the translations prepared by the American Consulate in Hong Kong. A recent survey [26] gives basic data for national income, and its separation into consumption and investment. These are reported in current prices.)

Official estimates of output are formulated with a Marxist concept of value: they exclude certain transport and service earnings which are included in Western (and Indian) national income figures—and in the Hollister data. The differences in the rate of increase in the two series can be attributed at least in part to important underestimates in official output figures in agriculture, 1950-53, thus exaggerating the upward trend in total output over the plan years [5, pp. 17-23].

With respect to investment, the present estimates have been built up by major sectors, on the basis of all the Chinese material available. For 1953-57 these components and their total are consistent with the complete account of investment expenditures presented by the Chinese in their 1956 publication on the first five year plan [1, pp. 27-37]. This governmental source gives 89.2 billion yuan as the five-year total, on both public and private account, for various kinds of investment and for depreciation as well as for expenditures for the "maintenance of schools" and for "operating expenses of urban public utilities"—items appropriately considered current rather than capital outlays. Nonetheless, this comprehensive total is below the five-year total (92 billion yuan) of the official investment figures—and these are presented as net investment in fixed and working capital. The offi-

*(Continued at bottom of page 289)*

product. In India, on the other hand, the expansion has meant new levels of output more or less since 1950. In addition to comparisons over the entire period therefore, it is appropriate to contrast rates of growth for the respective first-plan periods, from 1951 in India and 1953 in China. Table 1 shows a 19 per cent growth in India's gross product during 1951-56, the first-plan period, in contrast to China's 51 per cent for 1953-57. There has been a higher annual rate of population growth in China than in India—perhaps 2 to 2.2 per cent as against India's 1.4 to 1.7 per cent in these years; increases in per capita gross income show a somewhat narrower margin for China over India than do increases in total income.

Gross investment ratios were close to the same level in 1950; thereafter they increased about three times as fast in China. Moreover, given the greater expansion in Chinese product, these ratios mean that in 1957 and 1958 the real level of gross investment in China was about five times what it was in 1950; in India, it was not quite twice the 1950 level. Can these differences be in any way attributed to differences in foreign capital inflows over these years? Apparently 3.1 per cent of China's gross investment in the years through 1957[1] (and some 2.1 per cent during the first plan) were offset by a net import surplus. The comparable figures are 8.2 per cent (and 2.3 per cent) for India. If anything, therefore, China financed more of its investment program on the basis of domestic savings from current income. However, this is quite consistent with the fact that China obtained relatively more assistance from other countries. The net import surpluses in China were financed essentially by loans from other communist countries and especially the Soviet Union. In India, on the other hand, a reduction in foreign ex-

[1] Data for 1958 were not included in this comparison, which is thus confined to the 8 years ending in 1957. As indicated in the next paragraph of the text, the preliminary evidence is that the contrast will be strengthened further when firm 1958 figures are available.

cial investment series appears to be the result of applying a net investment ratio to estimates of national income. No sectoral data are given, and the total net investment is consistently larger than other investment data—for "economic expenditures" and for "capital construction expenditures"—used by the Chinese for budgetary purposes. Components of these latter are given in detail and over time; they include working capital, research and development expenditures, and outlays for repair and replacement as well as for new capital. Because of the lack of component data in the official series and because its totals of *net* investment are consistently above careful estimates—including some by the Chinese themselves—for gross investment, the investment series of Table 1 are considered to be more meaningful than those officially presented.

It should also be noted that, in converting estimates in current prices to a 1952 basis, separate price indexes were applied to gross investment. This retains the 1952 price relationship of capital to other goods. (There is some tendency in China for the prices of other goods to increase relative to capital goods in the years after 1952—the opposite of that noted above for India. It was therefore important to eliminate the effect of this divergent movement from the original data.)

change reserves made possible some 60 per cent of the import surplus, with only the residual amount dependent upon foreign grants and loans.

China seems to have achieved some balance in its foreign accounts. For 1957, for example, the international account shows a small export surplus such as would be consistent with the need to repay past borrowings [5, pp. 127-29, 132-33].[2] India's foreign-trade deficit was larger in 1956/57 and again in 1957/58 than in any other year considered here. The need for an import surplus may long continue. The differing experiences on foreign account mean that gross domestic savings ratios have expanded at an even more rapid rate in China, relative to India, than is the case for the gross investment series of Table 1. This is also borne out by comparing the increase in domestic savings as domestic product expands. Over the entire period 1950-58, China allocated at least 40 per cent of the expansion in gross output to investment; for the first plan period, 1953-57, it was 44 per cent. Over the same two time intervals in India, the marginal propensity to save was almost 20 per cent, although for the first plan itself it was 38.5 per cent.[3] Even in democratic India, the propensity to consume was significantly smaller than is usually assumed to be the case in poor agricultural countries.

2. *Absolute comparisons.* China's higher rates of domestic savings obviously mean that India can allocate to consumption a higher ratio of its domestic product. Furthermore, current expenditures by government —part of consumption—run at appreciably greater relative levels in China than in India: the respective ratios in 1952 were 10 per cent and 7 per cent. During many of the years 1950-58, not more than 75 per cent of national product thus became available for household consumption in China; in India the percentage has yet to fall as low as 80 per cent and is usually about 85 per cent. What are the absolute levels

[2] On June 29, 1957, the Chinese Minister of Finance stated "—we may say that we are now in a better position to rely on our own accumulation to carry on national construction . . ." (quoted by P. C. Mahalanobis [15]). Mahalanobis, director of the Indian Statistical Institute and the person responsible for the statistical basis for Indian planning, attributed the favorable performance in China to a concentration upon the production of "basic industries (heavy machinery, heavy electricals, machine tools, steel, fertilizers, trucks, etc.)" to which Russian aid was in considerable measure directed. India's foreign exchange difficulties arise, he argues, from the failure to pursue a similar source. But a sufficient interpretation must run in terms of the growth of output and savings. Thus, assuming that Russia and Czechoslovakia, for example, were prepared to continue to export capital goods, China's ability to invest might have been at least as great if the export surplus of grains had been expanded at the expense of a growth in basic industries. Which is better depends upon the relative costs incurred in acquiring the same amount of capital goods by the two methods.

[3] No inconsistency need exist between this high figure for 1951-56 and the ratios for 1951-58 and 1953-58. The savings series fluctuates: its peak is in 1955/56. These computations are crude approximations to the marginal propensities at best, since they have been computed directly from the figures in constant prices.

of income to which these very different consumption ratios are applied? Can China drain larger shares of national product away from consumption because the Chinese have higher levels of living than do Indians?

Some absolute comparison of output in the two countries is possible.[4] India and China have had similar enough economies so that analytical problems in making the comparisons are relatively small. On the other hand, some of the data needed are not adequately reported, especially the prices. The data available permit the following conclusions:[5]

a. Per capita agricultural product was about 15 per cent higher in China than in India in 1952. This is consistent with a higher differential in favor of China for food grains alone, since other foods play a larger role in India and since China usually exports and India imports grains.

b. For the rest of the economy, per capita output was higher in India in that year. This conclusion involves some judgment, since data did not permit a complete comparison for all or nearly all other goods and services. For commercial fuel and power the per capita advantage seemed to be with China; in heavy industry, India was ahead by at least a 20 per cent margin; comparisons for other items ranged between these limits. Quantitatively, nonagricultural product per person might have been some 10 per cent higher in India.

c. Per capita incomes in the two countries in 1952 were essentially of the same order of magnitude—about Rs. 260 or 130 yuan. This is concluded simply from the preceding two statements. Agricultural income provided more than half the total product in China in 1952; it constituted somewhat less than half in India. Despite very large differences in favor of India in the national income estimates generally made, it seems more likely that the actual incomes were about the same in 1952.[6]

At this relatively early stage of the development effort in both countries, China was investing some 50 per cent more than was India out of essentially the same real product.[7] After allowing for the different levels of government consumption, it appears that the average level of household consumption in China was actually 10 per cent to 15 per cent below that in India. As Table 1 suggests, this situation changed rapidly. Sometime in 1955 or 1956—despite China's larger allocations to investment and to other governmental uses, and despite its more rapid

[4] Gilbert and Kravis [3], for the techniques to be employed.

[5] I am indebted to George Perry and Delmar Underwood, graduate students at Massachusetts Institute of Technology, for computations upon which these were in part based.

[6] Comparisons in which estimates in national currencies were converted directly into U. S. dollars at the going rates of exchange place per capita income in China in 1946 at $23, and in India at $43 [24, p. 9].

[7] In 1952, net import surplus financed 9 per cent of China's gross investment. India had a net export surplus equal to about 5 per cent of its gross investment.

rate of population growth—the per capita levels of household consumption began to forge ahead of the levels prevailing in India.

### B. *Levels of Physical Output*

1. *Agriculture.* Table 2 presents series for the all-important food grains, as well as for sugar cane and the major fibers, cotton and jute. Up to 1958—for which results are still uncertain—the data indicate a larger expansion in food grains in China than in India. However, India's performance during its first plan was more impressive than was China's in 1953-57—a 30 per cent increase compared with a 22 per cent increase. On a per capita basis, domestically produced food grains in India increased very rapidly from 335 pounds in 1950/51 to 444 pounds in 1953/54. The level in this latter year of very favorable weather was not again attained in the four following years. Indeed output per capita in 1957/58 has fallen below 400 pounds. In China, there was an early increase from 500 pounds to 600 pounds; favorable harvests in 1954/55 then boosted the per capita figure to 645 pounds. The two years through 1957/58 have seen somewhat higher levels, close to 660 pounds per capita. Given the importance of weather in the year-

TABLE 2—AGRICULTURAL OUTPUT: MAJOR COMMODITIES

| Year[a] | Food Grains[d] (Million tons) | | Cotton (Thousand tons) | | Jute[e] (Thousand tons) | | Sugar Cane[f] (Million tons) | |
|---|---|---|---|---|---|---|---|---|
| | India | China | India | China | India | China | India | China |
| 1950 | 53.5 | 122.72 | 509 | 681.5 | 586 | 77.6 | 56.2 | 3.1 |
| 1951 | 55.06 | 132.92 | 548 | 1014.3 | 835 | 245.7 | 60.7 | 4.6 |
| 1952 | 62.45 | 151.96 | 559 | 1283.1 | 820 | 300.7 | 50.2 | 7 |
| 1953 | 74.08 | 154.42 | 690 | 1156.2 | 552 | 135.7 | 44.2 | 7.1 |
| 1954 | 69.76 | 157.9 | 740 | 1048.1 | 523 | 134.5 | 56.9 | 8.5 |
| 1955 | 69.93 | 174.81 | 700 | 1494.4 | 749 | 252.7 | 59.3 | 8 |
| 1956 | 73.2 | 182.5 | 830 | 1445 | 768 | 258 | 67 | 8.7 |
| 1957[b] | 67.1 | 185 | 835 | 1640 | 730 | 305 | 64 | 10.2 |
| 1958[c] | 72.5 | 225 | 825 | 2500 | 750 | 325 | 65 | 13 |

[a] Crop year beginning in:

[b] Official estimates for 1957/58 subject to final revision.

[c] Preliminary. Estimates given are conservative; final figures may turn out to be substantially higher.

[d] Excludes pulses but includes potatoes; rice in terms of paddy. (Data for India are usually given in terms of clean rice; on the average in India, 3 pounds of paddy yield 2 pounds of rice.

[e] Includes hemp for China.

[f] Excludes sugar beets which are important in China.

*Sources:*
India: 1950–54, (10, pp. 92–102)
1955–58 (9 current).
China: 1950–55 (10, pp. 92–102).
1956–58 (25 Despatch No. 884, April 30, 1958).

to-year pattern of output in both countries, however, it is perhaps more significant that only the Chinese data provide some evidence of a persistent upward trend. While this might suggest a greater measure of success in overcoming the natural and human deterrents to expanding production, the record is scarcely definitive.

Indian agricultural statistics are consistent with the claim that the potential for food-grains output has expanded in the course of the first-plan years to a new level, some 20-25 per cent above preplan production. At least half of this might be attributed to additional acreage, mostly the result of expansion in the area irrigated. The remainder could be due to the effects upon yields per acre of a number of developments, including more fertilization, better seed, and improved farm practices generally. However, these underlying factors would be expected to exert their influence gradually, while the output data suggest a shift to a new level during 1952 and 1953, without systematic expansion thereafter. Even the rather favorable estimate for the 1958/59 crop can be attributed in part to climatic conditions; in any event, grain output of 72.5 million tons does not necessarily mean that an upward trend in output has been resumed.[8]

Although food-grains output in China in 1957 is officially reported to exceed the five-year-plan targets by 1.9 per cent, Chinese sources have made clear that performance in this sector has been disappointing. And some observers abroad argue that the official record has actually exaggerated the true achievement.[9] But even so, limited progress is indicated through 1957. Of the 20 per cent expansion during the plan, about 75 per cent represented increases in yields per acre. To some extent this was also true in the preplan rehabilitation period, although acreage expansion was then relatively more important. Systematic change—the persistent growth in ouput, however small, and the consistency of the contributory factors—probably constitutes the most significant aspect of Chinese development in this area.

Other agricultural production in China also reflects the major re-

[8] A principal conclusion of the first round of India's National Sample Survey was that ". . . official statistics [of food-grains production in 1948/49 and 1949/50] seem to be under-estimates by something between roughly 20-25 per cent . . ." [7, p. 26]. While the official statistics do reflect a subsequent expansion of this order of magnitude, the NSS view is not universally accepted in India. See for example Dandekar [2, pp. 153-65].

[9] The American Consul at Hong Kong indicated that 1957 output levels were "not unreasonable but the means of derivation warn against treating them as solid" [24, Despatch No. 884]. Foreign experts claim that the data for 1950-1953 are too low and thus overstate the degree of improvement [5, pp. 17-23]. One careful study argues that the official figures, whatever the actual expansion they reveal since 1949, are still below the output levels of the early 'thirties. The 1957 target would only achieve that earlier production level—due to an underestimate (by about one-third) of the actual prewar output [22, *passim*].

habilitation efforts of the years through 1952/53. Thereafter it is only in cotton, a key product for Chinese industry, where there is evidence of a major drive for expanded output. In India products other than grain reveal about the same proportionate output increase over the entire period through 1957/58 as do the food grains. On the whole, the record in agriculture suggests a more impressive performance for China in the food grains; for India in other food products and industrial raw materials.

These offsetting tendencies are reflected in the indexes of agricultural output available for the two countries. From 1950 through 1957, aggregate output in agriculture rose by some 25 to 30 per cent in China and 15 to 20 per cent in India. This is a much smaller difference than exists for the food grains alone. Agricultural production has increased at a lower rate in China than has aggregate production; income from agriculture has become of lesser importance in the total product. For India, agriculture and the rest of the economy seem to have kept more nearly in line.[10]

The agricultural effort in China has been closely directed by government. While there was much talk of broadening and improving diets, the focus has been on the food grains, in order to derive maximum energy output per unit of expenditure for agricultural expansion. Even in adverse-weather years the Chinese did succeed in expanding grain output by about as much as population. The picture in India has been quite different. The degree of government direction was relatively small. Profitability considerations governed with respect to commercial production. The bulk of the output, especially grains, is produced for local or even for producer use, and the underlying motivations here are less readily characterized. Certainly the programs of government—for expanding the acreage under irrigation, for improving methods of cultivation including the greater application of commercial fertilizers—had a smaller influence than the development plans promised. Weather remained the predominant factor and accounts for the largest increases in production observed over past years.

The scope for improvement in agriculture remains large, and particularly in India [18, pp. 7-8, 11-12]. Thus rice, the preferred food grain, accounts for about half the grain output in both countries. Yet the yield per acre in China in 1957 was about half that in Japan; it is generally about twice that in India. Officialdom in both countries has recognized the need to devote much greater effort to this sector. Thus, over the past two years, Indian leadership has increasingly questioned whether a basis for systematic expansion of food-grain output has in

[10] See below, p. 301.

fact been established in India. Today this is the major problem on the Indian development scene, and new programs for agricultural progress are receiving ever greater priority.[11] In China, new efforts initiated in 1957 began to manifest themselves early in 1958. Already the rapid extension of small-scale irrigation, the increased rates of fertilization, and much more intensive cultivation bid fair to assure record levels of crop yields for 1958/59, and this despite relatively unfavorable weather in many parts of important agricultural regions.[12]

2. *Industry.* For major industrial products (Table 3) the comparative records are more straightforward. Aggregate industrial output in the modern sector has made much greater progress in China. Both indexes give some evidence of a slackening of the rates of growth of big industry during 1957, although current reports from China suggest that the revised figure for 1958 will testify to a resumption of a pronounced upward trend. In interpreting the series, it should be noted that 1950 was still a year when expansion was primarily the result of reactivation and rehabilitation of existing plant, and this was true to a greater extent in China than in India. Also the Chinese indexes suffer from considerable double counting given Marxist procedures for adjusting for intermediate goods in production. Fortunately, available data permit straightforward comparison for physical output of more or less identical products.

For every commodity in Table 3 Chinese rates of increase have exceeded India's by sizable amounts. In some cases—steel, cement, electrical power, textiles—Chinese output and capacity were smaller in 1950 than India's. In fertilizer and coal, larger absolute production in China was about the same, or even smaller on a per capita basis. In every case, however, production levels for these major commodities are now above those of India. Through 1957 India did retain some advantage in cement and fertilizer on a per capita basis, but it is probable that such margins are now disappearing.

On the basis of the less comparable information available for consumer goods, Chinese performance is not nearly so spectacular. As in

[11] India's National Development Council, in January 1958 and again in May, made pointed reference to the unevenness of past results and to the gap between expenditures and performance. Striking were the observations on the failure to make use of the irrigation facilities already constructed. For the outlines of the crash program adopted for the current crop, see [11, pp. 39-49] [12, pp. 13-15].

[12] Reports stress the "big leap forward" in crop yields. Thus the Chinese claim that the major early rice crops already promise a 50 per cent increase over the 1957 yields per acre. Total food-grain output is officially projected at 300 million tons, almost 65 per cent above last year's crop. [25, Despatch No. 995, June 13, 1958; No. 364, November 3, 1958]. While this may reflect early optimism, a record increase can be expected. As was indicated earlier (p. 293), there is evidence that first-plan levels of grain output are well below prewar figures of 200 to 220 million tons [22, pp. 11-20].

TABLE 3—INDUSTRIAL OUTPUT: MAJOR COMMODITIES

| Year[a] | Aggregate Production 1952=100 | | Steel (million tons) | | Cement (million tons) | | Electric Power (million kwh) | | Coal (million tons) | | Fertilizer (Ammonium Sulphate) (thousand tons) | | Textiles (million yards) | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | India | China | India | China | India | China | India | China | India | China | India | China | India | China |
| 1950 | 85 | 37 | 1.01 | .40 | 2.68 | 1.41 | 5,112 | 4,580 | 32.5 | 40.9 | 47.3 | 75 | 3,650 | 2,940 |
| 1951 | 96.5 | 76 | 1.08 | .79 | 3.19 | 2.48 | 5,856 | 5,790 | 34.3 | 50.8 | 52.7 | 129 | 4,065 | 3,570 |
| 1952 | 100 | 100 | 1.10 | 1.19 | 3.54 | 2.86 | 6,192 | 7,261 | 36.1 | 63.5 | 220.3 | 181 | 4,600 | 4,700 |
| 1953 | 102 | 129 | 1.02 | 1.56 | 3.78 | 3.88 | 6,708 | 9,165 | 35.8 | 66.6 | 319.6 | 226 | 4,875 | 5,900 |
| 1954 | 109 | 152 | 1.25 | 1.95 | 4.41 | 4.60 | 7,500 | 11,001 | 36.8 | 79.9 | 340.2 | 298 | 5,000 | 6,250 |
| 1955 | 118 | 166 | 1.26 | 2.51 | 4.48 | 4.50 | 8,496 | 12,278 | 38.3 | 93.6 | 393.1 | 324 | 5,090 | 5,330 |
| 1956 | 128 | 226 | 1.34 | 3.88 | 4.93 | 6.42 | 9,636 | 16,588 | 39.4 | 105.9 | 389 | 446 | 5,310 | 6,500 |
| 1957 | 133 | 244 | 1.35 | 4.26 | 5.58 | 6.69 | 10,836 | 19,025 | 43.5 | 130 | 383 | 535 | 5,315 | 5,825 |
| 1958[b] | 137 | 288 | 1.27 | 5 | 6.07 | 8.18 | 12,198 | 23,000 | 44.8 | 165 | 381.6 | 700 | 4,925 | 6,250 |

[a] Calendar years.

[b] Preliminary: for India, based on monthly reports through September 1958 [21]; for China, based on latest available U. S. government reports [25]. Estimates given are conservative; final figures may turn out to be substantially higher.

*Sources:*

India: Official Series [21].

China: Based on figures in [25]; prior to 1952, estimated from employment and other data.

the case of textiles, there is evidence of a deliberate limitation of industrial production for consumers whenever such output would curtail the supplies of power, transport, management and other scarce inputs for the hard producer-goods industries. On the whole, output of consumer goods by modern industry seems to have expanded more in India during 1950-57, and from an originally higher level.

Details as to China's handicraft and cottage industries under the Communists are not at hand, but there appears to be little parallel to the direct emphasis given this sector, so important in consumer-goods output, by the government of India. On the other hand, the Chinese have given much attention to the expansion of modern small-scale industry. This sector constitutes an important adjunct of big industry in a labor-rich country, and China has deliberately furthered an increase in output from a wide variety of small industries—including the production and processing of chemicals and pig iron. Achievement here is far beyond what has yet been accomplished, or even projected, by India.

3. *Education.* In regard to education and professional training, India seems to have started its development program with a considerable advantage. There were some 22 million children in school in 1950, almost one-third of those in the 6-14 age group. Today there may be 37 million, 45 per cent of those in this age group [14, pp. 501-4] [11, pp. 91-93]. China had a lower percentage in 1950 (22.5 per cent) but is reported to have almost 60 per cent of children aged 6-14 in school today [1, pp. 201-3] [25]. At the other end of the educational process, China in 1955 was training annually 30.9 engineers and 11.2 medical doctors per million persons in its population. Comparable figures for India were 18.4 and 8.1 respectively. Continuation of these rates would, within ten to twenty years, reverse the more advanced position which India had in these professional fields in 1955 [15, p. 5].

Additional activities might be mentioned, although the major areas have been considered. Thus, cinema attendance and passenger travel have increased considerably in India over the past decade or so—probably much more than in China and again from an initially higher level. It is not possible to extend such physical comparisons to encompass the entire national product. Nor will this ever reflect differences of a qualitative sort—the variety of food and other consumer goods in India, the opportunity to select goods and services. On the whole, however, the Chinese margin of 1952 in per capita agricultural production does seem to have been maintained—and even widened over the years to 1958. With respect to big industry, India lost ground relatively—the result of a large expansion in China which made its modern industry sector a much more important part of total product than is

India's. However, the actual availability of nonfood consumer goods to the Indian citizen seems to have increased more than in China. Given the major role of grain in consumption and of food in the levels of living in the two countries, these general statements certainly do not impair the plausibility of our earlier conclusion on the relative growth of national output in India and China.

The record of comparative performance thus reveals that China has taken greater strides in investment, and this on the basis of greater reliance upon domestic savings. Gross output per person actually increased more than twice as fast as India's. Indeed Chinese consumption (governmental and personal) per capita in 1957 was about 20 per cent above the 1952 figure; the comparable increase in India was 8 per cent in 1955/56 over 1950/51. The present analysis thus indicates economic developments overwhelmingly favorable to the Chinese effort, both with respect to actual performance and to potential for further growth.

## II. *Reasons Underlying the Contrast in Performance*

What explains the different results? One characteristic of the data which warrants special attention is the relationship between investment and total output. Table 1 yields the following ratios of gross investment to increase in gross product over the indicated time intervals:

| | *India* | *China* |
|---|---|---|
| 1950-1957 | 4 | 2.2 |
| First-plan | 3.1 | 2.5 |
| Pre-first-plan | 6.7 | 1.4 |
| Post-first-plan | 5.1 | 1.7 |

For the period as a whole, China seems to have generated a unit of gross-income flows with little more than half the gross investment that was applied in India.[18] Only for the plan periods proper are the cal-

[18] So striking are these differences that the computations warrant a few further observations. The underlying statistics of product and investment were presented as comparable sets of estimates (see notes to Table 1, above). In fact, the major departure arose from our inability to include changes in working stocks in India's gross investment; on this account the ratios above *understate* the difference between India and China.

Official Chinese figures for depreciation in the economy as a whole do tend to be on the low side. But these estimates do not play a direct role in the figures of gross investment used here. In any case, low depreciation levels in the official statistics mean high estimates of accumulation (net investment) rather than low estimates of gross investment. Another point is that, in communist countries generally, prices of capital goods, and indeed unit costs of capital formation, tend to be low relative to other prices. This might in itself provide lower capital-output ratios than prevail in a free market economy. However, while it was not possible to make a direct comparison of the pertinent price relationships in India and China in 1952, the deflating procedures used here maintained in each country the price relationships of that year. The different relative movements over the years should thus not affect the capital-output calculations.

With regard to the ratios themselves, it is true that they fluctuate markedly from year to year in each country; most of the specific figures above would thus be different were

culations for the two countries in reasonable line—with India's about one-quarter above the figure for China. The record thus indicates greater "efficiency" in China in converting a given amount of gross investment into additional capacity or at least product.[14]

This phenomenon is magnified by the fact that the Chinese have been able to increase their allocations of current product to investment at a more rapid rate. Rough calculations suggest that for the period as a whole, 55 per cent of the difference in the rate of growth in the two countries can be attributed to the greater efficiency with which the Chinese apply investment; 45 per cent is thus attributable to the more rapid rate of expansion in investment.

A. *Different Allocation of Investment*

The different results must be traceable to allocations of investment, and to the specific forms in which investment is actually made. Thus, scale of plant may reduce capital costs per unit of product; investment costs will be low if outlays for labor in construction, for example, exclude or undervalue contributed or forced labor, and the like. What evidence is there for differences betwen the development effort in India and China in these regards?

While data will not permit ready comparison of the allocation patterns in all years, it is possible to separate out for both countries the investment in two broad groupings: agriculture, including irrigation, water conservancy, community development and some simple handicrafts close to agriculture such as rice polishing; and industry, which includes modern industry and power. Table 4 gives these allocations on a percentage basis for groups of years.

Over the period, investment has become relatively less important in agriculture and more important in industry, as would be expected. Both movements have been more marked in China. More significant, however, are two other comparisons reflected by the table. Contrary to the impression which prevails generally,[15] a large percentage of total invest-

the time coverage altered somewhat. Yet, there are no groups of years which would give reverse results, or indeed very different relative results, for the two countries. Thus, were rough adjustments made for the special influences which weather had upon output in some years in each country, the relative ratios would reveal a wider margin in favor of China than is shown for the two plan periods, for example. Similarly, inclusion of the preliminary data for 1958 does not appreciably alter any of the calculations. Thus, differences observed are not attributable to the particular groups of years selected for the comparisons.

[14] In characterizing the conversion as more "efficient" I mean only that less capital is used for the same volume of output, presumably with the same lag.

[15] My earlier observations [18, pp. 13-14] were also in this vein. Actually, agricultural investment was planned at less than 10 per cent of the public program but much larger amounts were to be invested by the peasants themselves ". . . in addition the development

TABLE 4—ALLOCATION OF GROSS INVESTMENT
(Per cent)

| | Preplan | | First Plan | | | | Second Plan |
|---|---|---|---|---|---|---|---|
| | | | First 3 Years | | Last 2 Years | | |
| | India (1950) | China (1950–52) | India (1951–53) | China (1953–55) | India (1954–55) | China (1956–57) | India (1956–57) |
| Agriculture | 28 | 32.7 | 26.5 | 27.6 | 26.5 | 27 | 24.5 |
| Industry (including power) | 23.4 | 36.6 | 25.6 | 35.5 | 24.3 | 45 | 29.2 |
| Other sectors | 48.6 | 30.7 | 47.9 | 36.9 | 49.2 | 28 | 46.3 |

*Sources:*
India: My estimates based on provisional data released by government [9, pp. 154–59].
China: Estimates based primarily on [5] [25] [1].

ment has been allocated to the agricultural sectors in China; throughout the period, this percentage has exceeded India's. In addition, there has been a marked difference in the emphasis given by China to physical product as against services. Thus, despite the superiority of India's road and railway network in the preplan years, India in the last few years seems to have allocated almost as large a proportion of total investment to transport and communication as China allocated to these plus social services, trade and finance, education, health and the like.

The striking contrasts notwithstanding, Table 4 itself throws little light on the problem of "efficiency" of investment. China puts more intc agriculture, which has frequently turned out to be a sector where relatively large returns follow from a unit of new capital—at least in underdeveloped areas where yields per acre are very low initially. On the other hand, the still larger Chinese allocation to industry, and to a type of industry where the capital/labor ratio is high, might well operate the other way. Also India's emphasis upon tertiary sectors would suggest on the whole a larger increase in output from the same level of new investment, although this argument is not firm, since transport itself tends to have a high capital-output ratio, as does also investment in housing, included here under social services. On this last, one point does warrant mention. The greater allocations to services might mean more investment in the overhead sectors which in turn will permit greater returns from direct investment in the future. India's past pattern, in other words, may well be the more efficient, given a longer time

of agricultural credit cooperatives will make it possible to draw a huge amount of idle capital into agricultural production" [1, p. 33]. A delegation of Indian governmental specialists in agriculture also reported this same preponderance of agricultural investment in China relative to India [10, pp. 133-35].

horizon. However relevant this possibility, other evidence does not suggest that the rate of growth prospects for the years ahead can be considered more favorable to India as a consequence of the current investment patterns.

### B. *Some Sectoral Capital Coefficients*

A few precise relationships can be traced with the data at hand. Thus, the ratio between gross real investment and the increase in real income in agriculture in India is 2.33 for the period 1951/1952 through 1956/1957. Essentially the same figure (2.28) is obtained for China for 1951 through 1957.[16] A comparison of the investment which corresponded to an increase of (productive capacity for?) one million long tons of food grains over the period also reveals a similar parallelism in the experience of the two countries. Again, there is some basis for imputing an advantage to the Chinese on the ground of relatively more consistent results over the period.[17]

While the evidence in the agricultural sectors can be interpreted as suggesting reasonably comparable capital-output relationships, for industry it indicates a clear advantage for China. The gross capital-output ratio for Indian industry and power was at least 6:1. For China, valued-added computations for industry are less readily presented, given the problem of double counting. The ratio of gross investment in industry to the *total* value of industrial production lies in the .9-1.1 range for different groups of years during 1950-57. On the assumption that total value is of the order of three times the value-added,[18] this suggests that the comparable capital-output ratio for China would be about 3:1—or half the level of India's.

The national income series in the two countries show quite different patterns with respect to the relative importance of income from agriculture. In India this ratio was 50 per cent before the plan, 47 per cent during it, but seems again to be close to 50 per cent in the recent second-plan years. The accounts for China show a fairly steady decline from 70 per cent[19] in 1950 to 50 per cent in 1957. Thus, the major

[16] The ratios appear to be more volatile in India. Thus, a much less favorable relationship is obtained if the computation for India includes the disappointing crop year 1957/58.

[17] Essentially equal results in these calculations involve an assumption that the official yuan:rupee rate of 1:2 undervalues the yuan to some extent. Other evidence on relative prices does not support this inference. See for example, [10, p. 41] [20, pp. 60-61] and above, p. 291. (For whatever relevance it has, the recent Hong Kong free-market rate of 1:1.2 suggests the opposite.)

[18] Based on computations made by W. W. Hollister (as reported in a personal communication).

[19] This figure reflects the underutilization of existing nonagricultural plant in the 1949-51 period. By 1951 the ratio was close to 60 per cent and it has been declining rather steadily since then [5, p. 9] [6].

sector where capital-output ratios seem to be more or less the same in the two countries became of appreciably less importance in China. A sector where the ratios are notably different—modern industry—has grown relatively in China. Finally, the different rates at which the tertiary sectors expand would also favor larger product, at least in the short run, in China. Together, these different sectoral results may explain the differences in over-all capital-output ratios—although nothing in the above explains these sectoral results.

### C. *The Relative Scale of Public Investment*

There are important differences between India and China in the relative scales of public investment. While this direct role of government in development has been expanding at a more rapid rate in India (Table 5), the Chinese ratio for the economy as a whole in the past two years was still double that of India. Modern industry and power are today essentially entirely within China's public sector, and even in agriculture, which was relatively free enterprise in 1950, government participation is now more important than is true in India.

Does the degree of centralization of investment activity influence efficiency? Presumably, identical enterprises could be established and

TABLE 5—THE RATIO OF PUBLIC INVESTMENT TO TOTAL INVESTMENT
(Per cent)

| | Preplan | | First Plan | | | | Second Plan |
|---|---|---|---|---|---|---|---|
| | | | First 3 Years | | Last 2 Years | | |
| | India (1950) | China (1950–52) | India (1951–53) | China (1953–55) | India (1954–55) | China (1956–57) | India (1956–57) |
| All Sectors | 27.8 | 67 | 28.4 | 76.3 | 38.2 | 81 | 38.7 |
| Agriculture | 28.4 | 20.6 | 29.9 | 31.9 | 41.4 | 39.3 | 35.2 |
| Industry | 17.8 | 89.6 | 19.8 | 93.7 | 28.1 | 96.7 | 34 |

*Sources:*
India: My estimates based on provisional data released by government [9, pp. 154–59].
China: Estimates based primarily on [5] [25] [1].

managed with comparable efficiency by government, or by private interests or by the two combined in some mix. But limited delegation of authority in government enterprise might serve to make public operation less efficient in a relatively free-market economy. Conversely, the talents of the private entrepreneur must be less evident in an economic order in which his operations are intertwined with operations of government-controlled economic activities. On such counts alone, the output from enterprises in a mixed economy may be smaller than from the

same enterprises in a more monolithic economic order. However, the relative efficiency of the two patterns revealed in Table 5 cannot be assessed simply by evaluating the performance of public and private enterprises under different conditions. More relevant is the fact that the seemingly same type of enterprise becomes quite different in the highly centralized Chinese effort and in the much less controlled program in India.

Within agriculture, within industry, within each category of service, there is in each country a different collection of end-products which comprise output. Of the current flow of goods and services in the Indian economy the private sector still creates 90 per cent. Product must thus satisfy the demands on a relatively free market. Today, even steel is produced almost entirely in the private sector. While government allocates most of the steel available in India, significant percentages of the total supply (50 per cent or so) does not go to government. In China, essentially all steel finds its way into defense, railways and the broad public development program. Output per unit of investment must be lower for a steel industry which processes to meet the needs of many different users. This is even more true for consumer goods. Thus the modern textile industry in India produces a greater number of counts per million yards than does *any* other country's, and particularly China's. Increasingly, construction specifications for all buildings in China have been tailored to meet standards for costs per square foot established by region. India has moved much less far here.

As there are differences in product from the same type of enterprise, so also with respect to production techniques. Thus, it is relevant that China does use, in construction activity particularly, a large volume of unpaid or underpaid labor. Frequent attention has been called to the possibilities in this direction in lands like India and China where the social order generally provides for consumption even though workers are persistently underemployed. India seeks to mobilize this productive capacity through various voluntary programs, notably in the community development projects. Chinese efforts of this sort seem to have gone much farther and on a less voluntary basis [4, pp. 27-28].[20]

The predominance of public investment in China may thus contribute to lower investment costs per unit of product because government restricts the degree of choice in both factor and product markets.[21] The

[20] In India [13, p. 116] the value of all popular contributions to the community development program was of the order of 3.5 rupees per person in the project area. This would mean at the outside less than 5 days of contributed labor per person over as long as 4½ years in some villages.

[21] Many people would argue that freedom of choice and variety itself are attributes which enhance product values. The question here is only the point at which they impinge upon (physical) output in very poor countries.

private sector is indeed a decisive force in India's economy. In the plans, government has consistently overestimated the extent to which private enterprise will govern its investment and production activities "according to plan." Thus, since 1950, government has actually invested less and the rest of the economy more than was planned. A vigorous private sector has retained a greater volume of its savings for direct investment; there was a reduced transfer of private savings to the public sector. New investment in private industry has been of a more capital-intensive type than was appropriate to India's factor endowments [16, p. 29]. In contrast, China's centralized control of investment has meant investment more nearly as planned and with centralized decisions as to the techniques for production. When unplanned developments occurred—unfavorable harvests, lower grain deliveries—the control of savings in the economy gave government considerable flexibility in adapting annual programs to actual events.

D. *Contrasting Policies with Regard to Saving*

Nearly half the difference in output performance arose from China's higher investment ratios. Government has assumed responsibility for a very large part of the savings function, through an expansion in the scale and forms of taxation, especially upon agriculture, and through the growing importance of state enterprises in many fields. Thus tax receipts more than doubled between 1950 and 1952; thereafter they increased by about 50 per cent, aggregating almost 15 per cent of gross national income in each year. Limitations upon private consumption and investment meant also that private savings (especially nonagricultural) moved readily to the public sector through the banks, other saving schemes, and the bond market. Indeed, government—central, provincial, and local—in China has spent at least 25 per cent of the total national income in each year since 1952. About 11 or 12 per cent went for the usual administrative activities of government, including military services, and 13 to 18 per cent for capital maintenance and expansion [5, p. 6]. An increasing percentage of this last was accounted for by the profits and capital consumption allowances of state enterprises. The total of domestic gross savings mounted steadily (with one setback in 1955) and have averaged 22 per cent of GNP in the past few years. Government development plans envisage a slight reduction —or at most the maintenance—of this ratio in the future.

Savings in India are still essentially private. Government surpluses on current account were not expected to be significant over the second-plan period.[22] The public investment program has essentially depended

[22] The plan shows a surplus of about Rs. 10.5 billion, but this is more or less matched by current rather than investment outlays in the development program [16, p. 17].

upon borrowing domestically and abroad. Actual performance from 1956 to date has apparently intensified this situation. Revenue from taxation has varied between 7.5 and 9 per cent of gross national income since 1950. While direct taxes on agriculture (land and income) have been increasing, they still account for less than 10 per cent of the total tax revenues in 1956/1957. Rough estimates of the total tax burden for agriculture suggest an order of magnitude of about 20 per cent of all taxes, as compared with a ratio at least three times as great in China. Profits and capital-consumption allowances from state enterprises in India are still essentially confined to the traditional public enterprises—the railways and postal services. These contribute a relatively small amount to government finance. The government of India, currently responsible for less than 40 per cent of total investment in India, is thus much more heavily dependent on outside, nongovernmental sources of investment finance than is the government of China, which is responsible for more than 80 per cent of total investment in that country. The domestic savings ratios in India do not seem to have expanded since 1955, and government has had considerable difficulty in attracting them to public investment. As mentioned earlier, India's public development program over the next few years will certainly be more heavily dependent upon foreign resources than in the past, and the turning point in this dependence cannot yet be foreseen [17].

## III. *Implications for Economic Development Elsewhere*

There are no magic formulae for achieving an expanding per capita income. The tasks confronting nations with very low levels of average output are most difficult at best. The rapid growth periods in India during the last few years of the first plan and in China in some of the years treated here need to be considered abnormal. They were related to favorable weather or to the exploitation of existing excess capacity. These apart, continuing progress requires persistent efforts by the people under capable and inspiring leadership.

Consider a few key problems and the different approaches to them in India and China. Most poor countries have abundant supplies of labor relative to capital. Along with the need to move toward more capital-intensive methods of production in certain sectors, there is a complementary role for labor-intensive pursuits in rural areas especially, but in cities also. The gains from expanding the productivity of labor now inefficiently used are enhanced by the fact that there do exist complementary capital resources—in existing equipment, in nonmonetized savings (such as surplus agricultural products)—which must be used locally, if at all. Despite the greater emphasis given by the Chinese to the development of modern industry, they have also devoted major

energy to the task of mobilizing these underutilized resources for plan objectives. Through a sequence of devices, culminating in today's communes, China's government has played a fundamental role in organizing local resources—labor, existing plant, raw materials, savings (especially nonmonetized) and leadership of both enterprise and public administration—to expand agricultural and industrial production.

Mention has already been made of nonmonetized savings. Wherever a significant part of national product is imputed because it never goes to market, there will be savings in the form of inventories of final product which are used directly, perhaps as payments in kind, in capital creation. In both India and China some 25 to 30 per cent of all gross investment may have occurred in nonmonetized form in 1950. This ratio has apparently not changed much in India, but it has declined markedly in China, partly because the Chinese attempt to siphon out of the rural areas whatever is or can be monetized, and partly because of the differential expansion of nonagricultural investment. In contrast to China, India does not even take explicit account of this important form of rural savings and investment, despite the fact that Indian experience has pointed up both its size and potential [23, p. 63].

Unemployment constitutes a political as well as an economic threat in underdeveloped countries. The employment objective explains part of India's preoccupation with the handicraft sector; emphasis here has even been allowed to interfere with cost and efficiency considerations in the production of consumer goods by the modern industry sector. Progress is only now beginning to be made toward the expansion of the modern small-scale sector which either competes with large industry or complements it. Great attention is now given this sector in Chinese industrial development. There are benefits to employment and output gains from using resources raised locally. The Chinese have found that emphasis on this small-scale sector of industry has also facilitated the program of regional development by providing employment opportunities away from the large industrial centers.

The inability of the large cities to provide as much employment as people seek has become most apparent, even in China with its strong stress on industrial development. The Chinese have recognized explicitly the dangers arising from the unemployment of overurbanization and have devised various measures (involving involuntary transfers of people, compulsory registration, etc.) for dealing with the problem. In this context, the emphasis on employment opportunities in the small centers and rural areas takes on greater importance. In India, the parallel dangers have not yet produced a clear policy. Growing overurbanization has tended rather to expand social overhead expenditures in

urban areas beyond what the development requirements of these centers might otherwise have been.

For some parts of the program, particularly for industry, the government of India has chosen to rely heavily upon the private sector. The experience here leaves no doubt about the major gains possible from the energy and drive of private entrepreneurs. But these can be realized over a prolonged period only when government also fulfills major investment responsibilities: in the social and economic overhead sectors, and in certain directly productive fields where private investment has limited experience and resource requirements are very high (e.g., steel and producers' goods) or where effective economic change involves a major program of social action (as in peasant agriculture, rural output generally). Fulfilling this complementary role in a society of mixed enterprise requires careful study of the flow patterns of domestic savings, and the determination to adopt policies which can appropriately influence this pattern.

Structural unemployment, underutilized resources, overurbanization, nonmonetized savings and investment flows—these are illustrative of the types of problems that must be understood and treated if there are to be steady output gains in most of today's underdeveloped areas.

Indian leadership has not yet assumed the responsibilities for organization and planning required to meet these problems. This is highlighted by the fact that the ratio of government to total expenditure is lower than in any other country for which national product statistics are available.[23] China's relative success in its development effort bids fair to be maintained, if not expanded. It is not realistic to expect internal pressures to impede progress. Indeed, apart from adverse harvests and the like, the years ahead may well see some reduction in the relative allocations of income to investment, and some increases in levels of living.

Are the contrasts in methods—and achievements—inseparable from contrasts in political philosophy? It is true that Chinese resource mobilization and allocation, Chinese methods of dealing with unemployment and urban growth, do weigh seriously upon the individual citizen and especially on a peasantry long proud of its individualism. These procedures could not be reconciled with the dictates of the Indian constitution, nor with the political and social philosophy of present-day leadership in that country. But alternative actions might well accomplish the same, or nearly the same, objectives. For reasons which cannot be attributed to India's adherence to the tenets of democracy, rural taxation

[23] For a complete listing of a related statistic, see [19, pp. 382-84]. The ratio for India is less than 1/3 that for the United States or Canada, for example.

is minimal; tax evasion is high; government controls a small proportion of the economy. The community development schemes offer an excellent and democratic mechanism for mobilizing idle or poorly allocated resources in rural areas. Economic inducements might deter the rural urban population push. The scope for such actions under democracy is broad.

The growing awareness of Chinese achievement relative to India's can have a profound influence upon world political and economic developments. But the lesson to be derived from the comparative performance of the two countries over these years of intensive development planning is not that totalitarian methods serve better than those conceived and implemented under democracy. It is rather that government in nations aspiring to economic expansion needs to define the tasks of growth realistically; more, government must implement them faithfully.

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# POPULATION AND ECONOMIC GROWTH

*By* EVERETT E. HAGEN*

It is likely that the disagreement that occasionally arises concerning population growth theory and "the population problem" is due in part to failure of communication, resulting from unclear statement of assumptions; but also in part to the persistence in our minds of Malthusian conclusions, even though the assumptions on which they are based may have been forgotten. I hope that this paper will eliminate at least some of the disagreement.[1]

In some analysis all population increase is lumped together in one general case: if per capita income rises above subsistence, population grows. It is conducive to clarity of thinking to treat causation more precisely, and to distinguish three cases of population growth.

Two of these relate to low-income (peasant) societies. In such societies, crude birth rates are virtually everywhere above 40 per thousand. Although higher birth rates are biologically possible, rates of 45 per thousand are close to the practical maximum. Historically, death rates have been almost as high. Where population growth occurs in such societies, it occurs because death rates fall. One cause of falling death rates is the introduction of modern public health and other preventive medical measures. I shall term the resulting fall in death rates exogenous. The fall in death rates is followed only after a lag of undetermined but considerable length by a fall in birth rates. In the interval the rate of population growth rises.

A second cause, marking the second case treated here, is a rise in per capita income.[2] Historically, there have been two major causes of rise in *aggregate* income, the opening up of new territories and technological progress. The former merely enlarges the scale of the economy and permits it to support more persons without directly affecting per capita income.[3] Technological progress, however, in general directly

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[1] I do not deal with questions of optimum population size or factor proportions in a static setting, or with questions of cyclical change in population growth rates.

[2] Of course the availability of public health and other preventive measures constitutes a rise in income. The reference here is to a rise in general purchasing power.

[3] The filling of "empty lands" was conspicuous in Western Europe during the Middle Ages, in China after 1650, and more recently in the history of the United States, Canada,

ıcreases per capita income. People live more healthfully, the death ıte falls, and the rate of population growth increases. If it increases ntil it equals the rate of growth in aggregate income, per capita income f course ceases to rise. With birth rates between 40 and 45 per thouınd, a decline in death rates to Western levels could bring population rowth at the rate of 35 per thousand, or 3.5 per cent per year.[4]

The recent spurt in population growth in the West constitutes the ıird case. By the 1920's or 1930's, the "demographic revolution" in ll Western countries had been completed, and birth rates had reached ːcular minima. Early in the postwar period, the rates in most Western uropean countries and in the United States, Canada, Australia, and ew Zealand rose markedly above those minima. For a time this pheomenon was regarded as the making good of deferred births, but it as now continued so long, and family sizes have so clearly increased, ıat at least part of the rise in birth rates must be regarded as reflectıg some secular change.

If an analytical model is to be of general applicability, it must be constent with all three cases. Section I presents historical data indicating ıat, contrary to Malthusian expectations and common belief, incomeıduced population growth has nowhere prevented even a moderate ıte of rise in the aggregate income of an economy from bringing connuing rise in per capita income.[5] A reasonable interpretation of history ıggests a mechanism at work which has guaranteed that it would not ɔ so. Section II presents a model incorporating this mechanism. Secon III comments briefly on the Malthusian model, and Section IV ːlates my alternative model to the three empirical cases and discusses s relevance to the future.

## I. *Historical Population Growth: the Data*

The historical distinction between exogenous and induced population ːowth is fairly clear. Exogenous declines in death rates have attracted ːtention mainly since the second world war. Through technical assis-

ıstralia, and New Zealand. In all of the recent cases except China, continuing technogical progress was of course occurring at the same time. Even where there is no progress known technology, there may of course be economies of scale which increase producvity. I shall not discuss their relation to population. The reader will be able to apply e argument of Section II.

[4] Life expectancy in Western societies is consistent with a death rate of 14 or 15 per ousand even with a stabilized age distribution of the population; and crude death rates 4 or 5 per thousand are possible with population concentrated in the lower age groups, it is for some time after death rates decline. Birth rates of 50 or 55 per thousand are ologically possible, and have prevailed in limited areas for periods of moderate length. hus a rate of population growth much higher than 3.5 per cent per year is conceivable. owever, this rate may be taken as a practical maximum.

[5] Of course income-induced population growth may prevent the rise in per capita income from being as fast as it otherwise would be.

tance from abroad, death rates in a considerable number of peasant countries have fallen drastically, most abruptly of all in Ceylon. Birth rates have remained high, and as a result in recent years population growth has been approximately 3 per cent per year, or more, in Ceylon, Malaya, Mexico, Venezuela, Ecuador, and several Central American countries and Caribbean areas, and well above 2 per cent in a number of other countries. Because of the recency of the declines in death rates, the minimum length of the lag before birth rates fall is unknown, except that it is probably more than a decade.[6] In the historical cases of less spectacular fall in death rates, the lag has been as long as several generations. If it should now be a generation or longer, the effect on living levels in low-income countries where rapid technological progress is not occurring may be catastrophic.

It is not so widely observed that the slower but increased rate of population growth in a number of peasant countries over a longer period has been due to the same mechanism. From the beginning of the Christian era to 1650, the average rate of growth of world population was in the neighborhood of 1/20 of 1 per cent per year. It then began to rise, first in Western Europe, but during the last half of the nineteenth century also in the peasant societies, which were then colonial. The modal rate in peasant societies between 1900 and the second world war was probably between .5 and 1 per cent per year. While precise data are of course not available, historical evidence indicates rather clearly that the level of per capita income in such societies had not risen before the rise in the population growth rate. There is also historical evidence that the increased rate of population growth has resulted specifically from gradual introduction of improved medical and health practices under colonial administration.[7]

Because some observers point to this population rise in peasant societies as evidence that a rise in per capita income may be swamped by population growth, it is worth while to assert specifically that the rise in aggregate income in peasant societies within the century before the second world war was predominantly a result, not a cause, of popula-

[6] The crude death rate in Ceylon, as recorded in the United Nations *Demographic Yearbook*, fell from 19.8 in 1946 to 14.0 in 1947, and continued down to below 10 in 1956. The crude birth rate, which remained between 38.4 and 39.8 from 1946 to 1953, fell from 38.7 in 1953 to 35.7 in 1954, giving possible hope of a spectacularly short lag. But in 1955 it was 37.3 and in 1956, 36.4. Age-specific birth and death rates for these recent years are not available.

[7] In China, by way of exception, population growth presumably resulted primarily from introduction from abroad of the sweet potato, peanuts, and early-ripening rice, which permitted the settling of large land areas that would not previously support population.

ion growth. The forces at work in these areas tending to increase the ncome of the mass of the population were extremely weak. Colonial dministrations did not induce continuing technological progress in he areas they controlled.

Continuing technological progress begins when an adequate base of cientific-technical knowledge is available (a condition now everywhere ulfilled), and when social and psychological changes have occurred uch that a sufficient number of the individuals of the society devote heir energies to problems of technological innovation. Certain condiions of capital supply are also necessary; they may be independent onditions or may follow if the social and psychological conditions xist.[8] During the nineteenth and twentieth centuries, these conditions ame to exist in twenty-some countries of the world. Continuing techological progress began, and brought an accelerated rate of increase n aggregate output. As this increase got under way, per capita income radually rose. The death rate gradually declined. The rise in income ermitted maintenance of larger and larger families, if desired. If the imple Malthusian model of population behavior[9] were realistic, as per apita income rose in each of these countries, at some point population rowth would have reached a rate equal to the rate of growth in agregate output, thus checking the rise in per capita income. Or, alternaively, if the rate of rise in aggregate output was so fast that it exceeded he realistic maximum biological rate of population growth—say 3.5 er cent per year—population growth would have reached that rate. 'he historical facts of income and population growth will test the thesis.

I shall examine the facts only for countries in which a moderate or apid rate of rise in aggregate output (1.5 per cent per year or more)[10] egan before the end of the nineteenth century, since where it began nly in the twentieth century it is possible that population growth has ot yet reached its peak. There are only 17 such countries. Simon Kuznets [6] in a recent compilation presents data concerning growth f output and population for 13 of these (see Table 1). While data for he growth of output in the nineteenth century are not available for the

[8] Some economists believe that technological progress begins when certain economic arriers, bottlenecks, or vicious circles are broken, and they ignore the social and psychogical factors. This difference of opinion concerning the causal factors is not important r the present purpose, but it is important to note that technological progress, not merely apital formation, is in point. The hypothesis that socio-psychological changes are of entral importance does not rule out the necessity of political change. This may in some ases be necessary in order that the new groups can be free to act.

[9] I use the term "Malthusian model" loosely here. For a statement of the essential elements of the Malthusian model, see Section III.

[10] I include Ireland even though its rate of increase in output was slightly lower.

TABLE 1—POPULATION AND NATIONAL PRODUCT, 13 DEVELOPING COUNTRIES: PERCENTAGE CHANGES PER DECADE, AND DECADES OF HIGHEST PERCENTAGE POPULATION CHANGE

| Country (1) | Period[a] (2) | Percentage Change per Decade | | Decade of Highest dP/dt | |
|---|---|---|---|---|---|
| | | National Product (3) | Population (4) | Approximate Dates[a] (5) | Percentage Change (6) |
| Australia | 1886/94–1945/54 | 26[b] | 17[b] | 1890–1900 | 21.5 |
| Canada | 1870/79–1950/54 | 41.3 | 18.3 | | |
| | 1870/79–1905/14 | 47.1 | 17.8 | 1899–1909 | 30.2 |
| Denmark | 1870/78–1950/54 | 30.1 | 11.5 | | |
| | 1870/78–1904/13 | 32.7 | 11.3 | 1913–1923 | 17.5 |
| France | 1841/50–1949/53 | 15.3 | 1.3 | | |
| | 1841/50–1901/10 | 18.6 | 1.9 | 1855–1865 | 4.7 |
| Germany | 1860/69–1950/54 | 27.4 | 10.1 | | |
| | 1860/69–1905/14 | 35.6 | 11.5 | 1894–1904 | 15.2 |
| Ireland-Eire | 1860/69–1949/53 | 12.8 | −3.5 | | |
| | 1860/69–1904/13 | 11.6 | −5.4 | 1938–1948 | 0.0 |
| Italy | 1862/68–1950/54 | 18.0 | 6.9 | | |
| | 1862/68–1904/13 | 15.7 | 7.0 | 1923–1933 | 8.0 |
| Japan | 1878/87–1950/54 | 42.3 | 12.7 | | |
| | 1878/87–1903/12 | 49.2 | 11.6 | 1937–1947 | 14.7 |
| Sweden | 1861/68–1950/54 | 36.0 | 6.6 | | |
| | 1861/68–1904/13 | | | 1938–1948 | 12.5 |
| Switzerland | 1890/99–1939/48 | 21[b] | 7[b] | 1894–1913 | 10.5[c] |
| Russia-U.S.S.R. | 1870–1954 | 31.0 | 13.4 | | |
| | 1870–1913 | 27.7 | 15.7 | 1870–1885 | 15.3[d] |
| United Kingdom | 1860/69–1949/53 | 21.5 | 8.0 | | |
| | 1860/69–1905/14 | 25.0 | 11.1 | 1869–1879 | 12.4 |
| United States | 1869/78–1950/54 | 41.2 | 17.4 | | |
| | 1869/78–1904/13 | 56.0 | 22.3 | 1873–1883 | 24.7 |

[a] Generally, Kuznets presents data for overlapping decades. His population data are shown as for intervals from one overlapping decade to another. The dates given in col. 5 are the fifth years of the decades he cites.

[b] The decade rate from the first to the last period covered. Other percentages in these columns are trend-line rates.

[c] The decade rate for the 20-year period.

[d] The decade rate for the 16-year period.

*Source:* Cols. 5 and 6, Kuznets [6, Appendix Tables 1-5, 7, 9, 10, 13-15, 17-18]; cols. 2-4, [6, Table 2, except Australia and Switzerland, which were computed from Appendix Tables 18 and 5, respectively].

other 4 countries,[11] it is clear from general historical comments about those countries that the course of events in them paralleled that in the other 13. In each country, the death rate fell as nutrition, health care, etc. improved. The rate of population growth would have approached the biological maximum if birth rates had simply remained at their previous level as income rose. What happened?

1. In no country except the United States and Canada—where vast empty lands cried out to be filled—did rise in income stimulate population growth remotely approaching that which Malthusian theory indicates rising income should induce. In no case except those of "empty lands" did the rate of population growth exceed 17.5 per cent per decade even for a single decade.[12] The median peak decade rate among the 13 countries is 12.5 per cent. The median rate for the 50 years of fastest growth is much lower.

2. In no country did the rate of population growth approach the rate of growth in aggregate output. In fact, in none except the United States did the *peak* rate of population growth for a single decade approach the *average* rate of growth in output for the entire 50- to 100-year period covered.

3. In England, there is evidence that in the eighteenth century, before the period of most rapid growth, rise in aggregate income began slowly and accelerated only gradually. Various studies give conflicting evidence about the timing of the rise in output, but all indicate that over the century as a whole, output rose. Phyllis Deane's estimates [2] [3] indicate an average rate of growth for the century of about 15 per cent per decade, but she thinks the growth was concentrated in the first half of the century. Population growth during the century failed to keep pace even with this moderate rate; it rose by about 6 per cent per decade.[13]

The evidence thus indicates not merely that the "Malthusian" result did not occur generally, but that it did not occur anywhere. Instead, birth rates followed death rates downward long before a maximum rate of population increase had been reached, and both continued downward until they reached secular minima.[14]

[11] Belgium, the Netherlands, Norway, and New Zealand. For all but Belgium Kuznets presents data for the twentieth century. In the twentieth century, rapid growth began in a number of other countries, for example, Czechoslovakia, Poland, Mexico, Brazil, Colombia, and perhaps also Hungary, Turkey, Argentina, and Chile. Some other Latin American countries, and possibly one or two elsewhere, might be added.

[12] In Australia, New Zealand, the United States, and Canada, during an early preindustrial period of filling empty lands, population growth reached higher rates than those shown in Table 1, but this growth is not relevant to the present argument.

[13] Concerning other estimates of income or production, see T. S. Ashton [1] and sources cited by him.

[14] See the sources cited by Kuznets [6]; or for a general description of the trend see any standard demographic discussion, such as R. R. Kuczynski [5].

These facts raise the important question whether technological progress is apt to bring such favorable results wherever it begins in the future. The uniformity of the past result suggests that there was some causal mechanism at work. It is worth while to inquire what that mechanism may have been.

## II. *An Analytical Model*

Let aggregate income be indicated by $Y$, population by $P$; let $v$ and $r$ respectively represent the percentage change per time period in aggregate income and population. In Figure 1 income per capita ($Y/P$, or $y$)
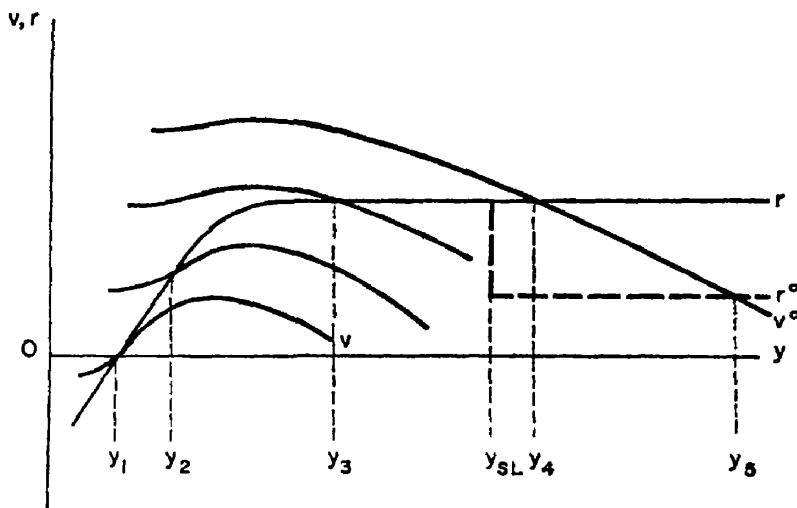
FIGURE 1. THE SIMPLE "MALTHUSIAN" MODEL

is shown on the horizontal axis, and rates $v$ and $r$ (abstract numbers) on the vertical.[15]

Full employment is assumed, and for simplicity the level of employment is assumed to bear a constant ratio to population. The $r$ curve is assumed to rise with rising $y$ until it reaches a maximum set by biological limitations (the practical maximum rather than the highest conceivable level), after which it remains horizontal.

Output is a function of capital and labor inputs only. "Land" will be introduced later. A conventional production function and constant

[15] The axes are those of Figure 3a in R. R. Nelson's article [9, p. 900], which seems to me the sharpest delineation in print of the Malthusian model. The $r$ curve is his curve, slightly adapted. For two other articles that use the graphic device of rates on one axis to deal elegantly with problems of economic growth, see R. M. Solow [10] and Trevor Swan [11]. I use a $v$ curve here instead of the more elegant Solow-Swan $sY/C$ curve ($s$ = ratio of saving to income), because the latter becomes unmanageable when land is introduced into the argument.

returns to scale[16] are assumed. Each $v$ function is a long-run function reflecting a constant state of the arts and a constant function relating saving ($S$) to income. An upward shift of the function results from technological progress or an upward shift in the $S/Y$ function or both. Movement along a $v$ function from a lower to a higher $y$ results solely from an increase in the ratio of capital inputs ($C$) to labor inputs ($P$). The shape of the $v$ curve depends on the operation of the law of variable proportions as the $C/P$ ratio varies with the advance to higher levels of $y$; for at different levels of $y$ the rate of saving and the rate of population growth will differ, thus producing corresponding changes in the $C/P$ ratio. The specific shape assumed does not affect the argument here, so long as the curve intersects the $r$ curve from above, as in Figure 1.[17] It must intersect thus to reflect the Malthusian assumption of a tendency for population increase to check the rise in per capita income. If by a fortunate accident or a lump contribution of capital from outside the system, $y$ were raised above $y_1$, for example, increase in population at a higher rate than in aggregate income would force it down again. This is the "Malthusian trap."[18]

It is assumed that there is some value of $y$ at which both saving and population growth are zero. This assumption, which is not in the least necessary, satisfies the simplest form of the Malthusian notion of a subsistence level of income. In Figure 1, let this point be $y_1$. Suppose now that through some exogenous force the $v$ function shifts upward. In this two-factor model, $v$ and $r$ will reach a new stable equilibrium, as at $y_2$. Population, capital, and aggregate income will increase indefinitely at a constant proportional rate, while $y$ remains constant at an increased level. If the intersection is in the horizontal section of the $r$ curve, as at $y_3$, then capital, population and aggregate income race upward at the same rate, and one also equal to the biological maximum possible rate of population increase. The rapid increase in population (labor inputs) causes no tendency for $y$ to decline, since labor force increase runs into no scarce cooperant factor.

[16] A different assumption concerning returns to scale slightly complicates the statement of the argument without affecting the conclusions.

[17] The more steeply the curve rises to the right of its intersection with the $r$ curve, the less the upward shift necessary for it to arch above the $r$ curve, eliminating the low-income-level intersection, and thus escaping the Malthusian low-income-level-equilibrium trap. Similarly, if the $v$ curve arches upward sufficiently, at the right of its intersection with the $r$ curve, it may intersect the horizontal portion of the $r$ curve from below, creating an unstable equilibrium that will permit escape from the low-level-equilibrium trap if some force pushes the system up beyond the point of unstable equilibrium. (See R. R. Nelson [9]). Since the empirical evidence indicates that the low-level equilibrium has not been escaped in either of these ways anywhere in the world, we are interested here only in the area in the vicinity of the low-level equilibrium; hence the precise shape of the curve is not important.

[18] The phrase "Malthusian trap" is Nelson's [9].

Suppose, however, that we assume the existence of a third factor, land, which is augmentable only at increasing cost per unit (or, if you choose, is available in absolutely limited supply). Then the value of $v$ at any given level of $y$ will depend not only on the level of techniques and the $C/P$ ratio, but also on the quantity of $C$ and $P$. As $C$ and $P$ increase, the marginal productivity of increments of capital and labor (for convenience think of a combined unit of capital and labor) will diminish after a point, and after a further point the value of $v$ for any given level of $y$ will fall. This fall in $v$ without change in the ratio of $C$ to $P$ constitutes a downward shift in the $v$ function.[19] This downward shift, inevitable in the absence of a renewed force tending to push the function upward, will continue until $y$ has returned to the subsistence level.[20]

Above some level of $y$, birth rates may fall because of a "standard of living" effect. That is, the attainment of this level of per capita income in some way induces a fall in birth rates.[21] In Figure 1, let $y_{SL}$ be this point. Then if the $v$ curve shifts upward to the position $v^0$, intersecting the $r$ curve to the right of $y_{SL}$, as at $y_4$, the effect of experiencing this level of living will be to shift the $r$ curve downward to the position $r^0$, and continued capital formation even without technical advance will cause $y$ to rise to $y_5$. However, $y$ will thereafter decline if the limitation on land supply continues to exert its influence, since that limitation will press the $v$ curve steadily downward and thus cause $y$ to move steadily to the left.

In the model described thus far in this section, which reflects fairly conventional assumptions about population growth, as per capita income rises the rate of population growth rises to the biological maximum. Further, per capita income turns downward secularly. Historically, neither phenomenon has occurred. A mechanism that will make the model more relevant to reality is created if we assume that technological progress continues throughout the time period being con-

[19] And is to be contrasted with movement along the curve because of change in the $C/P$ ratio.

[20] The simple assumption of land augmentability only at increasing cost is not realistic. The model is more relevant to reality if instead it is assumed that technological progress increases the quantity of land, making the upward or downward drift of the $v$ function a matter of the specific parameters employed. However, it may be well to carry through the analysis on the assumption of land augmentable only at increasing cost, since this is Malthus' assumption.

[21] Only a very broad assumption about the causal mechanism involved is necessary at this point. It should not be assumed that the direct operating force affecting birth rates is necessarily the increase in per capita income. The rise in per capita income is accompanied by a shift in the occupational composition of the labor force, urbanization, and a decline in death rates. Somewhere within this complex is the force that produces the standard-of-living effect. A specific assumption about the nature of the causal mechanism is made immediately below.

sidered and also posit the hypothesis stated below concerning the determinants of birth rates.[22] The hypothesis has two elements, a modified standard-of-living effect and desire to perpetuate the family.

1. The level of birth rates is determined by that of death rates. Birth rates are adjusted to death rates by the intention of the typical family to have two children grow up to parenthood so that the family will be perpetuated. Actually, to allow for the uncertainties of human life, and because in most cultures it is desired that one of the two children who perpetuate the family be male, the typical aim is to raise somewhat more than two to parenthood. Where death rates are at a level that birth rates cannot exceed, this intention is of course frustrated. But where conditions of sustenance permit, the basic long-run rate of population growth is a fairly low positive rate. In equilibrium it does not tend to rise above that, because it is limited by standard-of-living considerations. ("Fairly low" may be a rate varying among different societies between .5 and 1.3 per cent per year.)[23] It would not be inconsistent with this formulation to find this rate positively correlated with per capita income, but I find no empirical evidence that it is.

2. This birth rate and size of family calculus is, however, not primarily conscious and rational (though it is rationalized), but is imbedded in unconscious motives[24] relating to sex and family inculcated in children during their first six years.[25] Hence the minimum lag between

[22] The relevance of the model to reality is increased by a simple extension of the conventional standard-of-living thesis, namely, the assumption that a standard-of-living effect occurs at any level of per capita income above subsistence, if that income level is experienced for a minimum period. There is no theoretical justification for assuming any specific floor of per capita income below which a standard-of-living effect does not occur. This assumption eliminates the rise of the population growth rate to the biological maximum, except in the case of a very rapid fall in death rates. We should not, however, rest with this extension of the standard-of-living thesis, since further specification of the determinants of birth rates creates interesting results in the model.

[23] For consistency with the findings of modern social science, we must assume that this minimum rate is culturally, not biogenically, determined, and varies among societies. Ireland and China may perhaps be taken respectively as extreme cases of a low and a high tendency to population increase.

[24] I use the word "motive" in its technical psychological sense, as equivalent to the other technical terms "need," "motivation," or (as used by some psychologists) "drive." It does not refer to a conscious purpose.

[25] Perhaps during the "genital" period of the ages 3 to 6 years, when the relationship that Freud termed the "family romance" appears. Concerning this period, see, e.g., O. S. English and G. H. J. Pearson [4, Ch. 5]. The speculation that this period may be important in the determination of birth rates, while consistent with present-day theories of personality formation, is purely my own. The assumption stated in the text is oversimplified. Certainly the relevant motives are reinforced, or in the exceptional case altered, with resulting inner conflict, during adolescence. Thus during traumatic periods, birth rate changes might occur within a decade of events causing them, such as war. Such traumatic causes of change are ignored here.

a fall in death rates and the resulting fall in birth rates is the period from age 6 to parenthood. Further, since these sex and progeny mores are transmitted unconsciously, ones appropriate in association with high death rates may be transmitted for up to say four generations after death rates begin to fall. Between these limits, the upper one of which is based solely on historical data, the length of the lag varies inversely with the speed and conspicuousness of the decline in death rates. In England, where the decline was slow, gradual, and for some time hardly noticed, the lag was four generations; in Western Europe, it was shorter; in low-income societies now experiencing rapid and spectacular declines, it may be the minimum.

The alternative assumptions that a decline in birth rates depends on a rise in per capita income and on a fall in death rates are both consistent with the historical evidence, for until recent generations a rise in per capita income and a fall in death rates have occurred together. The latter assumption seems to the writer more consistent with modern psychological theory, but an empirical test must await another generation.

Since any single technological improvement will tend to shift the $v$ curve upward, continuing technological progress at any rate whatever will counteract diminishing returns sufficiently to hold per capita income above subsistence and cause a standard-of-living effect, though if land stringency is great and the rate of technological progress very slow, the system may return virtually to the subsistence level of $y$ within a few years, and the decline in the birth rate may be negligible. With a faster rate of technological progress, which holds the $v$ function appreciably above subsistence for a sufficient time, there will follow a noticeable decline of death and birth rates until both reach minima, after which a rise in per capita income may continue.

The process is illustrated in Figure 2. Let us assume for graphical simplicity that the $v$ function rises to its position 2 and hovers at that level. The upward shift causes a rise in $y$ and $r$ to the intersection $b$ of the $r_1$ and $v_2$ functions.[26] The rise in $r$ was of course via a fall in death rates. At the higher level of $y$, after a lag, birth rates also will fall. The resulting fall in the rate of population increase constitutes a fall in the $r$ function, as to position $r_2$. The value of $r$ falls from $b$ to $c$. Since $r$ is now less than $v$, $y$ and $r$ again rise, this time to its intersection $d$ of the $r_2$ and $v_2$ curves. The rise in $r$ is via a further fall in death rates. A further fall in birth rates then occurs, and the process is repeated, $r$ moving from $d$ to $e$ to $f$ to $g$ to $h$.[27] Here the process stops,

[26] The value of $y$ and $r$ will reach this short-run equilibrium via a path which lies between the two curves.

[27] While Figure 2 portrays the process as a "ratchet" effect, the downward shift of the $r$ function and the rightward shift of the values may be expected to be continuous.

if we assume that in position 4 of the $r$ function death rates (and therefore birth rates) have reached a minimum. Before death and birth rates reached their minima, the system passed through a period of considerable population growth, but the characteristics of the model guaranteed that as aggregate output rose population growth would not rise as fast, *i.e., that per capita income would rise cumulatively.*
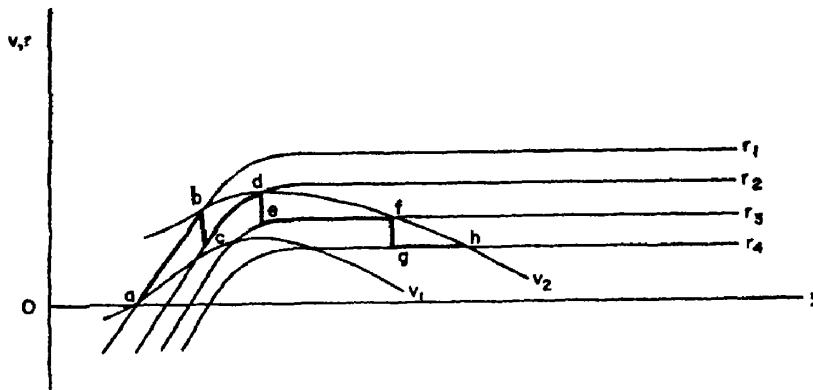
FIGURE 2. THE STANDARD-OF-LIVING EFFECT AT A LOW-INCOME LEVEL.

Before the system reaches point $h$, technological progress may have created new land (new minerals, cheaper ways of extracting them from low-grade deposits, plastics, new energy sources), thus pushing all or part of the $v$ function upward; or may have created new substitutes for labor (labor-saving inventions), thus flattening out the concavity downward of the function (raising the right-hand portion of the curve), and pushing the intersection $h$ to the right. For one or both reasons, the point $h$ may recede rightward, and rise in per capita income may continue. This has happened to date in the technologically progressive countries of the world, and the two tendencies show no evidence of coming to an end or even slowing down.

We may next consider the effects of exogenous declines in death rates in a system at the subsistence level of income. When improved medical and health measures are introduced, it becomes possible for the population barely to reproduce itself at a lower level of per capita income. Graphically, in Figure 1 the $r$ curve, retaining its former general shape, shifts to the left. There is a new subsistence level of income, the intersection of the $v$ and $r$ curves with the latter in its new position, somewhere to the left of $y_1$. There is now population growth at the income level $y_1$; it will cause per capita income to fall progressively until the new subsistence level is reached. In the process, death rates will rise again, starvation and diseases associated with malnutrition replacing malaria, venereal disease, etc., as major causes of death. Birth rates

may or may not fall during the shift, depending on the time period involved before the new equilibrium is reached, but in any case they will remain above death rates and the new equilibrium will inexorably be approached.[28]

If such an exogenous decline in death rates is brought about in a system experiencing technological progress, the increased rate of growth in population will tend to reduce per capita income, but equilibrium will be reached above the new subsistence level, and the process sketched above of reduction in death and birth rates to their minima and then continuing growth in per capita income will occur.

## III. *The Malthusian Model*

The reader, if he has a Malthusian model in mind, will have observed that the model presented here differs from it in various particulars. This is obvious even though Malthus' presentation in successive issues of the *Essay* [7] and in the *Encyclopedia Britannica* [8] varies sufficiently in detail and is sufficiently lacking in rigor so that it is difficult to reach an agreed-on simple statement of the model. But perhaps the following five points, either because of explicit statement by Malthus or by the requirements of the model, may be regarded as essential.[29]

1. The birth rate is not influenced by the level of income, and implicitly not by death rates. As a result, a decline in the death rate simply increases the rate of population increase.
2. Land is augmentable only at increasing cost.
3. A law of variable proportions operates.
4. Some sort of restriction on capital accumulation operates, so that capital accumulation cannot offset the limitation on quantity of land sufficiently to prevent per capita income from falling as population rises.
5. Technology is constant—or, technological progress is not related in any causal way to population growth, but if it occurs is only a coincidental development that may for a time stave off the results of diminishing returns.

The initial statement of a model associated with Figure 1 incorporates these assumptions, with the exception that a neo-Malthusian standard-of-living effect at a fairly high level of income is posited. It follows, as it does from any rigorous formulation of a Malthusian model, that if some force (not explained by the model) causes growth

[28] It is possible to assume that population growth, by "stirring up the society," induces a rise in the $v$ function and prevents this result. The assumption however seems unrealistic and uninteresting.

[29] I am indebted to H. J. Barnett for suggesting, in conversation, this summary of the Malthusian system.

in aggregate income, it can increase per capita income much above subsistence only if the rate of growth in aggregate income exceeds the maximum possible rate of population growth, or if moral self-restraint concerning procreation reduces the birth rate. The unrealistic assumptions would not be objectionable if they resulted in an analysis that satisfactorily explained reality. But the contradiction between Malthusian analysis and the facts of population history suggests that the Malthusian model is an impediment, not an aid, to our thinking about population.

Important Malthusian strains have persisted in our thinking and in university teaching. I suspect that they have persisted largely because the model predicts population growth, population growth has occurred, and we have not had a satisfactory alternative explanation. It is of course important to retain the simple notion that rise of income above subsistence tends to result in population growth. But we need to analyze that growth by tools more relevant than the Malthusian ones.

### IV. *Applications of the Model*

In conclusion, consider the application of the model presented here to the three empirical cases of population growth. An important application of the model is to the case of continuing technological progress. Previous doctrine has suggested that a rise in per capita income may itself induce population growth that will check the rise in the income level. The model presented here suggests that wherever continuing technological progress occurs, it will tend continuously to raise per capita income, and without an interval of population growth at the biological maximum rate. This implication of the model seems a reliable indicator of the course of events in the present low-income countries, if population growth is induced only by rising per capita income. The model suggests further that if technological progress is sufficient so that growth in aggregate income keeps pace with exogenous growth in population, after a period birth rates will follow death rates downward. Per capita income will thereafter steadily rise.

However, in many low-income countries, population growth is now occurring because of improved health and medical measures, and technological progress is not sufficient to prevent a fall in per capita incomes. The model suggests slightly greater hope for avoiding population catastrophe in such societies than does the conventional standard-of-living thesis, for the model suggests that the decline in the death rate may lead after a lag to a decline in birth rates, even though per capita income is falling. Even so, if the minimum lag between fall in death rates and that in birth rates is almost a generation, the increase

in population in the meantime may create serious economic, social, and political problems. Further, even after birth rates fall they may be expected to remain somewhat above death rates at any level of income above the new subsistence level. In the absence of technological progress or deliberate measures to reduce birth rates, per capita income may be expected to continue to decline. The implications of the model are more optimistic than those of the conventional model only in that it implies in some cases a longer period of grace, before per capita income falls to the subsistence level, in which to attain technological progress.[30]

Before the model can be used to forecast the trend of population in countries where birth rates have risen markedly after the second world war, it is necessary to test the consistency of the model with that development. F. W. Notestein has observed[31] that with respect to Western Europe and the English-speaking countries, the post-world war phenomenon is a dual one. With the conspicuous exception of five countries, Norway, the United States, Canada, Australia, and New Zealand, a new equilibrium in birth rates is being approached. Gross reproduction rates in the other countries involved are now tending toward a level roughly 15 per cent above the secular minimum. All but the five countries lie close to a regression line relating the ratio between average gross reproduction rates of recent postwar years and those of 1935-39 to change in the gross reproduction rate since 1950. Where the ratio is above 1.15, gross reproduction rates are falling; where below, they are rising. The five countries, however, lie far above this regression line.

[30] In Western Europe before the "vital revolution" associated with the industrial revolution occurred, birth rates were around 35 per thousand, or almost 10 per thousand below present birth rates in present peasant societies. Some observers see in this fact a cultural (or, conceivably, biogenic) difference that bodes ill for the peasant societies. The higher birth rates of present peasant societies result in a higher rate of population increase for any given level of death rates. This alone is extremely important. It is also possible that the higher birth rates may be associated with a greater resistance of birth rates to the downward pressure of decreasing death rates. On the other hand, it is likely that birth rates in Europe during the Middle Ages, for which we have qualitative historical knowledge but no data, were also above 40; and that the successive technological improvements of the Middle Ages or the sharp rise in living levels that followed the Black Death, had reduced death rates for a sufficient time to cause a fall in birth rates to about 35, long before the continuing technological improvement of the industrial revolution initiated further decline. If so, there is no reason to assume any fundamental difference in behavior between present peasant societies and Western Europe, though of course variation among individual societies is to be expected.

In some societies the mechanism by which birth rates were held to around 35 was late marriage. This is not inconsistent with the thesis just stated. Delay in marriage, which preserved an increased level of living, may have been a reaction to a reduced need for procreation.

[31] In conversation with the writer.

Even before I learned of Notestein's observation, I had assumed that because of the lag in adjustment of the relevant motivations to objective circumstances, the decline in birth rates might overshoot the equilibrium level and then reverse itself a generation later.[32] Acceptance of this "overshooting" thesis, however, leaves the cases of the five countries to be explained.

Four of these exceptional cases are among the six countries with the world's highest per capita incomes.[33] This suggests that there may be a positive correlation between per capita income and the rate of population growth which is concealed by other influences until the demographic revolution is completed.[34] However, this thesis is contradicted by intercountry comparisons for the 1920's, 1930's, and 1950's, and it seems necessary to reject it.

Alternatively, it is possible that among the satisfactions to which an increase in income from one generation to the next will be devoted is that of having a larger number of progeny. A rise in income will tend to cause a rise in the population growth rate via a declining death rate, if the death rate is in fact declining, and via a rise in the birth rate, after decline in the death rate has ceased. Under this hypothesis the population growth rate will tend to fall back to the basic level referred to above (p. 319) if the rise in income comes to an end, even though the higher level of income persists. On grounds of personality theory rather than of population data, this thesis is plausible. Casual observation suggests that it is in general agreement with historical facts. I have not attempted a more rigorous test of it.

Another possible explanation is that the recent rise in birth rates results from some immediate connection between creative energy and sexual activity, and that the sources—whatever they may be—of the creative energy which has lifted output rapidly after the second world war are also the sources of the current desire among young parents for larger families. Some psychologists studying personality believe that there is a close connection between creativity and sexual activity.[35] So far as our uncertain historical knowledge indicates, this hypothesis is consistent with the behavior of population during early periods of industrialization. Acceptance of this thesis, however, requires an explanation of why birth rates in other countries of Western Europe now

[32] This is a refinement not introduced into the model above.

[33] The other two being Great Britain and Switzerland.

[34] In research begun while each was a graduate student at the University of Chicago, G. S. Becker and R. E. Weintraub believe that they have found evidence to support this thesis. Each has presented his findings in a manuscript not yet published.

[35] According to F. W. Barron, in conversation with the writer.

economically vigorous are not higher, and the thesis can be fitted to the facts of the 1920's only with some difficulty.

Therefore, while in the writer's judgment the hypothesis concerning the effect of rise in income level has support in personality theory, it may be necessary to state simply that the rise in birth rates after the second world war constitutes a shift in the population function resulting from the successive stresses of depression, war, and postwar tensions.[36] The hypotheses that suggest themselves now concerning that shift are not inconsistent with those presented above relating to other aspects of population growth. Since this is true, it is reasonable to apply the model presented here to the high-income industrialized countries of the world as well as elsewhere.

A word about the very long run is in order. Even at a rate of growth of 1 per cent per year, the world's population would double in 70 years, quadruple in 140, reach a mass equal to that of the earth in a few thousand years, and so on. But this fact does not seem to me the cause for alarm that it appears to some observers. These observers do not seem to have taken account of the prospective trend in technology. In the countries of the world where population has grown the fastest during the past 150 years, the advance in technology has far outstripped it, and per capita income has steadily risen. There is reason to assume that this relationship between technological progress and population growth will hold true in the future for the entire world. Technological progress is now spreading to the other major countries of the world, and during the twentieth century will probably have spread to virtually all countries. There is no reason to place any given limits on the creation of resources by technology. One of the determinants of the rate of technological progress is the base of existing scientific and technical knowledge; increasingly rapid expansion of that base provides a foundation for increasingly rapid advance of technology, and without any definite limit. While population is reaching a mass equal to that of the earth, technology may progress so that subsistence and energy are obtained from the entire known universe, or several such universes, and only one man in a thousand may be living on the earth.

Of course, in the very long run human beings may be devoting less of their energies to technological progress, and it may slow down. But population functions in the far future may also be radically different from those now operating. One may well feel deep concern about the intermediate-run problem in certain low-income countries that are not experiencing technological progress, but it seems reasonable to be more concerned with any of a dozen other world problems than with that of prospective "standing room only."

[36] See n. 25, above.

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# THE SHAPE OF THE INCOME DISTRIBUTION

*By* STANLEY LEBERGOTT*

A good deal of received theory on the distribution of income rests flatly, if a bit uneasily, on the truism that "the" distribution of income is highly skewed. Since Quetelet and Galton emphasized how many human characteristics were normally distributed, the contrasting skewness of the income distribution has presented something of a problem. Yet, as Tinbergen put it, "no generally accepted interpretation of the statistical regularities seems to exist. . . . This is the more remarkable since the inequality in the income distribution is at the bottom of some of the most important problems of economic policy" [38, p. 156].

We have not, of course, lacked for explanations. Some stipulate component distributions which, joined together by one analyst, may not be torn asunder by any other. Some invoke a mysterious necessity that inheres in the Pareto coefficient. Many rely on the theory of noncompeting groups. Few explanations are without merit; few without some relevance to the way in which income is distributed in the United States today. But a closer examination suggests that most explanations fail to explain. Some are too effective. They describe with equal facility, and by identical functions and parameters, the distribution among Prussian taxpayers in 1852 and American families in 1958. Others explain only part of the distribution—the part most readily fitting a particular mathematical function. Some premise social or economic structures that differ massively from those that surround us. Others report an economy seen only by the light that never shone on land or sea.

The present paper reviews some of these theories, looks to the present facts on the distribution of income in the United States today. It concludes that the relevant U. S. income distribution shows far less skewness than we are accustomed to think, while such skewness as does exist is readily explained by the nature of our financial organization.

## I. *A Review of Past Theories*

The apparent skewness of the income distribution, as demonstrated by Ammon and Pareto, is a triumphant first fact upon which a mass of economic literature has been erected. Explanations have been generously varied.

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A. *Transformation of the Normal Curve*

In one explanation the normal curve is transformed by Edgeworth's method: "the frequency curve of a variate is . . . 'translate' from a normal curve by the aid of an arbitrary function of its argument"[1] [55, p. 6] [5, p. 364] [33]. Essentially this method was used by H. L. Moore, R. Gibrat, D. G. Champernowne, M. Kalecki and others who add increments of income to an initial income distribution in proportion either (a) to the levels already achieved, or (b) to the excess in ability of given members of the distribution over the lowest (or median) member[2] [29, p. 89] [7] [8] [21] [39]. The difficulty with many such functions is a simple one: they do not fit the data. The original Pareto function described only a portion of the reported income distribution—a point initially recognized by Pareto but apparently later subject to underemphasis. Neither does the Gibrat formula, even as modified by Kalecki or Champernowne, fit the entire distribution at all well[3] [21, p. 169] [7, pp. 614-15].

What is particularly incomprehensible is the willingness of some analysts to deal with only whatever part of the income distribution fits the function—and then deduce serious economic consequences and deep social necessities. (The "virtual identity" of Pareto coefficients in many societies, to which many since Pareto have called attention, is a charming artistic observation, not a scientific one. The coefficient is capable of such limited variation that it would not appear much different in the ocular analysis of data for a Fourierist society, a communist or a capitalist one.) It is of more than passing interest that both a leading mathematician (W. Feller) and a distinguished economist (Pigou) have independently picked the same adjective when they describe Pareto's function, or the neccessity for it: "mysterious[4] [11, p. 418] [32, p. 650].

There exists, however, one method of "translating" the normal curve to derive a skewed distribution, and fit it comprehensively, that rests on a sounder and more penetrating economic analysis. It emphasizes the ownership of capital as the deciding element. Thus, Kapteyn states that

[1] Wicksell refers to the distribution of logarithms of variates developed by Fechner—presumably prior to Pareto's particular use.

[2] Cf. the work of Van der Vijk as noted in [10] and [39].

[3] Champernowne [7] develops formulae that fit better than Gibrat's [16], but if we apply them to the only set of data he presents that include low-income recipients—for the United States in 1947—his results are hardly satisfying. Thus even his 5-parameter formula gives estimates in error by 13 per cent or more for 4 of the 12 income intervals.

[4] Pigou refers scathingly to Pareto's distribution function "for the existence of which a mysterious necessity seems to have been discovered."

one of the principal causes of deviation in wealth is to be found in gains and losses of capital, these being "roughly proportional to the capital [already] possessed"[5] [23, p. 43]. This assertion, of course, forces the inquiry back one stage: how *did* the variations in capital themselves come about? The solution presumably is found in a factor emphasized by Pigou and Herbert Hoover—inheritance: the capital accumulation of many generations provides the basis for substantial current streams of property income[6] [32, p. 65] [19, p. 29]. These explanations are closely related to those of H. Bernadelli, R. S. G. Rutherford, and a brilliant unpublished study by Robert Solow—each of whom relates income in this year to income in the prior year[7] [2] [33] [34].

Now surely the biblical apothegm—to him who hath shall be given—is uncontestable, even when veiled in mathematical and analytic form. For despite the most random and extreme movements from year to year, persons do tend to maintain the same position within the income distribution. If we merely stop with last year's income, however, we are left with the question of how that pattern arose. If we continue the regress until we explain the income in a given year by the role of inheritance, what do we have? Certainly an explanation that might apply to some countries in the historic past.

But is it of substantive use for understanding how income is distributed in the United States? Suppose we take as an indirect indication of the power of inheritance in molding our income distribution the data we possess on the occupational ladder. Whether we look at the tremendous occupational shifts from immigrant parents to children of immigrants or to spot studies of such shifts in earlier decades, it is hard to develop a great role for inheritance in the past.[8] We have more recent evidence, though still very partial. It suggests that many in the upper occupational groups—where we would expect inherited property to be of most consequence—come from parents in the semiskilled, clerical and labor

[5] Kapteyn rejects Pearson's rather tautological analysis of skewness as originating "when the tendency to deviation of one side of the mean is unequal to the tendency to deviation on the other side" [23, p. 5]. He emphasizes instead that "causes independent of the size *of the individual* produce the normal curves, [while] causes dependent on this size produce skew curves" [23, p. 13].

[6] Herbert Hoover [19, p. 29] recommended inheritance taxes after the first world war "to thaw out frozen and inactive capital and the inherited control of the tools of production."

[7] Solow points out that in principle there is no reason why we need allow only for the income of the prior year. His model, however, and his empirical findings suggest that such other elements are unessential variates.

[8] Data on occupational shifts appear in E. P. Hutchinson [20]. The study by Taussig and Joslyn [37] is well known, while David A. Wells [54, p. 351-53] has some interesting data on the proposition of shirt sleeves to shirt sleeves in three generations.

groups.[9] And the income tax data for the nation as a whole show that current earnings play a dominant role even at high-income levels.[10]

## B. *Two Overlapping Distributions*

An alternative approach contends that two independent factors or distributions are involved, each perhaps normal, but so overlapping that they create the characteristic skewed income distribution. In its simplest form, as in Boissevain's two-factor analysis, this analysis falls into difficulties because it must presume—without any particular warrant—that the factors are quite uncorrelated. Waxweiler [53], for example, proposes a completely independent factor on the supply side and one on the demand side. Unless we posit zero correlation, we are up against the difficulty that the joint distribution of two normally distributed variates will itself be normal. A distribution of intelligence quotients, for example, is normal even though it reflects half a dozen separate achievement distributions for different components of the standard I. Q. tests. Yet what warrant do we have for positing zero correlation to begin with? It is not particularly reasonable as an hypothesis. It is not especially heuristic as a premise.

Tinbergen, too, has distinguished two distributions. He points out that we are concerned not merely with the distribution of abilities to earn income but likewise with a distribution of preferences for income: "the valuations for the labour to be performed, on the one hand, and for the wages to be received, on the other hand."[11] He assumes the latter to be unequal, since "a person who has special ability for a job which requires an effort *s* will be prepared to accept a relatively lower wage than somebody who does not possess this ability" [39, pp. 199-200]. Let us concede that the resistless instinct of workmanship does persuade some expert members of the labor force to accept only average (or below-average) earnings rates. Let us go on to stipulate that it

[9] Cf. the occupational data in the six-city mobility study as reported in Herman Miller [27, p. 32].

[10] [51, Part I, Table 3] [49, Table 7]. From these sources it can be seen that if upper-income recipients with earnings *and* with other types of income earn no more than the average amount earned by others at the same income level, the residual contribution of their nonearned income to total income is relatively small. This residual must be increased somewhat to reflect the fact that in some instances earnings from self-employment may have been made possible in the first place only by inheritance of the business, or capital to buy the business. Yet even after making an allowance for such a factor, we would be hard put to maintain that inheritance is now a major factor in shaping the U.S. income distribution.

[11] [39, pp. 207, 198]. Tinbergen refers to the work of J. Van der Wijk, who develops a normal distribution not for income but "the logarithm of income less a certain minimum" —a procedure similar to that of H. L. Moore, Harold Davis, etc.

could persuade still others to do so if necessary to keep their skills burnished bright. But the evidence of today's market suggests that few persons do make such concessions: superior skills generally command superior rates. Where they fail to, it is not because their possessors willingly accept lower incomes [24, p. 79 and *passim*].[12]

As there is a taste for work, so there is a taste for risk—and this too has been invoked to explain the shape of the income distribution. This explanation starts from the common dichotomy between "those who prefer a secure though modest return . . . and those who play for big stakes and are willing to assume risk in proportion" [31, p. 289][13] As developed by Friedman the theory does not presume two groups of individuals but rather two sets of actions by every individual, one set having "results which are not accessible to redistribution," while the results of the other are [12, p. 288] [cf. 13] [cf. 2, p. 359]. Since the former set of actions involves participation in a lottery, no (sizable) negative income arises for any participant; the "fraction of winners is close to zero," but nevertheless positive; and a group of very high incomes appears. Such a process could readily account for the characteristic tail at the upper end of the income distribution [12, p. 289]. This well-developed and ingenious model is one of the very few that offers an explanation for the lack of a tail at the lower end of the income distribution. And it sorts out the key elements that must be included in a really useful analytic model.

But it does not necessarily bear closely on the actual distribution of income in the United States today. The person in our society with the greatest taste for risks is of course not the entrepreneur or millionaire but the small-income wage earner. It is the wage earner, and not the entrepreneur, who regularly bets against impossible odds—on policy lotteries, sweepstakes and football pools. Atkinson's fascinating data suggest that it is the small investor (more often than the wealthier) whose portfolio is dominated by low-rated, speculative stocks [1, pp. 125-26] [36]. Above all "a taste for risk" more obviously motivates the millions of wage earners who, in any year, throw up their jobs and uproot their families in the venturesome hope of bettering their incomes, than it does the prudent investor. For the latter hazards only a portion of his capital (after consulting his lawyer, broker and accountant), frequently seeking investments that have been guaranteed by a government agency, and relying on the consolation offered by government tax offset if the worst happens. Yet it is such investors with a taste for caution and

[12] More than one firm has adopted high-wage policies as a method of securing top-quality workers.

[13] Perlman refers the distinction back to Sombart's contrast between the guild aim of "a secure livelihood" and the businessman's "boundless desire to amass wealth" [31, p. 6].

*the sure thing, who dominate the upper end of the income distribution.*

*However, most of these considerations are irrelevant:* since neither the income receipts of the risk-taking wage earner nor those of the entrepreneur are "accessible to redistribution," the premises of the model are not met so far as concerns the present-day distribution of income in the United States.[14] Fruitful conclusions may yet flow from such premises. But there is, as yet, no substantial store of such conclusions.

It is on a related difficulty that Wicksell's (and latterly, Hayakawa's) analysis fails. Wicksell emphasized that earnings from business are "the sums of the profits in an (often great) number of transactions . . . the greater the risks . . . the greater eventual losses or returns" [55, p. 12] [18, p. 181].[15] Such theories could account for a symmetrical distribution, with entrepreneurs appearing in equal numbers at the lower end of the income scale and at the upper. They do not explain the actual one, with its positive skewness.

C. *Noncompeting Groups*

Of all the explanations of income inequality, the chief is probably the lack of mobility between social strata: the theory of noncompeting groups.[16] There exists, in Cairnes words, "a series of industrial layers . . . the several strata are, for all purposes of effective competition, practically isolated from each other" [6, p. 72].

What is most striking about this explanation is how distant its origins are. Can Adam Smith's bitter comments on social policy in the eighteenth century, when poor laws shackled the worker who sought to leave one shire for better wages in another, when free public education was nonexistent and when the secret ballot, trade unions and Chartist proposals were idle dreams—can such an analysis be used to explain income distribution in a society where public education is all but universal; where a host of incompletely differentiated occupations compose the labor force; and where substantial geographical mobility characterizes the labor market? How relevant to today's income distribution are Mill's once trenchant references to "hereditary distinctions of caste; each employment being chiefly recruited from the children of those

[14] At least in terms of this analysis; redistribution via the tax mechanism and government subsidy programs is another matter. The tax redistribution process helps destroy, not produce, incomes at the upper end of the distribution.

[15] Hayakawa [18] finds that the skewness derives from the sum of separate distributions for labor income, salary income, etc. For entrepreneurial income he specified that "positive skewness will be great"—but does not touch on the critical point of why it should exist, or why it should be great.

[16] J. E. Cairnes [6, Pt. I, Ch. 3]. Cf. the position of Benini and Gini as summarized in Hans Staehle's admirable study [35, p. 79] and Staehle's own exposition [35, p. 87]. Pareto's discussion of the conditions that preclude entry to *cadres* and classes in his later work [30, p. 2046] might be noted here.

already employed in it," with "the great body of labouring people" excluded from the most remunerative pursuits because they could acquire neither education nor training? [26, Bk. II, p. 459, and Bk. III, p. 544]. There are, unquestionably, monopoly elements which preclude free entrance into certain occupations.[17] The selection of talent from the population at large is limited by differential access to education, to training, to financing. Kind words do not an open society make. But for many decades the "career open to talents" has characterized U. S. society far better than the theory of noncompeting groups has characterized it, or than the latter characterized the English social order in which it was developed.

To pin such qualitative statements down a bit, let us attempt a crude numerical guess. We can assume that in eighteenth-century England the number of job changes during a year would have been extremely few—possibly less than 10 per cent. In contemporary America we can estimate that the ratio is well over 200 per cent. In 1954, for example, an average of 68 million persons were in the labor force at any one time. Yet roughly 100 million entrances into and exits from the labor force and shifts between farm and nonfarm jobs took place in that year; while we must add perhaps 70 million more job-changes within nonfarm or farm employments.[18]

Given such an enormous volume of labor market choices, entrances, exits, and shifts, our initial expectation would be that here, if anywhere, the classic conditions for producing a normal distribution must exist. Islands of noncompeting, of monopolistic and protected groups will certainly appear. Yet the broad pattern should be much like a normal distribution. A priori explanations in terms of noncompeting groups

[17] Walter Gellhorn [15, pp. 105-51] notes that "By 1952 more than 80 separate occupations, exclusive of owner businesses like restaurants and taxicab companies, had been licensed by state law." But however serious the principle involved, the proportion of the labor force concerned is clearly small.

[18] The annual average appears in [43, pp. 19, 35]. The shifts into and out of the labor force are shown in [44, Tables 17, 18, 19]. Estimates of shifts between jobs not already covered in these data were made by developing shift rates from two sets of data. For construction and trade, rates for 1949-52 were used as shown in a paper prepared by David Kaplan (based on Current Population Survey data) [22]. For all other employment, except manufacturing (i.e., primarily service and transport) the trade rate was used. For manufacturing employment OASI records on the percentage of covered workers changing industry code group from one quarter to the next were assumed to be more reliable. Rates were derived from these data by weighting rates for the separate age groups as reported in Isadore Blumen *et al.* [4, pp. 39-41, 67-73]. The estimate made here is a conservative one, since no allowance was made for job changes within agriculture not already accounted for by the labor force change data. (Since the number of persons with work experience in 1952 and 1954 was much the same, it was assumed that the gross change data for 1952, the last year published, applied equally well to 1954. The 120 million figure thus estimated was arbitrarily reduced to 100 million to compensate for the response variation present in these data.)

can hardly carry the conviction that they did when knowledge of the actual magnitudes of change in the U. S. labor market was far dimmer.

## II. *The Ability of Individuals to Earn Income*

When so much expert analysis fails to give a reasonably coherent and applicable theory of why the income distribution in the United States today is as skewed as it is, we are well advised to return to first principles. What is it that we are seeking to measure when we deal with the income distribution? And how best shall we measure it?

We are concerned here with income as a measure of productivity rather than of welfare, hence with the ability of individuals to earn income under existing social and economic conditions. Conditions on the supply side have developed their talents for work, their abilities to invest, and have fired their desire for material goods. On the demand side still other factors have determined what places are available in the labor force, what skills and investments are requisite. Out of the interactions in the market come the rates of pay offered to ability and capital.

Given these market conditions, however, there can be no better measure of the distribution of the ability of individuals to earn income than the tautological one—namely the income they earn. But once the issue has been put in so flat-footed a fashion, it is clear that many of the income distributions used in developing the theories mentioned above are irrelevant.

1. *Tax Returns.* The distribution of persons filing tax returns has frequently been used, *faute de mieux,* for studying the distribution of income. The two are related, but only in a coarse and inconclusive fashion. For the tax population has no boundaries other than those set by the multitudinous provisions of this year's tax law and the vigor of this year's tax administrator. As the law is amended to cover more and more of the total population, the distribution inevitably changes. Even for the same year can we reasonably compare results for Norway (where tax returns cover 64 per cent of the population) with the Netherlands (where they cover 97 per cent) [40, p. 3]? And if we cannot, then how relevant is comparison of distributions and Pareto coefficients for the income tax populations of different countries at different times?[19] The scope of the income tax population is too narrow and arbitrary to carry us far if we seek to understand and measure the distribution of abilities to acquire income.

2. *Families.* The income distribution of families, or families and

[19] Pareto, of course, had no difficulty in doing so, even when the income tax population included corporate entities as well as persons.

single individuals, is a useful welfare indicator but it does not measure the ability of individuals to earn income. For that matter, it does not even measure the ability of families to earn income, since a family's income reflects not merely its ability to acquire income but the nature of its financial goals. Thus with fixed or slow-moving financial goals, the wives in many nonwhite families stopped working when the onset of the prosperity of the second world war increased their husbands' income [17]. But surely family earning-ability did not decrease. With a backward-sloping labor supply function growing incomes would at some point actually produce greater and greater income "inequality."

3. *All Earners*. The distribution of all income earners is no more satisfactory. Its chief limitation is that it includes both men and women. To exclude women from the income distribution studied would clearly have a major impact. But this must be done because a large proportion of all women with income do not attempt to utilize their full ability to earn income. Some women work only part of the year. Others work a full year but only part time. And many in either group do so because—given family obligations, their attitude toward work, the income goal they seek, etc.—they voluntarily limit their participation in the labor force. Under such circumstances what do we do if we include them in the income distribution being considered as a measure of abilities? Roughly the same thing as if we were to study the skewness in a distribution of intelligence quotients that combined scores achieved by persons who took only part of the examination together with scores for those who took the entire examination.[20] Fifty years of social legislation can change the Pareto coefficient less than an increase in Saturday jobs for women—if we are concentrating on the distribution of all earners or income recipients. (Possibly a meaningful distribution function could be developed for incomes of females who seek to work a full year but that is a separable problem, and a very complex one.)[21]

If we restrict our attention to the data for males, however, the same train of reasoning leads us to rule out two groups because they too do not attempt to exercise their full income-earning abilities. One group includes the youngsters, who attend school and at most work part time. The other is made up of older persons, many of whom are semiretired or completely retired. Perhaps the clearest demonstration that these groups must be excluded lies in a comparison of the trend in the earn-

[20] George Garvy [14, p. 40] has made the point very specifically. "The economic and social significance of part period workers is different from that of part time workers. Obviously $500 earned by a young man during the balance of the year on his first full time job after . . . graduation" differs from the same amount obtained in part-year employment of a family head who was in the labor force the entire year.

[21] The problem is analogous to that involved in developing tables of the work life for females.

ings and employment of males in these two age groups with that for the prime 35-44 group (Table 1):

TABLE 1—TRENDS IN EARNINGS AND EMPLOYMENT OF MALES, BY AGE

| Age | Median Money Income of Males with Income | | | Per Cent of Males in Each Age Group Working a Full Year | | |
|---|---|---|---|---|---|---|
| | 1945 | 1956 | Increase 1945–56 | 1939 | 1956 | Change 1939–56 |
| 35–44 | $2,473 | $4,575 | +$2,102 | 56 | 78 | +22 |
| 14–19 | 389 | 412 | + 23 | 9 | 6 | − 3 |
| 65 and over | 1,225 | 1,421 | + 194 | 27 | 22 | − 5 |

*Source:* Income data from Bur. Census, Current Population Reports, Ser. P-60, No. 2, Mar. 1948, Table 15, and No. 27, Apr. 1958, Table 18. Work experience data from Bur. Census, *1940 Census, Employment and Personal Characteristics,* Tables 1, 11, 31 and 33; and Current Population Reports, Ser. P-50, No. 77, Nov. 1957, Table B.

In a period when massive gains took place in wage rates and employment opportunities, full-time work by men in the prime 35-44 group rose greatly; their median income rose by roughly $2,100. Full-time work by youngsters and older workers, however, actually decreased; their gain in earnings consequently was no more than the mere rise in wage rates would have brought. Such relative stability for these two groups reflects one basic fact: their annual income measures primarily a level of incidental earnings, and not that of attempted full-year income.[22]

A third age group must likewise be excluded from the data for recent years—men 20-24. The reason for so doing is simply that a substantial number of these men serve in the armed forces during part (or all) of the year. Their annual income fails to measure what their income earning abilities would yield when not arbitrarily limited by government action.[23]

Of course if we could include in the income distribution being studied only the members of these age groups that did seek a full year's work (including the youngsters who enter the labor market early and untrained, the older workers who linger on unwanted for full-time work

[22] In studies by Dorfman and others, older workers have reported not working because they were "unable to work." One might, therefore, treat them as the institutional population is treated below. The meaning of such declarations, however, is as yet too obscure to warrant such classification. Some studies suggest that some older workers report "unable to work," meaning thereby "unable to secure work at wages and at skill levels to which they had been accustomed."

[23] Were recruiting from the civilian population a random matter, this contention would not apply, although there would still be a problem of part-year income receipt.

and who are paid at the lowest rates) a more realistic distribution would be developed. Should the data permit us to do so, however, it is certain that many of those to be included would not be at the lower end of the distribution. Moreover the entire adjustment could have only marginal impact: most persons in these age groups would continue to be omitted.

The principle on which we exclude such groups, however, reaches only to invariant characteristics such as age or sex. Some analysts, in an endeavor to develop a symmetrical distribution, have excluded part-year workers per se, the unemployed, those with zero income, those in certain occupations. There is little basis for such exclusions. For all we know about such groups is that they have not worked a full year or earned a sizable income. Neither fact demonstrates anything more than this—that they have brought to the market the least desired set of talents, and received the least employment and monetary reward in consequence. (We are not concerned here with native talent, use value, or wealth and illth: as a hack writer Oliver Goldsmith was too unreliable, as a bureaucrat Whistler was too volatile to enter the income distribution at a point where future generations might choose to put them.) The facts of low income and a short work-year per se give us no warrant for excluding any group from the income distribution. By the same token, what virtue lies in excluding a given occupational group to develop a distribution of Elysian symmetry?[24] For one of the basic mechanisms by which the economy rewards different earners differently is to allocate them to occupations, to industries, and to areas with characteristically different levels of income receipt.

One puzzling group is the institutional population. Though customarily excluded from income distributions, we cannot omit them here. Since society pays for their maintenance, we may most realistically define their income as though it were negative—since it is as clearly a withdrawal from other incomes (or capital) as are the losses of entrepreneurs.[25] Therefore, instead of classifying them with the zero-income group—a reasonable alternative—their limitations of physical and mental qualities seem best reflected by classifying them as having negative incomes.[26]

[24] For a convenient and lucid presentation of the data by employment status and occupation, see Herman Miller [27, esp. Ch. 3] [28]. For an effective review of the wage rate data in one area see Frederic Meyers [25].

[25] Ammon implied this classification for "beggars, inmates of institutions" and those "who generally do not receive any income of their own, and yet are being maintained" [35, p. 77].

[26] The 1950 Census report [42, Table 3] gives us the basis for estimating the number of males in the 25-64 age group. This total was raised to 1954 levels, but reduced to exclude persons in correctional institutions [42, Table 4] on the analogy of the armed forces, their

## III. *Incomes of Males Aged 25-64*

Suppose, then, in our attempt to measure the ability of individuals to earn income under existing social and economic conditions we pass on from income distributions for taxpayers, families, individuals, to that for all males, and thence to the male distribution exclusive of those age groups in which the typical person does not attempt to earn a full year's

TABLE 2—MONEY-INCOME DISTRIBUTION, CIVILIAN MALES, 1951
(Per cent distribution)

| | All Males | Males Age 25–64 | |
|---|---|---|---|
| Under $1,000 | 17.5 | 8.7 | 8.4 |
| $1,000–1,999 | 13.9 | 11.8 | 11.4 |
| 2,000–2,999 | 19.4 | 20.9 | 20.2 |
| 3,000–3,999 | 22.6 | 26.3 | 25.4 |
| 4,000–4,999 | 12.7 | 15.5 | 15.0 |
| 5,000–5,999 | 6.4 | 7.8 | 7.5 |
| 6,000–9,999 | 5.4 | 6.9 | 6.7 |
| 10,000 and over | 1.9 | 2.1 | 2.0 |
| Total with income | 100.0 | 100.0 | 96.6 |
| Without income | | | 1.9 |
| Institutional | | | 1.5 |
| Grand Total (civilian) | | | 100.0 |

*Source:* Bur. Census, Current Population Reports, Ser. P-60, No. 11, May 1953, Table 3; and 1950 Census, *Institutional Population*, Table 3.

income. We are left with the distribution of Figure 1 and Table 2 for the 37 million males aged 25-64.[27] This distribution covers a large, endlessly varied set of persons—the fully employed and those who secure

income, too, being limited by society. One could, of course, make an argument to the effect that the very fact of their being in correctional institutions was a reflection of their inability to earn income under current social arrangements. However, the empirical effect on the income distribution of either choice is minute.

[27] Data on 1951 incomes from Bureau of the Census [48, Table 3]. These and other survey data have certain biases for which extensive adjustment has been made by the Office of Business Economics [52, pp. 27ff.]. (A comparison between family income distributions in Table 1 of the parallel Census report for 1950 [47] and the adjusted OBE family distribution in Table 19 of the latter study indicates that the major adjustments are in the under-$1,000 income group. These adjustments were made on an over-all basis. One cannot therefore demonstrate, but only assume, that the disproportionate dominance in the low-income group of under-20 and over-65 persons would have accounted disproprotionately for the bias in that group. Hence, the restriction of present data to the 25-64 age group would have removed a major share of the differential bias present in unadjusted data.)
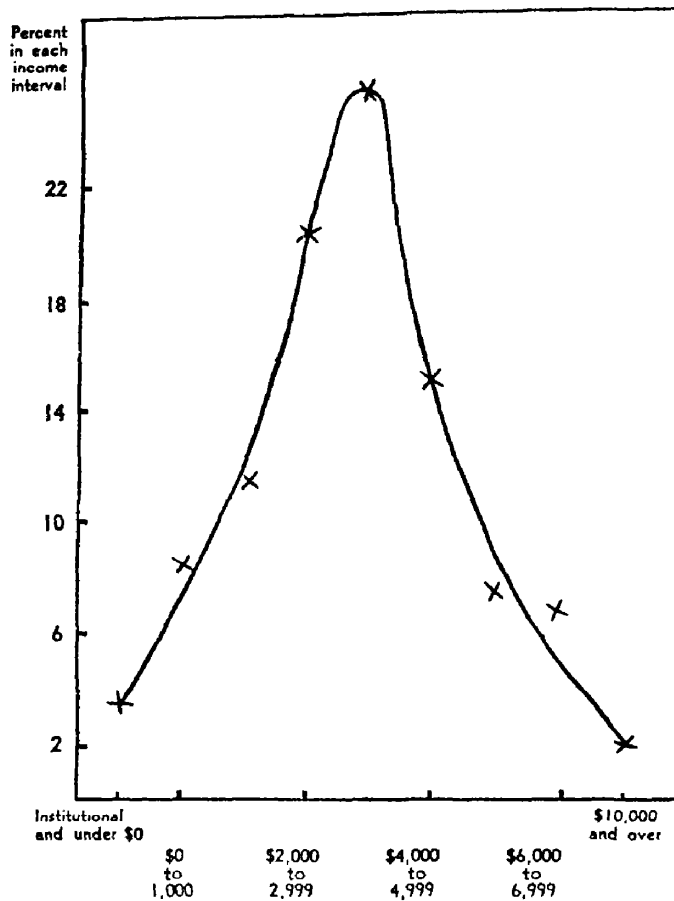
FIGURE 1. MONEY INCOME IN 1951: MALES 25-64.

only part-year work; the quick and the halt; the brilliant and the slow; those with wealth beyond the dreams of avarice and those who live on society's bounty. Nevertheless, the joint distribution of all these persons is clearly much more symmetrical than the typical Pareto curve. One hardly has to invoke the shades of noncompeting groups, inheritance, preference for risk, etc. to explain this pattern. A log-normal function is not so obviously apposite. On the other hand, distinct skewness still appears. Can we explain it?

## IV. *Credit Rationing and Labor Demand*

In Table 3 there appear estimates for two major groups that compose the 25-64 distribution—those who are self-employed, and those who are not. Since the time of Solon (and certainly since that of Wicksell) it has been clear that the self-employed contribute disproportionately to the skewness of the income distribution. These data reaffirm this gen-

TABLE 3—MONEY INCOME DISTRIBUTION OF MALES, AGE 25–64 in [illegible]
(Percent distribution by class of worker)

| | Total | Employees | Self-Employed |
|---|---|---|---|
| Under 1,000 {Loss | {0.7 | 0.2 | 1.8 |
| {1–999 | {7.7 | 5.0 | 13.9 |
| 1–1,999 | 11.4 | 11.9 | 10.8 |
| 2–2,999 | 20.2 | 23.1 | 14.5 |
| 3–3,999 | 25.4 | 29.8 | 16.5 |
| 4–4,999 | 15.0 | 16.4 | 12.4 |
| 5–6,999 | 7.5 | 6.7 | 9.5 |
| 7–9,999 | 6.7 | 3.2 | 14.7 |
| 10,000 and over | 2.0 | .3 | 5.8 |
| Total with income | 96.6 | 96.6 | 100.0 |
| Without income | 1.9 | 1.9 | — |
| Institutional | 1.5 | 1.5 | — |
| Grand total | 100.0 | 100.0 | 100.0 |

*Source:* Bur. Census, unpublished data. The Census data relate not to class-of-worker but to occupational group. Farmers, professionals, managers and proprietors were here combined as an approximation of the self-employed group. The resultant inclusion of wage earners is probably most important for the $1,000–$3,000 income level but would affect the contrast of the two distributions very little.

erally accepted point. But if we examine them closely, they do more.

The wage-earner group itself is merely composed of those persons who were wage earners as of the week of enumeration. During the year an average of 400,000 wage earners a month entered self-employment and almost as many self-employed persons entered the wage-earner group.[28] The presence of this component may have been a factor in shaping the distribution for employees, but when we turn to the distribution for the self-employed it is clear that it is this latter group that is the source of asymmetry. Customary explanations of the skewness in the income distribution concentrate on explaining the presence of the tail at the upper portion of the distribution. But it is clear that the self-employed largely account for this portion. Since the self-employed can lose as well as make money, however, why should not there also be a lower tail of equal length, also produced by the presence of the self-employed?

Let us consider a simple model of the supply and demand for labor

[28] Estimate based on unpublished data from the Census Bureau's Current Population Survey. These gross-change figures, as others, are peculiarly subject to response-variation. However, for present purposes it makes little difference whether the true figure is markedly smaller.
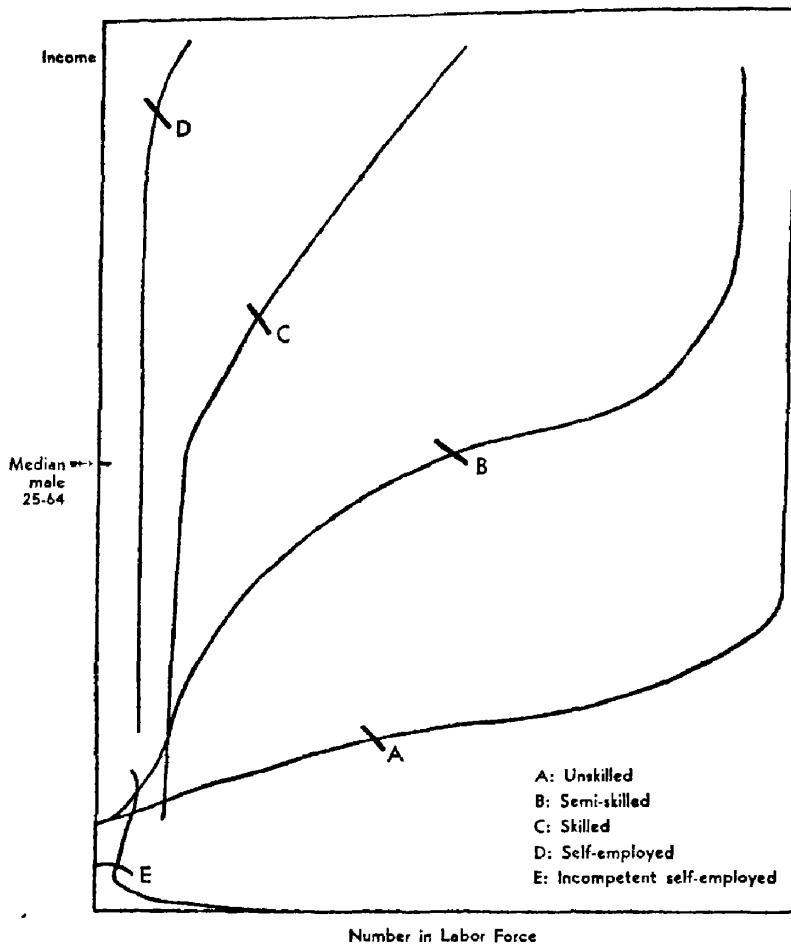
FIGURE 2. SUPPLY AND DEMAND SCHEDULES BY OCCUPATION GROUP

in different occupations as a means to answering this question. Figure 2 portrays supply schedules for selected occupational groups—together with the portions of the relevant demand schedules where they intersect the appropriate supply schedules. The income of the median male is arbitrarily set to equal that of the occupation group in which a majority of males are engaged. (In fact the median for male semiskilled employees in the United States is much the same as that for full-time males in the labor force.) Averages for the other occupation groups are indicated. These then translate into a symmetrical distribution if the rates for one group are smoothed into those for the other. This smoothing occurs in practice for two reasons. First, in reality not 5 groups but hundreds (possibly thousands) of clearly differentiated occupations, with their several wage rates, exist. Second, the market even for one of these occupations is hardly perfect. Ignorance of prevailing rates exists

among employers as well as employees, the momentary exigencies of supply and demand in the process of reaching toward a never-achieved goal of full equilibrium result in a stochastic process, producing a spectrum of rates paid in even the most specific, keenly differentiated occupation. The conversion of rates into income is in turn affected by illness, personal preference, irregularities in the production process and so on.

That the numbers and prices that clear the markets translate into a smooth distribution can therefore be seen as reasonable. But how does this symmetrical distribution relate to the actual one? Here one may refer to a key dialogue between Holmes and the Inspector:

> "Is there any point to which you would wish to call my attention?
> "To the curious incident of the dog in the night-time.
> "The dog did nothing in the night-time.
> "That was the curious incident, remarked Sherlock Holmes."

The most noteworthy aspect of the usual income distribution is not that it has one long tail—but that it does not have two. The mere presence of one tail is no more sufficient to produce asymmetry than is the presence of one leg sufficient to make a man one-legged. The symmetry that derives from the schedules in Figure 2 comes about because of the presence of occupation group E. This group is composed of the incompetent self-employed—those capable of losing large sums of money as entrepreneurs. A rare combination of persuasiveness, avarice, and recklessness is needed. How widespread is it? Perhaps an indication appears in the income tax figures for 1956, which report more than 3 million persons with incomes over $10,000—but less than 30,000 with an adjusted gross loss from business of over $10,000 [51, Tables 1, 6]. But surely there must be more than 1 in every 100 would-be entrepreneurs sufficiently incompetent to lose $10,000. If so, and if there is no reason to assume (as is frequently done) that the income distribution must begin at $0, why does the real distribution lack a significant number with negative incomes? The answer of course is that, while there may be a great potential supply, the demand is almost minute. And the key lies in our credit system.

Who can be a Samuel Insull, Ivar Kreuger or John Law on his own savings? As it generally requires credit to acquire substantial incomes so it requires credit to achieve spectacular bankruptcies. By extending credit to those with great ability to make money the credit agencies help produce one end of the income distribution. By refusing credit to those with great abilities to lose money, they truncate the other end.

## V. *Some Conclusions*

Incomes that are relatively high compared to typical incomes appear in every country in the world. Even in socialist societies, theory has

aught up with practice in arguing the necessity for distinctions in earn-ng rates in order to encourage workers to train for the more skilled oc-cupations and to reward higher productivity.[29] It is not so much the higher incomes which require explanation, therefore, as the lower ones —in this case the negative ones.

The range from median to top income will vary from sheikdom to sheikdom, from capitalist to communist state. But all appear to have high incomes relative to the median while none have any considerable number of negative incomes. Credit availability plays a sufficient part in facilitating the receipt of high incomes in some countries, a neces-sary part in others—but in all countries with private credit systems credit rationing appears to be a major factor in preventing large nega-ive incomes. As such it has a decisive role in producing the skewness in he income distribution.

What, in summary, has the preceding review of income distribution heories covered? It has not attempted to touch on the welfare aspects of income distribution. Income may be normally distributed and yet a given society may decide that all incomes are "too low" because produc-ive potentials have not been fully tapped, or that incomes are "unfairly listributed" because a tax-subsidy program could make many persons "better off" and only a few "worse off." On such problems welfare conomics may yet be able to say a good deal but this is not our present oncern.

The present subject has been whether income, as a measure of the roductivity of income recipients, is normally distributed in the United tates today. In examining this question we have concluded that the ustomary conclusion (or premise?) that this income distribution is ighly skewed tends to derive from the study of nonrelevant distribu-ions—such as those for taxpayers, for families, for all individuals. We ave contended that a relevant (and most available) distribution for is purpose is that for males 25-64—and that such a distribution is emarkably like the normal gaussian, remarkably unlike the usual kewed distribution. In explaining the skewness that does nonetheless ppear in some measure in that distribution it has been suggested that e are really confronted by the impact of credit rationing in truncat-ng a normal distribution. By and large, credit agencies succeed in enying credit to persons capable of losing large sums of money (and ius contributing a long lower tail to the distribution) while affording edit to those who use such leverage to acquire large income.

Few theories that have been developed fail to help in understanding

[29] It is interesting to find as presumably accepted doctrine a statement by Oscar Lange dicating "la nécessité d'une differentiation des salaires, en vue de stimuler la productivité travail," quoted in [3, p. 793].

how income is in fact distributed. Inheritance may play a greater role in other societies, but it is certainly a factor in our own. Noncompeting groups and the ability of those with finer educations and preferred social origins to acquire larger incomes are factors of real force in all societies. The taste for risk, a willingness to grasp the hazard of new fortunes is a further element, certainly in the United States. But if we seek to understand the shape of the income distribution relevant to the measurement of productivity (and not of welfare) it would appear unnecessary to invoke these other elements. The shape is reasonably well defined as a normal distribution, truncated by credit rationing.

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# THE ACCELERATION PRINCIPLE: DEPARTMENT STORE INVENTORIES, 1920-1956

*By* NEWTON Y. ROBINSON*

The primary purpose of this investigation was a partial testing of an adaptation of the multiplier-accelerator type of theory[1] to an explanation of the minor business cycles which are frequently considered to be "inventory cycles." The basic assumptions of this adapted theory are (1) that changes in the level of income during the periods immediately preceding period *n* will, on the average, lead to a substantial amount of induced investment[2] during period *n*, and (2) that consumption during period *n* is largely the result of income in several previous periods. This study represents only a partial testing because it does not test the relationship between consumption and income and because it measures the amount of induced inventory investment in only one segment of the economy.

## I. *Method*

At the time this study was begun, the amount of monthly inventory data available was severely limited, a condition which is gradually being corrected. This project, consequently, was restricted to an intensive study of the substantial body of excellent seasonally adjusted monthly data on department store stocks and sales.[3] The statistical method used

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[1] The theory being tested is essentially an adaptation of Hicks' general case. See [1, pp. 74-81]. The principal adaptation consists of using a short fixed period (one month) in order to reveal inventory fluctuations more completely.

[2] The exact amount necessary for an expanding cycle will depend on the consumption function and the lag pattern of induced investment. Hicks argues that the total investment coefficient (i.e., the ratio of induced investment to change in income) must at least be greater than 1 [1, p. 78].

[3] The data used were slightly more precise than those published in the *Federal Reserve Bulletin*. They were supplied through the courtesy of O. K. Thompson of the Division of Research and Statistics of the Board of Governors of the Federal Reserve System.

was that of multiple regression. The regression of change in inventories for period $n$ on changes in sales in several antecedent periods was calculated by the method of least squares.

It is possible that department store officials are more affected by "current dollar" figures than the "real" quantities underlying them. Use of undeflated data, however, could yield misleading results. If, for example, there were no change in real stocks or real sales but price changes did occur, an observed correlation might be found in the undeflated data which was due solely to the fact that changes in prices caused simultaneous changes in the current dollar figures for both stocks and sales. To eliminate this possible source of bias, deflators were constructed from two components of the Bureau of Labor Statistics Consumer Price Index: apparel prices, given a weight of 2; and housefurnishing prices, given a weight of 1.

To prevent bias due to the fact that both department store sales and inventories have an upward trend, changes in deflated sales and inventories were respectively expressed as percentages of a twelve-months moving average of deflated sales and inventories. The moving average computed for any given month was the average of the month in question and the 11 immediately preceding months.

The use of percentage differences also guards against another important possible source of misleading results. For example, inventory investment during any given month will be affected by sales in each of several preceding months. If sales for period $n$-1 are included as one independent variable and sales for period $n$-2 are also included as another independent variable, a quite high correlation would exist between these two independent variables. Where high correlations exist between independent variables, the system becomes unstable (i.e., small changes in the input data cause large changes in the regression coefficients). Use of percentage changes in sales, rather than sales themselves, substantially reduced the correlation between those independent variables, resulting in a stable system. Evidence of the stability of the system was obtained from a study of the inverted correlation matrices. Since none of the elements of the inverse were unduly large, the system was stable.[4]

Another statistical technique which should be explained is the use of absolute values (i. e., numerical values without regard to sign) of per cent changes in sales to test the hypothesis that upward changes have a different effect from downward changes in sales. If the absolute values

[4] Indicated orally by Mark Robinson who quotes John von Neumann and H. H. Goldstine [2]. Actually, a number of the multiple correlations performed in the course of this study were considered to be unstable. The results of such multiple correlations are neither presented nor discussed here.

of changes in sales for a certain lag show a statistically significant positive *partial* correlation with inventory changes, when algebraic changes in sales for the same lag are included as another variable and also show a significant positive partial correlation, then the evidence indicates that an increase in sales for the given period has a greater effect on inventory change than does a decrease.[5]

In addition to the variables of primary interest, other variables were included in the multiple regression to investigate their effect upon inventory change. These additional variables included changes in wholesale prices (expressed as a percent of the twelve-months moving average of wholesale prices) for several different lags, inventory changes (expressed as a per cent of the twelve-months moving average of inventory change) for several different lags, the prime rate of interest and change in the prime rate of interest. Other variables that were included did not yield results of much value.[6] Since several series were included for different lags, each lag constituting a separate independent variable, a total of 33 variables were included.

The multiple regression program permitted the elimination of variables which proved undesirable. The last phase of the multiple regression (matrix inversion) could then be performed without those variables. Consequently, a substantial number of multiple correlations were performed to test the effect of eliminating or not eliminating certain variables or groups of variables, or to eliminate variables which were not statistically significant or which caused instability.

The observations were broken into two periods which eliminate the months that were most disturbed by the second world war and the Korean war. The first and most important of these periods was the interwar period, March 1920 through July 1941. October 1948 through June 1950, and November 1951 through March 1956 were included in the postwar period. All the observations included in the interwar or postwar periods were included in what is called the combined period. Thus we have three sets of results: for the interwar period, for the postwar period, and for the combined period.

[5] If both algebraic sales-changes for period $n$-2 and the absolute values of sales-changes for period $n$-2 are included, we have two regression coefficients for period $n$-2. If we assume that the regression coefficient of the algebraic changes in sales for period $n$-2 is .5 and the regressional coefficient of the absolute values of changes in sales for period $n$-2 is .2, an increase in sales of 1 per cent during period $n$-2 will, on the average, cause an increase in inventories during period $n$ of .5 per cent plus .2 per cent, or .7 per cent. On the other hand, a sales decrease of 1 per cent during period $n$-2 would cause, on the average, a decrease in inventories of .5 per cent plus an increase of .2 per cent, or a net decrease in inventories of .3 per cent.

[6] These were deflated sales for period $n$-2, deflated stocks for period $n$-2, and the average stocks/sales ratio (average of period $n$-1, $n$-2 and $n$-3).

## II. *Results and Their Interpretation*

By far the most interesting and important results were the estimates of the amount of induced investment in department store inventories associated with changes in department store sales. Since a considerable number of multiple correlations were performed, there is a question as to which provides the best estimate of the amount of induced investment. For the purpose of testing multiplier-accelerator theories, it is not necessary to determine whether changes in sales directly cause changes in inventories or whether changes in sales are associated with changes in some other variables which cause changes in inventories. Hence, it is not merely unnecessary but actually undesirable to include any variables other than the dependent variable per cent change in inventories during period *n*, and per cent changes in sales in various periods. Inclusion of any other variables would tend to distort measurements of the extent to which changes in sales are followed by changes in inventories.[7]

Great care must be taken in interpreting the regression coefficients. Since the regression equation has been computed as an expression of the relationship of per cent change in deflated stocks and per cent change in deflated sales, a regression coefficient of .3 for changes in sales during period *n*-2 indicates that an increase in sales of 1 per cent during period *n*-2 is, on the average, associated with an increase in inventories of .3 per cent during period *n*. The effect on the economy, however, is more clearly measured by the dollars-and-cents change in inventories (using constant dollars) than by the percentage change. To estimate the dollars-and-cents change in inventories associated with a change in sales of $1.00 it is necessary to know the relative sizes of stocks and sales.

To meet this need, the average stock/sales ratio for the period has been computed. This ratio, however, differs from the usual type of

[7] A possible exception would be the existence of accidental correlation. For example, if $x$ and $y$ are two independent variables, both causal factors with respect to the dependent variable, and are accidentally (not functionally) correlated, the omission of either $x$ or $y$ will improperly cause the other to act as a proxy variable for the omitted one. The proper safeguard against such an occurrence is testing by means of confidence limits. Due to the large number of observations included in both the interwar and combined periods, the one per cent confidence limits are quite small. This means that any accidental correlation between $x$ and $y$ is almost surely so small that they are extremely poor proxy variables for each other. Consequently, the likelihood of obtaining misleading results from this cause is quite limited.

On the other hand, if $y$ is functionally related to the dependent variable but causation runs from the dependent variable to $y$, inclusion of $y$ will almost surely distort the relation between $x$ and the dependent variable. In the current study this represents a far greater danger. Furthermore, if $x$ causes $y$, while $y$ is significantly correlated to the dependent variable solely for this reason, inclusion of $y$ will tend to rob $x$ of some of its true effect. This, again, seems far more serious than the first-mentioned danger.

stocks/sales ratio in which both stocks and sales are valued at retail prices. The true amount of investment by department stores must be measured by what department stores have spent on laying in inventories, rather than by the retail value of those inventories. Consequently, it was necessary to compute a rather usual type of stocks/sales ratio, in which stocks are expressed at cost and sales are, of course, expressed in actual retail prices. The average stocks/sales ratios obtained in this manner for the interwar, postwar and combined periods were 2, 1.7 and 1.9, respectively.

These stocks/sales ratios were used in the following manner. The ratio of 2 for the interwar period indicates that a 1 per cent change in inventories involved a change of 2 times as many dollars as a 1 per cent change in sales. The regression coefficient, for any one of the variables expressed as percentage changes, indicates the percentage change in inventories associated with a 1 per cent change in that variable. Consequently, a regression coefficient of .3 for proportional change in sales during period $n$-2 indicates that a change in sales of $x$ dollars during period $n$-2 is associated with an increase in inventories, during period $n$, of .3 times 2 times $x$ dollars or .6 times $x$ dollars. The product of this multiplication (the regression coefficient times the stocks/sales ratio) will be called the acceleration coefficient. The sum of the acceleration coefficients for the antecedent periods which are found to be statistically significant will be called the total acceleration coefficient.[8]

We have used the total acceleration coefficient to measure the sum of the effects of changes in sales over several past periods on inventory investment during period $n$. The total acceleration coefficient, however, also measures the sum of the effects of changes in sales during period $n$ on inventory investment during various future periods. For example, the month of January is period $n$-1 when $n$ is February, $n$-2 when $n$ is March, $n$-3 when $n$ is April, etc. Consequently, the estimated total effect of changes in sales during January, on inventory changes during the following months, can be found from the sum of the regression coefficients for periods $n$-1, $n$-2, $n$-3, etc.

More important than the acceleration coefficients are the estimates of the investment and total investment coefficients. The invesment coefficients were obtained in the following manner. The regression coefficients (which relate percentage changes in inventories to percentage changes in sales) were multiplied by the ratio of stocks (at cost) to value-added by department stores. The product represents the ratio of inventory change to change in value-added. Since value-added is the

[8] These coefficients are not the same as the Hicksian investment or total investment coefficients. They are presented here only because the acceleration principle is frequently couched in terms of the relationship between investment and sales.

income created by department stores, this is also the ratio of inventory investment to change in income created by department stores. The sum of the investment coefficients which are found to be statistically significant will be called the total investment coefficient.

The relationship between the total acceleration coefficient and this total investment coefficient is not complicated. The total acceleration coefficient is the ratio between inventory change and change in sales. The total investment coefficient is the ratio between inventory change and change in value-added. Consequently it is possible to convert the total acceleration coefficient into the total investment coefficient by multiplying it by the ratio of sales to value-added. This is feasible since value-added is a stable percentage of sales. Since our adapted multiplier-accelerator theory is concerned with the relationship between investment and changes in income or production, the total investment coefficient is more convenient than the total acceleration coefficient in testing that theory.

As mentioned above, regression coefficients which are not statistically significant are excluded from the total acceleration and total investment coefficients. Unfortunately, however, tests of statistical significance rely on the assumption of a known probability distribution of the quantity whose significance is being tested. Since these probability distributions were not known, the tests of significance are questionable. Questionable tests of significance, however, are probably better than none at all.

Since no preferable assumption was available, the tests of significance were based on the assumption of normal distribution. The standard error of each regression coefficient was calculated. Under the assumption of normal distribution, each regression coefficient is significant at the 1 per cent level if at least 2.58 times as large as its standard error. Regression coefficients of the sales-change variables which were not significant by this test were not included in the computations of the total acceleration or total investment coefficients.[9] Variables which consist of the absolute values of percentage changes in sales were also not included.

The results of the multiple correlations considered most suitable for measuring the amount of induced investment are given in Table 1. Variables other than percentage inventory change for period *n* and percentage changes in sales for several periods were not included in these particular multiple correlations.

[9] Due to the fact that the multiple regression program permits the elimination of only a limited number of variables, elimination of any of the variables included in the multiple correlations presented in Table 1 would require a substantial amount of recomputation. Elimination of other sales-change variables (i.e., other than those included in Table 1) which were not statistically significant had very little effect upon the regression coefficients of the remaining sales-change variables.

TABLE 1—INDUCED INVESTMENT IN DEPARTMENT STORE INVENTORIES

| Variable[a] | Interwar Period (March 1920 through July 1941) | | | | Postwar Period (October 1948 through June 1950 and November 1951 through March 1956) | | | | Combined Period (Combined Interwar and Postwar periods) | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Regression Coefficient | Standard Error of Regression Coefficient | Acceleration Coefficient | Investment Coefficient | Regression Coefficient | Standard Error of Regression Coefficient | Acceleration Coefficient | Investment Coefficient | Regression Coefficient | Standard Error of Regression Coefficient | Acceleration Coefficient | Investment Coefficient |
| $\Delta I_n$ | 1.000 | — | — | — | 1.000 | — | — | — | 1.000 | — | — | — |
| $\Delta S_n$ | 0.048 | 0.027 | 0.096[b] | 0.245[b] | —[e] | — | — | — | 0.033 | 0.025 | 0.063[b] | 0.162[b] |
| $\Delta S_{n-1}$ | 0.222 | 0.029 | 0.444 | 1.132 | 0.285 | 0.060 | 0.485 | 1.254 | 0.236 | 0.027 | 0.448 | 1.156 |
| $\Delta S_{n-2}$ | 0.279 | 0.028 | 0.558 | 1.423 | 0.213 | 0.072 | 0.362 | 0.937 | 0.271 | 0.026 | 0.515 | 1.328 |
| $\Delta S_{n-3}$ | 0.153 | 0.029 | 0.306 | 0.780 | 0.090 | 0.073 | 0.153[b] | 0.396[b] | 0.148 | 0.027 | 0.281 | 0.725 |
| $\Delta S_{n-4}$ | 0.128 | 0.029 | 0.256 | 0.653 | 0.062 | 0.071 | 0.105[b] | 0.273[b] | 0.113 | 0.026 | 0.215 | 0.554 |
| $\Delta S_{n-5}$ | 0.071 | 0.027 | 0.142 | 0.362 | 0.209 | 0.063 | 0.355 | 0.920 | 0.084 | 0.024 | 0.160 | 0.412 |
| $(\Delta S_{n-6}+\Delta S_{n-7}+\Delta S_{n-8})$ | —[e] | — | — | — | 0.130 | 0.047 | 0.221[d] | 0.572[d] | —[e] | — | — | — |
| $\lvert(\Delta S_{n-1}+\Delta S_{n-2})\rvert$ | −0.081 | 0.035 | −0.162[c] | −0.413[c] | −0.256 | 0.085 | −0.435[c] | −1.126[c] | −0.101 | 0.032 | −0.192[c] | −0.495[c] |
| $\lvert(\Delta S_{n-3}+\Delta S_{n-4})\rvert$ | 0.063 | 0.035 | 0.126[c] | 0.321[c] | 0.169 | 0.085 | 0.287[c] | 0.744[c] | 0.072 | 0.032 | 0.137[c] | 0.353[c] |
| Total | | | 1.706 | 4.350 | | | 1.865 | 4.827 | | | 1.619 | 4.175 |
| $\bar{R}$ | 0.611 | (0.040)[f] | | | 0.576 | (0.083)[f] | | | 0.587 | (0.037)[f] | | |

*See facing page for explanation of symbols.*

## III. *Conclusions*

Little confidence is placed in the results for the postwar periods, since the small number of observations causes the standard errors of the regression coefficients to be comparatively large and since the disturbed nature of the times might logically be expected to cause abnormal results. Since results for the postwar period are somewhat different from the interwar results, and this may be due to a fundamental change in the relationships between the variables or to the disturbed nature of the postwar period, there are possible objections to combining the interwar and postwar periods. Consequently, unless otherwise stated, only results obtained for the interwar period are discussed below. Results for the combined period, however, are very similar to the interwar period, with only minor exceptions.

1. *Induced investment.* The estimate of 4.35 for the total investment coefficient for the interwar period means that a change of $1 in the

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[a] Symbols used in this table are as follows:
$n$ is the period;
$P$ is the retail price deflator centered in the middle of the month;
$p$ is the retail price deflator calculated for the end of the month;
$S$ is the seasonally adjusted index of sales;
$I$ is the seasonally adjusted index of stock (inventories);

$$\Delta I_n = \frac{100\left(\dfrac{I_n}{p_n} - \dfrac{I_{n-1}}{p_{n-1}}\right)}{\dfrac{1}{12}\left(\dfrac{I_n}{p_n} + \cdots + \dfrac{I_{n-11}}{p_{n-11}}\right)};$$

$$\Delta S_n = \frac{100\left(\dfrac{S_n}{P_n} - \dfrac{S_{n-1}}{P_{n-1}}\right)}{\dfrac{1}{12}\left(\dfrac{S_n}{P_n} + \cdots + \dfrac{S_{n-11}}{P_{n-11}}\right)};$$

$|(\Delta S_{n-1}+\Delta S_{n-2})|$ is the absolute value (i.e., numerical value without regard to sign) of the sum of $\Delta S_{n-1}$ plus $\Delta S_{n-2}$.

[b] Variables whose regression coefficients are less than 2.58 times their standard errors are not included in the total acceleration and total investment coefficients.

[c] Variable which consist of the absolute values of changes in sales are not included in the computation of the total acceleration and total investment coefficients.

[d] Since the coefficient for this variable is the average coefficient applying to 3 different periods it is counted 3 times in computing the total acceleration and total investment coefficients.

[e] These variables were so highly insignificant in other multiple correlations that they were eliminated in the multiple correlations presented here.

[f] Standard error of $\bar{R}$.

*Note:* The regression coefficients and their standard errors, which were calculated to 8 significant figures, are here rounded to 3 decimal places.

level of monthly income created by department stores led to an average change of $4.35 in department store inventories (valued at cost). This estimate is sufficiently large to lend considerable support to the adaptation of the multiplier-accelerator type of theory to the explanation of minor business cycles. There is no reason to assume, however, that the coefficients applying to other sectors of the economy were equally large.

The estimated total investment coefficient for the postwar period of 4.83 is similar to the estimate for the interwar period. The average lag in the adjustment of inventories to changes in sales, however, was considerably longer. Although the postwar period used in this study does not include times of extensive shortages after the second world war or during the early part of the Korean war, it remains possible that minor shortages of goods (i.e., partial contact with the "ceiling" in Hicks' terminology [1, Ch. 10]) could be responsible for the greater average lag.

Results obtained for the variables which consisted of the absolute values of changes in sales indicate that the average lag was greater during expansions than during contractions. This suggests the possibility that partial contact with the ceiling was responsible for some of the delay during expansions. The fact that the difference between expansions and contractions was greatest during the postwar periods, when partial contact with the ceiling seems most likely to have been a significant factor, tends to support this belief.

2. *Forecasting by store officials.* Another point suggested by the sales-change variables is that department store officials were rather good at forecasting changes in sales. This follows from the fact that the partial correlation between sales change for period *n* and inventory change for the same period was generally positive. Negative partial correlations might have been expected, since an unforeseen increase in sales tends to cause a temporary drop in inventories.

3. *Other variables.* A substantial number of variables, not included in Table 1, were used in other multiple correlations performed in the course of this study. The conclusions obtained from these other variables are as follows:

*a. Past inventory changes.* Past inventory change (expressed as a percentage of the 12-months moving average of deflated inventories) was included for several different lags, each lag constituting a separate independent variable. Results appear to indicate that past inventory change tended to reduce or forestall changes in the same direction and to reinforce any influences causing a change in the opposite direction.[10]

[10] An exception was percentage inventory change lagged by only one month, which howed significant positive partial correlations. Due to the delays involved in a decision to ncrease inventories as well as the uncertain length of these delays, a decision to increase

The measured effect was so slight, however, that in only one multiple correlation was it significant at the 1 per cent level and then only barely so. The maximum negative partial correlation was found for a lag of about 7 months.[11]

*b. The rate of interest.* The rate of interest on prime commercial paper, 4- to 6-months, lagged by one month, was included as an independent variable in several multiple correlations. The simple and partial correlations obtained were uniformly positive but of such small size as not to be statistically significant,[12] which contradicts the belief that high interest rates significantly deter inventory investment or that low interest rates favor inventory investment.

Change in the prime rate of interest between periods *n*-1 and *n*-7 was included as an independent variable in some of the multiple correlations which also included the prime rate. In every case, results did not approach statistical significance.

*c. Wholesale prices.* An index of wholesale prices was calculated from two components of the Bureau of Labor Statistics Wholesale Price Index: textile products prices, given a weight of 2; and house furnishing goods prices, given a weight of 1. Changes in this index, expressed as a percentage of the 12-months moving average of the index, were included for several different lags, each lag constituting a different independent variable. The only variable of this group which attained statistical significance was percentage change in wholesale prices for period *n*-2. This variable showed a positive partial correlation which was only barely significant at the 5 per cent level in only one of the numerous multiple correlations in which it was included. Consequently, it appears that changes in wholesale prices were not an important factor affecting inventory investment by department stores.

*d. The remaining variables.* No significant conclusions were obtained for the remaining variables: deflated sales for period *n*-2; deflated stocks for period *n*-2; and the average stocks/sales ratio for periods *n*-1, *n*-2, and *n*-3. This, however, is not a wholly negative conclusion.

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inventories during period *n*-1 will usually also be a decision to increase inventories during period *n*. If this is true, the positive partial correlation does nothing to show what conditions were associated with decision to increase inventories, but instead, tends to reduce the observed effect of the variables which led to this decision.

[11] More precisely, the maximum negative partial correlation was found for a single variable which consisted of the sums of per cent inventory changes in periods *n*-6, *n*-7 and *n*-8.

[12] In two multiple correlations the prime rate showed a positive partial correlation which was significant at the 1 per cent level. In both cases the multiple correlation included another independent variable which was so highly correlated with the prime rate as to create a condition of instability when both are included in the same multiple correlation. Consequently, the only two cases in which the prime rate was statistically significant should be disregarded.

The inability to find any variables other than past changes in sales which importantly influenced inventory investment by department stores tends to support the belief that this type of investment was primarily induced by changes in sales.

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# CANADA'S ECONOMIC PROSPECTS

## *A Review Article*

*By* SIMON KUZNETS*

This paper is a review, necessarily selective and sketchy, of the *Final Report* and supporting monographs that are the published product of the Royal Commission on Canada's Economic Prospects. The task of the Commission, in the language of the relevant Privy Council order of June 1955, was to "inquire into and report upon the long-term prospects of the Canadian economy, that is to say, upon the probable economic development of Canada and the problems to which such development appears likely to give rise . . .".[1] The order then lists "without limiting the generality of the foregoing," specific topics, to wit: "(a) developments in the supply of raw materials and energy sources; (b) the growth to be expected in the population of Canada and the changes in its distribution; (c) prospects for growth and change in domestic and external markets for Canadian productions; (d) trends in productivity and standards of living; and (e) prospective requirements for industrial and social capital."[2]

In executing this assignment, the Commission journeyed extensively in Canada, held public hearings in 14 cities from October 1955 through March 1956, heard more than 750 witnesses, and received 330 submissions. It also sponsored the 33 monographs listed in the appendix, most of them prepared by the Commission's staff but some by other organizations and individuals. The present review is limited to the published studies and the *Final Report*;[3] and for the most part concentrates on the monographs.

The grouping of the monographs in the Appendix below, like any classification of a complex and somewhat heterogeneous product, is partly arbitrary. But the three broad groups distinguished—basic aggregates, industry studies, and foreign economic relations—suggest the main divisions of the inquiry and are a guide to the order followed in subsequent discussion. We deal first with population, labor force, product, and capital and their quantitative projection over the next 25 years. These constitute the framework for deriving the projected volumes of consumer expenditures and housing and social capital, and

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[1] See *Final Report*, p. 471, in the classified list of publications in the Appendix to this paper. Throughout the paper monographs are referred to by number as given in this list.

[2] *Ibid.*, pp. 471-72.

[3] A review of the *Preliminary Report* by D. McC. Wright appeared in the June 1958 issue of this Journal, pp. 463-65.

for consideration of regional problems; and most of them are indispensable bases for evaluating prospects of specific industries and trends and problems in the area of international economic relations. Next, we touch upon the industry studies, but because there are so many and they are so specialized, our comments can do less justice to them than to the other groups of monographs. We then turn to the studies dealing with the international economic relations of Canada, particularly with the United States. A few general comments on the scope of the inquiry conclude the review.

## I. *The Aggregates and Projections*

Monograph 2 is the key study of the aggregates and, in fact, of the whole range of inquiry of the Commission. Drawing partly upon the review of growth experience in monograph 1, partly upon the longer record back to 1926, partly upon some general knowledge of intersectoral differences in productivity and capital requirements, Hood and Scott follow a simple but systematic procedure. They derive first a projection of total population by age and sex—with three variants for differing amounts of *net* immigration; then a projection of the labor force, with a distinction between the armed services (kept by assumption at a constant level of 120,000) and the civilian labor force, the latter reduced to the employed by an allowance of 3 per cent for unemployment. Next the employed civilian labor force is distributed among three major sectors—agriculture, business, and civilian government and community services; and for each, gross domestic product per worker in constant prices is projected —with an allowance for a downward trend in average hours in agriculture and in business and with two variants of rates of growth of product per man-hour in the latter. The number employed is multiplied by projected product per employed in each sector and to the total, after some minor adjustments, residential rents are added to yield gross domestic product at factor costs. Next, indirect taxes are added, at a flat 13 per cent mark-up, to give gross domestic product at market prices; and a projection of net payments abroad of dividends and interest provides the final step by which gross national product at market prices is derived. These estimates are then used to project new nonresidential construction and durable equipment by an extensive use of gross capital-output ratios (combined with depreciation adjustments) for the various sectors; and, by reference to the sectoral estimates and to estimates in other monographs, the industrial distribution of gross domestic product is estimated.

Many of the estimates in monograph 2 provide the basis for projecting consumer expenditures in monograph 4. Personal expenditures on goods and services are derived from gross national product, in two steps: the first yields personal disposable income as a ratio to gross national product (expenditures) varying from 67 to 68 per cent; the second yields aggregate consumption by an allowance for personal savings of about 6 per cent of personal disposable income. Then total personal expenditures on goods and services, in constant prices, are reduced to a per capita basis, and on the basis of changes in the structure of consumer expenditures from 1926-29 to 1952-55 the total projected to 1980 is distributed among 9 major categories and a few subdivisions (4 in

household operation, 3 in transportation, and 3 in personal and medical care). In monograph 5, population estimates, combined with a projection of further urbanization and consideration of income trends, yield estimates of *needs* for new residential construction; and for each social capital category—hospitals, schools and universities, roads and streets, waterworks, sewerage systems, churches and other religious buildings—the relation of construction and capital needs to various specific indexes in the past is briefly discussed and a global projection is derived for 1956-80. Here also extensive reliance is placed on the projections in monograph 2, and on some of those in monograph 4. Monograph 6, on regional aspects, is the only study in which projections are limited to population and labor force; while the quantitative aspects of present interregional differentials and problems are pointed up by means of a variety of specific information, only a qualitative appraisal of growth prospects is given.

Two additional comments may clarify the nature of the quantitative framework of the inquiry. The first relates to the broad assumptions, explicitly stated, which govern the whole cast of the study—in the monographs and in the *Final Report*. There were four of these, and they are, as stated in the *Final Report*: (1) "that a global war will be avoided" (p. 3), although sizeable defense expenditures will continue; (2) "that although there will continue to be cyclical business fluctuations, the recurrence of a major depression such as that of the 1930's need not be anticipated" (pp. 3-4); (3) "that over the next twenty-five years there will be no changes either in the general price level or in price relationships" (p. 4), although this assumption is clearly recognized as unrealistic and elsewhere is stated somewhat less categorically (e. g., in monograph 2, p. 2, "it is assumed that the eroding force of inflation will be restrained"); (4) "that there will be no major changes in the economic policies either of the Canadian or other governments" (p. 4), again, as stated in the text, an assumption as unrealistic as assumption (3) but just as necessary if projections are to be made. Clearly, these assumptions impart a special meaning to the estimates, to be commented upon below.

Second, the forecasts require some specific procedures for projecting past levels or rates of change into the future—at numerous points in the sequence of steps only barely sketched out above. While it is impossible to describe these procedures in detail here, we can characterize them as essentially empirical judgments involving free-hand extrapolations of the past, not rigidly defined functions fitted mathematically to past records and then calculated for the future. To illustrate, in the projections in monograph 2, mortality rates for the separate age and sex groups are gradually reduced—for male infant mortality from 36 per thousand in 1954 to 16 in 1975-80 (and for females, from 28 to 13)—in view of the rates in other countries and of the continuous reductions made in recent decades; rates of labor force participation by age and sex are allowed to drift gradually up or down in accordance with recent trends; rates of growth in product per man or man-hour accord with recent changes in those rates in the major sectors of the civilian economy; and rates of change in the distribution of consumer expenditures by major categories also follow recent changes. The experience in the United States, with its sub-

stantially higher per capita income and relatively well-studied records, may foreshadow the growth of Canada over the projected period, is ferred to frequently.

What do the projections show? A few figures, given in Table 1 wit parable estimates for the past 25 years, will perhaps suffice. Possit should also note that the projected percentage distribution of gross n product (expenditures) among personal expenditures, government pur and capital formation will not differ much from the early 1950's; bu the rise in per capita personal expenditures, the shares of expenditures o clothing, and rent will continue to decline and the shares of expendit autos, electrical household appliances, and medical care will continue

One would expect the projected rates of growth to be higher than th for the 25 years covered by the quantitative records used so extensi deriving the projections, viz., the late 1920's to the mid-1950's, since th ticular period was affected by the great depression of the 1930's and the world war. However, two aspects of these projections deserve note. Fi rates of growth that they suggest seem high in comparison with the ra equally long periods in Canada's past back to the mid-19th centur natural increase in the projected population estimates is 41 per cent initial population) for 1955-75, and 40 per cent for 1960-80; while gro projected total population, based on the middle assumption for immi is 54 per cent for 1955-75 and 52 per cent for 1960-80 (monograph 2, 4.15, p. 175). Of the 9 overlapping periods of two decades each, cover span of a century (1851-1951), in only 3 (1851-71, 1861-81, and 1901 the rate of natural increase as high as or higher than those projected, only 2 (1851-71 and 1901-21) is the rate of increase of total populatic 50 per cent (*ibid.*, Table 4, p. 156). Comparison with past rates of gro gross national product per capita also suggests that the projection i According to O. J. Firestone, per capita gross national expenditure in co (1935-39) dollars rose at the rate of 17.8 per cent per decade from 1 1900; 16.1 per cent per decade from 1900 to 1929; and 19.3 per ce decade from 1929 to 1953.[4] The projected rate of growth for 1955-80 the average of the two variants, is 22.8 per cent per decade.

Second, whereas the over-all rate of growth for the projected 25 y relatively high, the change in the structure of national product is mod at least when compared with the immediately preceding past. Thus fron 29 to 1953-55 the share of personal consumer expenditures in gross n product declined from 72 to 63 per cent while the share of government ditures rose from 10 to 18 per cent; but the shares of these two majo ponents in 1979-81 are set at 64 and 17 per cent respectively—not ve ferent from the shares in 1953-55 (see monograph 2, Table 7.4, p. 3 similar impression is conveyed by the distribution of gross domestic p (excluding residential rent and the gross product of the armed forces) 7 major industry sectors. If we measure the total shift from 1927-29 to

[4] See his *Canada's Economic Development 1867-1953*, Income and Wealth, Ser International Association for Research in Income and Wealth. London: Bowes & 1958. Table 10, p. 66. See pages 431-33 this journal for review of the book.

TABLE 1—BASIC AGGREGATES AND PERCENTAGE CHANGES, PAST AND PROJECTE
25-YEAR PERIODS[a]

| | Past Period | | | Projected Period | |
|---|---|---|---|---|---|
| | Initial Absolute Figure (1) | Terminal Absolute Figure (2) | Per Cent Change (3) | Initial Absolute Figure (4) | Terminal Absolute Figure (5) |
| 1. Population (million) | 10.46 (1930) | 15.57 (1955) | + 49 | 15.57 (1955) | 26.65 (1980) |
| 2. Labor force (million) | 4.06 (1930) | 5.68 (1955) | + 40 | 5.56 (1955) | 9.93 (1980) |
| 3. GNP, 1949 prices, ($ billion) | 8.45 (1926–29) | 20.33 (1952–55) | +133 | 21.6 (1955) | 61.75 (1980) |
| 4. GNP, per capita, 1949 prices ($) | 863 (1926–29) | 1,355 (1952–55) | + 54 | 1,387 (1955) | 2,317 (1980) |
| 5. Personal expenditures, per capita, 1949 prices ($) | 597 (1926–29) | 880 (1952–55) | + 45 | 880 (1952–55) | 1,560 (1980) |
| 6. Gross domestic investment, 1949 prices (col. 1–3) and 1955 prices (col. 4–6) ($ billion) | 2.26 (1927–29) | 4.99 (1953–55) | +114 | 6.3 (1953–55) | 18.3 (1979–81) |

[a] Years to which estimates refer are shown in parentheses. Whenever the aggregat to a period longer than a year, the average per year is given. The percentage ch columns 3 and 6 is reduced to a 25-year basis whenever the period covered is lon

*Sources:*

Line 1: from monograph 2, Tables 4.14 and 4.15, p. 175. The 1930 figure is adjus wards by 2.6 per cent to allow for the inclusion of Newfoundland; and the projec ure is that based on the assumption of 75,000 annual immigration.

Line 2: columns 1 and 2 from monograph 29, Table 35, p. 226, and include armed columns 4 and 5 from monograph 2, Table 4.23, p. 186, and exclude armed forces.

Line 3: columns 1 and 2 from monograph 29, Table 36, p. 228; columns 4 and monograph 2, Table 5.20, p. 226. The mean of the high and low estimates (based variants of the growth in product per worker in the business sector) is shown h

Line 4: columns 1 and 2 from monograph 29, Table 36, p. 228; columns 4 an rived from lines 1 and 3 above.

Line 5: from monograph 4, Table 1, p. 3.

Line 6: columns 1 and 2 from monograph 2, Table 7.D.2, p. 506. This tabl domestic investment excluding government nondefense investment expenditure. The la added on the basis of its percentage shares in the totals in current prices for 1927 1953-55 (11 per cent and 17 per cent respectively), as shown in *ibid.*, Table 7.6, Columns 4 and 5 from *ibid.*, Table 7.6, p. 324. The average in column 4 is in curren but would differ little from the figure in 1955 prices.

55 by taking the differences between the percentage shares for the two date for each sector and adding them regardless of sign, the resulting index c change over the past 26 years is 25; the same index for the projected change from 1953-55 to 1979-81 is only 19.8 (*ibid.*, Table 7.2, p. 315). To be sur the major reason for this moderate change is that the share of agricultur already low in 1953-55, could not be reduced absolutely as much as its highe share in 1927-29; and the results might be different if a more detailed classifi cation of the nonagricultural division were available. But the more detaile distribution of consumer expenditures among 15 categories yields a simila result: from 1926-29 to 1952-55 the over-all measure of shift is 19 wherea from 1952-55 to 1980, a slightly longer period, it is 13.3, almost a third lowe (monograph 4, Table 26, pp. 74-75).

In evaluating the projections, it is helpful to distinguish between question relating to: (a) the general meaning of the forecasts; and (b) the degree o confidence that can be attached to the quantitative results, given the empirica data and the methods used in the estimation.

(a) Projections of the type involved in this inquiry can be characterized a measures of the probable course of an economy—given the major assumption or conditions actually used in preparing them. The central question is then on of defining the assumptions—as they were *used*, not as stated. The reason fo the distinction becomes clear if we ask: Were *all* four major assumptions, ex plicitly stated and quoted above, indispensable conditions upon which th projections were calculated, and were they the only major ones?

There is no doubt that an all-out war and a major depression like that of th 1930's were excluded from consideration in preparing the projections. Th high rates of over-all growth and the relatively moderate structural change projected are, in themselves, evidence of this exclusion. But is it evident tha continuing price rises of the magnitude that occurred during the years fol lowing the second world war were excluded? To be sure, the projections ar given in constant dollars, and neither the probable changes in the genera price level, nor, with few exceptions, the differential price movement for th several sectors in the economy are estimated. But this does not mean that th projection is explicitly *conditioned* by an absence of general or differentia price changes—in the sense that any future price changes, unless they rep resent a "runaway" inflation (whatever this somewhat vague term means) would seriously invalidate the projections, as an all-out atomic war or a grea depression would. On the contrary, one might argue that continuation of sus tained price rises, of a magnitude whose limit would have to be explored, would make the calculated projections more rather than less probable. Since they have been derived from a record of the past in which prices did increase substantially—the price index implicit in gross national expenditures having risen from 77 in 1945 to 124 in 1955 (1949 = 100), or over 60 per cent (see monograph 4, Table 4, p. 33)—the projected high rates of growth combined with relatively full employment are perhaps more likely to be realized with rising rather than constant prices. In short, the stated assumption of price constancy is merely an indication of the omission of changes in prices from the projection, not a *condition* of its validity.

A similar question can be raised about the assumption relating to policy changes. What exactly is meant by absence of "major" policy changes on the part of the Canadian and *other* governments? To begin with, the most revolutionary changes in a number of the 100-odd sovereign states in the world are unlikely to have much effect on the economic progress either of Canada or of the countries important to it. But even if we consider only Canada and its close neighbors and partners in the network of international economic relations, the vague term "major" leaves room for misconception. Canada experienced rapid growth during the recent decades when, if we can judge by contemporary reactions, substantial changes in government policy did occur, not only in Canada but also in the United States and other close partners (e.g., a phenomenal increase in the personal income tax burden, an almost revolutionary change in foreign aid policy, etc.). Could it not be argued that the projection would stand so long as changes in government policy do not inhibit economic growth any more than they have in the past? With the historical period in the past specified, the assumption underlying the conditional nature of the projection could then be defined properly.

Thus, as the above comments suggest, of the four assumptions stated one is superfluous and one is too vague; and, moreover, a host of governing assumptions have not even been mentioned. Some are so obvious that they need no explicit statement, e.g., the prospective stability of the universe in which mankind, Canada included, lives, a stability easily assumed over periods that are long in human terms but short for universal natural processes. Others, however, that are readily accepted, are not too obvious. Clearly, in making *any* projection, we are assuming the continuation of the cumulative effect of human knowledge on economic production; and in particular the continuation of the dynamism of a country's economic and social institutions, of the search for greater economic product by its inhabitants, and of the country's capacity to adjust its institutions to changing conditions. This complex of what might be called constitutive assumptions (as distinct from the conditional ones, exemplified by exclusion of a war or a major depression) is at the base of every projection; and unfortunately these basic assumptions cannot be tested by any meaningful probability approach—except perhaps an impossibly wide study of many large human societies existing under differing conditions. Since these assumptions underlie the whole of the present inquiry, we can reformulate our definition of the projections and describe them not as *forecasts* but as the quantitative *implications* of the assumed continued dynamism of the Canadian economy and society, on conditions of absence of an all-out war and a great depression.

(b) If the projections are quantitative implications of an assumed set of social institutions and drives, their reliability can be appraised only by considering the empirical record on which the projection coefficients have been based. The crucial questions are then: how stable were the coefficients in the past under a variety of relevant changes covered in the record (relevant meaning those that could affect growth); and how effective were the procedures used to establish these coefficients? Thus the most reliable basis for projections requires records for many long periods and for many countries, the

periods excluding major wars and great depressions and the countries belonging to a group of which Canada could be considered a unit with some unique but also many representative features. Analysis of these records would reveal the patterns of growth over spans of 25 years that proved stable under a variety of relevant changes. These common and stable features would constitute empirical generalizations, whose relation to each other could be tested by economic and statistical theory. This basis for projections would, of course, have to be modified for any one country to take account of features unique to it. In our present state of ignorance of quantitative patterns of the comparative economic growth of nations, no such basis for projection is available, or likely to become available for years to come.

Consequently, the judgments necessary for a quantitative projection can be checked only by a narrow body of organized empirical data. In the present inquiry this body was limited almost exclusively to the record for Canada since 1926, with repeated reference to recent data for the United States and to the absence of detailed and consistent data for Canada (except for population) for years before 1926. But some measures of the economic growth of Canada for longer periods do exist—although less detailed and subject to wider errors than the data for the last 25 years—and at least one recent attempt to organize and interpret them has been made (by Firestone; see footnote 4). It is not clear why no effort was made to utilize the longer records and why a projection over a 25-year period was built essentially on the empirical record for one period of comparable length in the past. The time span from 1926 to 1955 is altogether too short to provide an objective basis for projections, especially since over half of it was affected by the great depression and the second world war. It is, of course, this circumstance that necessarily minimizes the use of mathematically fitted trend lines and compels extensive exercise of judgment and of free-hand interpolation and extrapolation.

It may also explain the rather high over-all rate of growth and the rather moderate extent of structural changes in the projections. With the empirical record limited to 1926-55, even a line connecting the late 1920's with the early 1950's may underestimate the probable rate of growth, since it covers the years of the great depression. The least affected span within the total period is the decade following the second world war, but even then adjustment must be made for the transitory effects of the readjustment immediately following the end of the war. The rates of growth in that decade were quite high: population grew 26 per cent from 1945 to 1955 (adding 2.6 per cent to the earlier year for Newfoundland) and gross national product per capita (in 1949 prices) grew at a decade rate of 20 per cent from 1948-50 (to omit the readjustment years after the war) to 1953-55. The projected rate of growth of population, using the middle assumption for immigration, is 24 per cent per decade; and that for gross national product per capita, using the mean of high and low, is 23 per cent per decade. It is tempting but unfair to say that the projected rates are merely extrapolations of rates observed in the short postwar period. However, it may be fair to suggest that, given the rapid growth in the recent decade and no explicit reference to the longer-term past in Can-

ada and elsewhere, it would have been difficult to defend appreciably lower rates of growth in the projection, or consider much higher ones.

But what of the narrow range of structural change suggested by the projections? This result may seem plausible since the depression and major war might have produced wide structural changes in the past 25 years, even though total population and per capita product did not grow as rapidly as it is assumed they will grow in 1955-80. But ordinarily a high rate of growth, particularly of per capita product, is associated with large changes in structure, in the distribution of product by both industrial source and type of use, at least within consumer expenditures. One wonders whether the brevity of the empirical record of reference, combined with some of its peculiarities, has not led to an underestimate of the structural changes projected. To illustrate, with per capita consumer expenditures rising from 1952-55 to 1980 by 77 per cent, the part spent on food drops from 27 to only 24.5 per cent, thus allowing for a rise in per capita expenditures on food of 60 per cent—whereas from 1926-29 to 1952-55, with per capita expenditures rising only 47 per cent, the part spent on food dropped from 30 to 27 per cent. Why should there be a similarly small decline in the share of expenditures on clothing and personal furnishings —from 12.4 to 11.2 per cent, whereas the decline over the past 25 years was from 13.9 to 12.4 per cent? To be sure, past changes in shares should not be extrapolated without regard to limits set by arithmetic and by the gradual exhaustion of the change-potential; and all long-term projections may tend to underestimate structural shifts because of the difficulty of visualizing changes that can radically modify the current structure, which is, after all, the springboard for the leap into the future. It is perhaps because of lack of knowledge of what new goods will emerge in the future to displace those which, like food and clothing, have low secular income elasticity of demand, that the shares of the latter in consumer expenditures were not reduced further—for such reductions would only swell unrealistically the shares of the more rapidly growing goods or of the quite meaningless "miscellaneous" group into which the unknown new goods would have to be thrown. Yet the question remains whether a longer historical perspective would not have led to a somewhat bolder treatment of changes in structure.

In the light of these comments one may conclude that the margin of error in the projections is large, certainly much larger than those shown in the summary of forecasts of gross national product in monograph 2 (p. 224). The ±7.7 per cent shown for gross national product (for the estimate of population based on 75,000 annual net immigration) is merely the range between the low and high estimate of the rate of growth in product per worker in the business sector. Clearly, compared with the middle projection of growth in gross national product per capita of 23 per cent per decade, the rate could easily be as low as 15 or as high as 30 per cent (it averaged 29 per cent in Sweden over a long period).[5] And correspondingly, the structural changes

[5] Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations: I. Levels and Variability of Rates of Growth," *Econ. Develop. and Cult. Change*, 5 (1), Oct. 1956. Table 1, p. 10.

could be wider or narrower than those suggested—in positive association with the rate of growth of the aggregates, particularly of product per capita. For reasons suggested above, defensible limits of the range cannot easily be set. Yet it is regrettable that no such attempt was made in the inquiry.

## II. *The Industry Studies*

The list includes eighteen monographs in this group, and it could be extended to cover monographs 30 and 32. These reports range widely in size—from pamphlets of less than 50 pages to weighty tomes of more than 400 pages; and in scope—from those dealing with major complexes of industries accounting for substantial shares of product and labor force, e.g., agriculture, mining and mineral processing, energy, secondary manufacturing, and service industries, to those concerned with limited local problems, e.g., the Nova Scotia coal industry. There are also notable differences in treatment, resulting partly from the large number of authors, some individual and some institutional, and partly from differences in availability of basic data and in the character of the problems in the respective sectors. It is impossible to provide anything approaching an adequate review here. One can only note the general pattern of the studies, some of the major conclusions, and a few questions that they raise.

In all the studies the first task was to provide a consistent and fairly detailed quantitative description of the specific industrial sector—its output, the labor force engaged in it, the variety of its products, the degree of its reliance upon domestic and foreign markets, the extent of competitive pressures on it from abroad. And for all, a historical, quantitative record usually extending back only to the late 1920's and sometimes for a shorter period is provided, although illuminating comments on the earlier decades are made for the sectors that played important parts in Canada's history. The concluding, and much briefer, parts of the monographs deal with the projection of future trends, usually linked with those for population, total product, and expenditures per capita; and the problems suggested by such projections are often raised, and sometimes discussed at length. For some sectors, e.g., energy, forest industries, and minerals and mineral processing, the question is largely whether the expected growth in demand—domestic and foreign—can be adequately met out of the resources, usually natural, available in Canada. For others, e.g., agriculture, fisheries, and secondary manufacturing, the emphasis is upon the possible limits to demand—either because of limited domestic markets or because of competitive pressures from abroad (often the United States); and the problem is one of adjustment of production and employment to these possible limits upon demand. And, of course, in the monographs on major sectors, which comprise a range of industries differing in their economic characteristics and growth potentials, there is detailed consideration of these components and the past and prospective shifts in weight among them.

Without question, these 18 monographs constitute a most valuable reference library on the past development, current structure, and prospective trends of Canadian industry. It is difficult to think of a collection of similarly competent, systematic, up-to-date industry studies for any other country. Despite the

uneven quality of the analysis and writing—inevitable with so many authors and with studies completed in the relatively short period of about two years—the wealth of data organized, the skill of the presentation of the major trends and problems, and the generally high level of interest evoked are all most impressive, at least to this reviewer who is not too familiar with industrial economics in general, or with Canada's industries in particular. The monographs by John Davis on mining and mineral processing, and on energy prospects; by Drummond and Mackenzie on agriculture; by Fullerton and Hampson on the secondary manufacturing industries; by the Royal Bank of Canada on the construction industry; by J-C. Lessard on transportation (containing some valuable calculations of costs and burdens of competing forms of transportation); and by the Bank of Montreal on the service industries are especially to be noted.

What do these monographs show with respect to differences in growth prospects for the various industries? In considering these differences, and the shifts in the industrial structure of Canada's national product and labor force over the next 25 years, we can perhaps most effectively summarize the contribution of these monographs in Table 2, which indicates the broad conclusions. The coverage of the specific industrial sectors is given in the notes to the table.

Some of the shifts in the structure of gross domestic product and of the civilian employed labor force, both past and projected, accord with what we would expect in a developed country in the process of further growth. Thus the decline in the share of agriculture in both product and labor force is marked over the last 26 years and is projected to continue further to 1980. Likewise, the rise in the share of total manufacturing in gross product is large from 1927-29 to 1953-55 and is projected at an appreciably lower rate of rise to 1980; and the rise in its share in the labor force over the past 26 years is to be succeeded by a slight drop from 1953-55 to 1979-81. Finally, the rise of the shares of the service groups in the labor force, particularly trade and finance, government, and all services, is accompanied by a failure of their shares in gross domestic product to rise. All of these trends have been observed in other countries, e.g., the United States.[6]

Yet two Canadian trends appear to be distinctive. One is the large rise in the share of the resource industries in gross domestic product, although not in their share in the labor force—a reflection of Canada's growing importance as a major supplier of industrial raw materials, associated with recent discoveries and with the shift of its neighbor, the United States, to the position of a raw material importer. While the discoveries may seem to be accidental, it is legitimate to argue that with the growth of Canada's population density, with the shift of its population to the West, and with the expanding market for raw materials, the discoveries are largely a function of more intensive exploration and demand rather than accident. The other distinctive element is that revealed by the differentiation between primary and secondary manufacturing—

[6] For a discussion of the trends in the shares of industrial sectors in income and labor force see Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations: II. Industrial Distribution of National Product and Labor Force," *Econ. Develop. and Cult. Change*, Suppl. 5 (4), July 1957.

TABLE 2—STRUCTURE OF GROSS DOMESTIC PRODUCT (EXCLUSIVE OF RESIDENTIAL RENTS AND GDP ARISING IN THE ARMED SERVICES SECTOR) IN 1949 DOLLARS AND OF THE CIVILIAN EMPLOYED LABOR FORCE, AND RELATIVE INCOME PER WORKER, PAST AND PROJECTED

| | Percentage Change | | Percentage Shares or Relatives | | |
|---|---|---|---|---|---|
| | From 1927–29 to 1953–55 (1) | From 1953–55 to 1979–81 (2) | 1927–29 (3) | 1953–55 (4) | 1979–81 (5) |
| *A. Structure of Gross Domestic Product*[a] | | | | | |
| 1. Agriculture | 10 | 31 | 23.4 | 12.7 | 5.7 |
| 2. Resource industries[c] | 198 | 379 | 6.4 | 9.4 | 15.4 |
| 3. Primary manufacturing[d] | 178 | 196 | 5.3 | 7.2 | 7.2 |
| 4. Secondary manufacturing[e] | 173 | 232 | 16.5 | 22.3 | 25.3 |
| 5. Total manufacturing (3+4) | 174 | 224 | 21.8 | 29.5 | 32.5 |
| 6. Construction[f] | 124 | 159 | 5.7 | 6.3 | 5.6 |
| 7. Transportation, storage, and communication | 89 | n.a. | 8.9 | 8.3 | n.a. |
| 8. Trade, finance, and services[g] | 95 | n.a. | 24.8 | 23.6 | n.a. |
| 9. Total transportation and trade (7+8) | 93 | 200 | 33.7 | 31.9 | 32.8 |
| 10. Civilian government and community services[h] | 127 | 131 | 9.0 | 10.2 | 8.0 |
| 11. Total | 103 | 192 | 100.0 | 100.0 | 100.0 |
| *B. Structure of Civilian Employed Labor Force*[a] | | | | | |
| 1. Agriculture | −30 | −13 | 33.1 | 16.2 | 7.6 |
| 2. Resource industries[c] | 35 | 103 | 5.5 | 5.2 | 5.8 |
| 3. Primary manufacturing[d] | 62 | 60 | 5.1 | 5.8 | 5.0 |
| 4. Secondary manufacturing[e] | 90 | 82 | 15.0 | 20.0 | 19.8 |
| 5. Total manufacturing (3+4) | 83 | 77 | 20.1 | 25.8 | 24.8 |
| 6. Construction[f] | 101 | 78 | 4.8 | 6.7 | 6.5 |
| 7. Transportation, storage, and communication | 35 | n.a. | 8.2 | 7.7 | n.a. |
| 8. Trade, finance, and services[g] | 80 | n.a. | 21.1 | 26.5 | n.a. |
| 9. Total transportation and trade (7+8) | 67 | 116 | 29.3 | 34.2 | 40.4 |
| 10. Civilian government and community services[h] | 140 | 128 | 7.2 | 12.0 | 14.9 |
| 11. Total | 43 | 83 | 100.0 | 100.0 | 100.0 |
| C. *Gross Domestic Product per Employed Worker*[b] | | | | | |
| 1. Agriculture | 58 | 52 | 0.71 | 0.79 | 0.75 |
| 2. Resource industries[c] | 121 | 135 | 1.17 | 1.81 | 2.67 |
| 3. Primary manufacturing[d] | 72 | 85 | 1.03 | 1.24 | 1.44 |
| 4. Secondary manufacturing[e] | 44 | 83 | 1.10 | 1.11 | 1.28 |
| 5. Total manufacturing (3+4) | 50 | 83 | 1.09 | 1.14 | 1.31 |
| 6. Construction[f] | 12 | 46 | 1.20 | 0.94 | 0.86 |
| 7. Transportation, storage, and communication | 41 | n.a. | 1.08 | 1.07 | n.a. |
| 8. Trade, finance, and services[g] | 8 | n.a. | 1.17 | 0.89 | n.a. |
| 9. Total transportation and trade (7+8) | 16 | 39 | 1.15 | 0.93 | 0.81 |
| 10. Civilian government and community services[h] | − 5 | 1 | 1.27 | 0.85 | 0.54 |
| 11. Total | 42 | 59 | 1.00 | 1.00 | 1.00 |

*(Footnotes at bottom of page 371)*

a valuable distinction not ordinarily made in the literature. While the shares of both in national product grew from 1927-29 to 1953-55, only the share of secondary manufacturing is expected to continue to grow to 1979-81—implying a shift within manufacturing in favor of the secondary industries with a similar but more limited shift within the labor force employed in manufacturing.

While the summary of the past record and the projections in Panels A and B of Table 2 suggest plausible trends in the structure of gross domestic product and the employed labor force, those in gross domestic product per worker in Panel C raise some questions. In some sectors, the projected rise in product per worker is not too different from that found over the past 26 years: e.g., in agriculture, with its 50 to 60 per cent rise both in the past and in the projected span to 1980; in the resource industries, with their more than doubling of product per worker, both from 1927-29 to 1953-55 and from the latter date to 1980; and in primary manufacturing, with its rise in product per worker by about three-quarters over the last 26 years and more in the span to 1980. In all these cases, the past 2½ decades have witnessed impressive rises in product per worker despite the depression and the war, and perhaps partly because of them; and the projection of these or only slightly higher rates to 1980 does not raise obvious questions.

But in some sectors—secondary manufacturing, construction, and the combined group of transportation and trade—the projections involve a marked acceleration of the rate of growth of product per worker. It may help to indicate the basis for these projections. For secondary manufacturing the projections anticipate an "annual rise in output per man-hour of close to 3¼%" (monograph 16, p. 183), and the productivity assumptions have "been based on three sources, roughly in order of importance: (a) The performance of each industry since the war with particular importance attached to its record since 1949; (b) Guesses about future productivity in the studies and submissions; (c) The long-term trends, 1926-1955" (*ibid.*, p. 186). For construction "the

[a] Panels A and B from monograph 2, Tables 7.2 and 7.1, pp. 315 and 311.

[b] Panel C derived by dividing entries in Table 7.2 by those in Table 7.1 and calculating the appropriate percentage changes and sector relatives to the countrywide gross domestic product per employed worker.

[c] Resource industries include forestry, fishing, mining, quarrying and oil wells, and central electric stations.

[d] Primary manufacturing includes industries involving relatively minor processing of domestic resources—canning and processing, dairy products, grain mill products, meat products, saw and planing mill products, pulp and paper, nonferrous metal smelting and refining, abrasives, cement, and primary chemicals.

[e] Secondary manufacturing includes all other manufacturing industries involving more elaborate processing and often using imported raw materials or parts.

[f] Construction includes contract construction only; excludes own-account work done by private units and government departments not primarily engaged in the construction industry.

[g] Trade, finance, and services includes wholesale and retail trade; finance, insurance, and real estate; and business, personal, and recreation services.

[h] Civilian government and community services includes, in addition to government, educational, health, religious, welfare, and other community services.

nature of the industry will prevent any dramatic increase in productivity" but in view of preceding discussion which "outlined many reasons for believing that the industry will become increasingly dynamic and progressive . . . an increase of approximately 2% per annum in construction productivity is probably the best guess that can be made for the quarter century between now and 1980" (monograph 22, p. 214). The authors then add that "this is well above the long-term average of the past, although below the higher rates achieved in the special circumstances of the last few years" (*ibid.*, p. 214). Clearly, in these two major sectors the basis for projecting higher rates of growth in product per worker is largely the experience of the few years after the second world war; and the comments in the preceding section, concerning the tenuousness of extrapolating from so short a period, apply here.

But in the trade, finance, government, and service sectors results are even more puzzling. If we set the share in the labor force of the transportation, storage, and communication sector at 7.7 per cent in 1979-81 (i.e., at its 1953-55 level in the table), and assume that its product per worker at that date would equal 1.07 (the level for 1953-55), its share in gross domestic product in 1980 would be 8.3 per cent. This would leave a share in gross domestic product for trade, finance, and service in 1980 of 24.5 per cent, a share in the civilian employed labor force of 32.7 per cent, and a product per worker relative to the countrywide product per worker of 0.75. Yet the implied rise in product per worker from 1953-55 to 1979-81 would be 34 per cent, compared with an 8 per cent rise from 1927-29 to 1953-55.

To be sure, the shares of some service sectors in the table, particularly those for the combined group of transportation and trade (line 9), are residuals derived from the total projection for business by subtracting those for the other specific industrial divisions. But disregarding for present purposes the exact nature of these projected shares, one may properly raise two sets of questions. First, what meaning can be attached to constant price estimates of total product, and hence of product per worker, in the whole range of service industries? This question is sharpened by the findings that the product per employed worker in government and community services declined and that in trade, finance, and other services barely rose from 1927-29 to 1953-55. To what extent have these results been due to shifts within each sector (presumably from high product per worker to low product per worker segments)? To what extent have they been due to the fact that, with general price rises over the period, the groups in question did not possess the bargaining power to secure rises in their income that would have at least allowed for whatever increase in real product did in fact occur? To what extent has the lag in property incomes, particularly those with fixed or sluggishly changing returns, reduced total product and product per worker in the trade and finance sector? If these questions are relevant, the measures of product are much affected either by large internal shifts or by changes in bargaining power under conditions of sustained and large price rises and full employment, or by both.

Given these, the second set of questions follows. If the movement of gross domestic product per worker from 1927-29 to 1953-55 in the service sectors, and perhaps also in trade and finance, has been due in substantial part to

internal shifts and/or inflation and resulting changes in the bargaining power of various income recipient groups, should the projection extrapolate this trend into the future—as was done for the government and community services sector, if not for trade, finance, and other services? Can one assume the same internal shifts? Can one assume that at a constant per worker product—which presumably means little rise in per worker real income—there will be a flow of labor into the government and community services sector which will raise the share, as projected, from 12 to 15 per cent of the total employed labor force? How is this projection reconciled with the increased weight of educational and health services in the government and community services sector and with the need for and advocacy of higher rates of return in education and health to attract the necessary human resources? Is it reasonable to expect that trends will be allowed to continue which would by 1980 bring per worker product, as a relative of countrywide per worker product, as low as 0.54 in government and community services, and as low as 0.75 in trade, finance, and other services? This last implication could be checked only if we had a distribution of total product for each sector between that flowing to the workers engaged in it and that flowing elsewhere; and it is regrettable that no such distributions are available in the monographs. But it seems clear that the implied movement of product per worker in the trade, finance, and service sectors raises puzzling questions that cast some doubt upon the validity of the projections.

With respect to this large segment of the country's economy, the inquiry under review, like almost all similar studies for developed economies, faces difficult problems of measurement. Such problems are serious enough for the commodity-producing and transporting segments of the economy; but for them we have quantity volumes that, for all the qualifications involved in measuring quality and the effects of changing weights, do permit some acceptable approximation to volumes in constant prices. No such bases are available for the services segment of the economy, unless the compensation of factors is treated as a measure of output on the possibly unrealistic assumption that the markets in themselves assure its consonance with marginal productivity and hence with output; or unless the volume of services is derived as a direct function of the volume of commodities handled by the economy. In the former case, the results are likely to reflect internal shifts and the bargaining weakness of the factors involved under conditions of price rises and full employment; in the latter case, the share of services in national product remains relatively constant in the long run, and independent measurement of the product of a sector that accounts for an increasing share of the total labor force is sacrificed.

These comments are made not because another answer or treatment can easily be suggested, but rather to stress the problem and the need for a more detailed and incisive approach, which would at least try to probe the difficulties and experiment with some alternative solutions. As matters stand, the whole cast of the inquiry reflects the emphasis put on the commodity-producing and transporting sectors of the economy and the relative neglect of the others. Of the 18 monographs in the list, just one, monograph 24, deals with the service industries—although it is full of valuable information and analysis.

Yet by 1953-55 at least a third of gross domestic product, and close to four-tenths of the labor force, are accounted for by the trade and service sectors; and in the projections for 1980 these shares are a third of product and close to half of the labor force.

### III. *Foreign Trade and Capital*

With its relatively small population, abundant natural resources, and emphasis on individual business enterprise, Canada favors the kind of productive specialization that calls for extensive foreign trade as a complement and provides opportunities for foreign capital investment. Indeed, with the ratios of imports and exports to gross national product respectively about a quarter, and with long-term foreign capital amounting in 1955 to almost half of gross national product (and, assuming a capital-output ratio of 3 to 1, presumably a sixth of its total reproducible capital), Canada is most intensively engaged in the network of international trade and capital investment.

Table 3 presents the more important data and projections provided in monographs 25, 26, and 29 by Slater, Anderson, Brecher, and Reisman. The historical perspective here, at least for the over-all aggregates, is somewhat longer than in the other monographs, extending back to the beginning of the century; and the major trends are clearly discernible. The findings can be briefly summarized in three statements.

First, the projected trends, in continuation of those observed in the past, are toward a smaller relative weight of imports and exports in gross national product, and toward a lesser contribution of foreign capital investment. Within foreign trade in commodities, the shift in imports is away from food, clothing, and even some consumers' durables toward machinery and equipment; and the shift in exports is away from agricultural, animal, and forest products toward petroleum, minerals, and metals.

Second, the United Kingdom accounted for declining shares and the United States for rising shares in Canada's imports and exports, and in foreign capital invested in Canada. The growing share of the United States in foreign investment in Canada was accompanied by a shift from portfolio to direct investment: in 1926 portfolio investment was still 66 per cent of the total and direct investment only 30; by 1955, the share of the former declined to 38 per cent and that of the latter rose to 57 per cent (monograph 29, Table 18, p. 91). There was a related shift from debt to equity: the share of the former declined from 57 per cent in 1926 to 30 in 1955, whereas that of the latter rose from 43 to 70 per cent (*ibid.*, Table 19, p. 92).

Third, the projections carry forward the trends just observed in the shares of the United Kingdom and the United States, although at diminished rates. By 1980, the United States would, according to the projections, account for three-quarters of Canada's imports, seven-tenths of its exports, and well over eight-tenths of total foreign investment in Canada. There would presumably also be associated further shifts from portfolio to direct investment and from debt to equity.

Monograph 29 by Brecher and Reisman is devoted to the special problems of economic relations between Canada and the United States. One topic cov-

ered is the role of this country as foreign investor in Canada, some aspects of which have already been indicated, and it is further amplified by data on ownership and degree of control (the latter for United States direct investment corporations in Canada, with majority ownership by stockholders in the United States, including a few known to be controlled by parent firms in the United States, although with less than 50 per cent ownership of stock by nonresidents). For the branches of the Canadian economy with large amounts of foreign capital (manufacturing, petroleum, mining and smelting, railroads, other public utilities, merchandising, and construction), the share owned by all nonresidents declined from 37 per cent in 1926 to 32 per cent in 1954, but the share owned by United States residents rose from 19 to 25 per cent. The share *controlled* by nonresidents rose from 17 per cent in 1926 to 28 per cent in 1954, and almost all of the rise is accounted for by the United States, whose share rose from 15 to 25 per cent (monograph 29, Tables 24 and 25, pp. 100-101; the shares are of book value of capital invested).

The ratios for more narrowly defined branches are more significant. By 1955 nonresidents controlled 54 per cent of investment in Canadian manufacturing, of which United States controlled 45 per cent, compared with 35 and 30 per cent respectively in 1926. By 1955, nonresidents controlled 59 per cent of investment in mining, smelting, and petroleum exploration and development, of which the United States controlled 57 per cent, compared with 38 and 32 per cent respectively in 1926. Finally, "In a number of important Canadian industries—including oil and gas, nickel, aluminum, asbestos, iron ore, automobile, electrical, rubber and chemicals—a few enterprises owned and controlled by United States residents account for a preponderant share of total investment, output and employment in these industries" (*ibid.*, p. 155).

In addition to capital investment, ownership, and control, the monograph discusses other aspects of economic relations between Canada and the United States: the sensitivity of Canada's economy to business cycles in this country; the effects of United States tariff policy and of its agricultural policy; the trade union links between Canada and the United States; and concludes with a comparison of the economic growth of the two countries. The findings are not unexpected: the Canadian economy is sensitive to cyclical movements in the United States; the tariff and agricultural policies of this country have important repercussions in Canada, and while the trend toward lower tariffs and easier administration of customs is helpful, the "over-all impact of United States agricultural policy has likely been to restrict total farm output in Canada and to distort the pattern of Canadian agricultural production and exports" (*ibid.*, p. 194); "the trade-union movement in the United States has exerted a powerful and continuous influence on Canadian organized labor [but] Canadian autonomy is particularly strong in the key areas of collective bargaining, including the use of the strike weapon" (*ibid.*, p. 220); for the last 30 years (1926 to 1955) Canada grew at a somewhat higher rate than the United States and, if we compare projections (for the United States essentially that of the Paley Commission raised by 10 per cent to allow for an upward revision of population forecasts), Canada's prospective growth is likely to

TABLE 5—TRENDS IN AND PROJECTIONS OF FOREIGN TRADE AND INVESTMENT[a]

| | Past | | | Projection |
|---|---|---|---|---|
| | Beginning of Century (1) | 1920's (2) | 1950's (3) | 1980 (4) |
| **Ratio of Foreign Trade and Investment to G.N.P. (percentages)** | | | | |
| 1. Total imports | 31.2 (1901-15) | 29.0 (1921-29) | 23.9 (1952-56) | 19.7 |
| 2. Total exports | 21.3 (1901-15) | 28.6 (1921-29) | 21.7 (1952-56) | 18.4 |
| 3. Net foreign investment | 9.9 (1901-15) | 0.4 (1921-29) | 2.2 (1952-56) | 1.3 |
| **Percentage Share of Commodities** | | | | |
| 4. In total imports | | 65.6 (1927-29) | 71.3 (1953-55) | 69.8 |
| 5. In total exports (inc. gold) | | 77.2 (1928) | 76.8 (1955) | 76.8 |
| **Percentage Shares of Selected Groups in Merchandise Imports (1955 in col. 3 throughout)** | | | | |
| 6. Food, tobacco, and alcoholic beverages | | | 12.0 | 10.2 |
| 7. Clothing, textiles, leather | | | 12.1 | 10.8 |
| 8. Furniture, appliances, and misc. manufactured consumer goods | | | 11.0 | 13.8 |
| 9. Machinery and equipment | | | 20.2 | 27.7 |
| 10. Petroleum | | | 8.2 | 5.3 |
| 11. Chemicals | | | 4.1 | 6.7 |
| 12. Misc. industrial materials and cap. goods | | | 14.6 | 14.4 |
| **Percentage Shares of Selected Groups in Merchandise Exports (1955 in col. 3 throughout)** | | | | |
| 13. Wheat and wheat flour | | | 9.5 | 4.3 |

[a] Years to which estimates refer are shown in parentheses.

*Sources:*

Lines 1-3: columns 1-3 from monograph 25, Table 19, p. 70. The absolute amounts were added for the periods, and the percentages calculated from the totals. Column 4 from *ibid.,* Table 59, p. 164.

Line 4: columns 2 and 3 from monograph 25, Table 3, p. 27; column 4 from *ibid.,* Table 58, p. 161.

Line 5: columns 2 and 3 from monograph 26, Appendix A, p. 299; column 4 from *ibid.,* Table 4, p. 119.

Lines 6-12: monograph 25, Table 32, p. 97.

TABLE 3—(*Continued*)

| | | | | |
|---|---|---|---|---|
| 14. Other agricultural and animal products | | | 12.2 | 6.9 |
| 15. Lumber | | | 8.8 | 5.7 |
| 16. Newsprint | | | 15.3 | 11.8 |
| 17. Woodpulp | | | 6.8 | 6.2 |
| 18. Petroleum and products | | | 0.9 | 13.7 |
| 19. Aluminum and products | | | 4.9 | 9.5 |
| 20. Copper products and nickel | | | 9.0 | 6.2 |
| 21. Chemicals (ex. uranium) | | | 4.3 | 5.7 |
| Percentage Shares of U.K. and U. S. in Foreign Trade and Investment | | | | |
| Shares in Merchandise Imports | | | | |
| 22. U.K. | 24.0 (1900, 1912) | 15.0 (1929) | 9.5 (1953–55) | |
| 23. U. S. (same years as line 22) | 61.3 | 68.8 | 73.0 | |
| Shares in Total Imports | | | | |
| 24. U.K. | | | 9.4 (1955) | 10.6 |
| 25. U. S. | | | 73.4 (1955) | 76.2 |
| Shares in Merchandise Exports | | | | |
| 26. U.K. | | 22.2 (1926–29) | 17.1 (1952–55) | |
| 27. U. S. (same years as line 26) | | 38.3 | 57.9 | |
| Shares in Total Exports | | | | |
| 28. U.K. | | | 17.2 (1955) | 14.2 |
| 29. U. S. | | | 62.1 (1955) | 69.5 |
| Shares in Total Foreign Capital Investment | | | | |
| 30. U.K. | 85 (1900) | 44 (1926) | 17 (1955) | |
| 31. U. S. | 14 (1900) | 53 (1926) | 77( 1955) | |

Lines 22-23: monograph 25, Table 6, p. 29. The entries in column 1 are arithmetic means of the ratios for fiscal 1900 and fiscal 1912; in column 3 of the ratios for each calendar year included.

Lines 24-25: monograph 25, Table 60, p. 164.

Lines 26-27: monograph 26, Table III, pp. 314-18. The absolute amounts were added for the periods, and the percentages calculated from the totals.

Lines 28-29: see source for lines 24–25.

Lines 30-31: monograph 29, Table 16, p. 88.

›ntinue at a somewhat higher rate, further reducing the gap in per capita ıd per worker income between the two countries. Expected as the findings 'e, it is of value to have a systematic, and at many points detailed and ıantitatively tested, treatment of all these aspects of economic relations be- reen Canada and the United States—a treatment that assembles new data ı several topics and breaks new ground.

In much of the discussion of Canada-United States relations, particularly the *Final Report*, there is a conflict between apprehension over the recently creased "dominance" of the United States in the foreign trade of Canada, ; foreign capital, and some of its important industries, and recognition that ese ties with its larger neighbor to the south are an important and increasing urce of economic strength, a basis for past growth and a promise for the ture which it would be irrational to forego. The discussion recognizes that e shift in the character and identity of Canada's main creditors and trading rtners is in large part a result of the country's greater maturity, attained th the completion of its extensive fixed-capital network (exemplified par- :ularly by the railroads)—which could easily be financed by portfolio urces from areas other than the United States; that in a sense the greater volvement in trade and capital flows with the United States is evidence of 'ength which permits Canada to take advantage of its opportunities vis-à-vis is country as it could not do through the 19th and early 20th centuries.

Yet there is apprehension lest the control of important industrial sectors Canada by large parent companies in the United States lead to policies ricing, marketing, production, research, etc.) not as beneficial to Canada's ›nomy, either in the short or long run, as those of domestically controlled erprises might be; lest the increased dependence upon United States mar- s, for imports and particularly for exports, make Canada more sensitive to vagaries of United States economic policy; lest the ties with international ›or unions in the United States impose policies (with respect to wage rates, ıditions of work, etc.) that are not as beneficial to Canada as they might if the unions were domestically centered. And definite recommendations are de for greater employment of Canadians by foreign-controlled corporations high managerial and professional positions, of wider publicity of accounts 1 clear presentation of the distinguishable results of operations within nada, of greater participation on boards of directors by "independent" nadians, and of the sale to Canadian interests of substantial proportions of ıity shares (about 20 to 25 per cent, see *Final Report*, p. 393); as well as changes in Canadian tax laws and other regulations to make investment equity capital more attractive to Canadians.

While it is difficult to weigh the elements of apprehension and economic tification in this mixed attitude toward Canada's economic relations with United States, one can fairly characterize it as cautious; or perhaps even ter, as responsive (although not explicitly) to noneconomic factors and ce reluctant to accept fully the economic arguments—particularly when y cannot be thoroughly tested (which is usually the case). This attitude erges most clearly in connection with Young's monograph 27, on Canada's ımercial policy, which presents a clearly and cogently reasoned case for

free trade. Its main argument, in the author's words, is: "In general and over the long run, increases in protection can be expected to lead to economic losses and decreases in protection to economic gain for the country as a whole. This follows not only from the direct effect the Canadian tariff has on the Canadian economy, but also from the effect Canadian commercial policy has on the treatment accorded this country's exports" (p. 160). In its brief introduction, the Commission is somewhat apologetic about Young's "more abstract case for free trade . . . than perhaps some people would expect or think justified in a staff study for a Royal Commission" and then adds: "Understandably, the Commissioners have been more concerned with tariff and commercial policy in the light of the existing structure of the Canadian economy under the conditions and circumstances of today and those which we foresee in the future than we have been with theories which in themselves involve certain assumptions and preconceptions and which are, therefore, subject to different interpretations when applied in practice" (*ibid.*, front page). In the *Final Report* the Commission concludes that it would not be wise for Canada to embark upon any general program of tariff reductions on a unilateral basis, while "the impetus which has been given by the United States to a policy of freer trade is now virtually exhausted" (p. 440).

The difficulty in accepting the position indicated—in respect to commercial policy, foreign involvement, and other aspects of relations with other countries—does not lie in what may to some seem to be excessive caution, and to others excessive nationalism. Given the variety of noneconomic factors —political, sociological, cultural—that play on decisions concerning international economic relations, to claim the dominance of the economic argument and analysis is hardly warranted (and Young, incidentally, does *not* do so). The difficulty is rather that the references to these other factors are so general and vague that no firm ground for analysis and testable judgment is provided. What is meant by the "light of the existing structure of the Canadian economy"? Does it mean that no changes are desirable; or that rapid changes are undesirable, and if so how rapid? Given the account in monograph 29 of Canada's enterprises "controlled" from abroad, which reveals no serious defects in policy from the standpoint of Canada's economic growth, what specific useful purpose would be achieved by a greater share of Canadian equity holdings in such enterprises? This is not to say that there are no good answers to these questions; the point is rather that none have been given. A more explicit consideration of at least some of the noneconomic factors that lurk behind the scene might have helped.

At the risk of unwarranted speculation, one might suggest that greater economic "dependence" of Canada upon the United States, a neighbor over ten times its size, whose political decisions and cultural patterns may often not be to the taste of the Canadian community, despite obvious economic gains, involves some psychological costs. The relevant factors are cultural, socio-psychological, and political, rather than economic. Yet it is not impossible to note them explicitly, and even consider some uses of economic resources that could be expected to compensate by encouraging cultural and political leadership in Canada in respects and directions in which the United

States may be deficient. Of course, the Commission, having been instructed to deal with *economic* prospects of Canada, may have considered itself barred from discussing these noneconomic forces and factors. Yet in the case of international economic relations this limitation may be a serious disadvantage; and the refusal to deal more explicitly with the noneconomic factors may encourage a greater unwillingness to accept the purely economic arguments than might otherwise be the case.

### IV. *Concluding Comments*

Several complexes of topics have been referred to only incidentally in the Commission's inquiry, and have not been treated with the fullness that they warrant.[7] The first is the mechanism, immediate determinants, and prospective implications of the substantial price rise that occurred in the years following the second world war. A rise of 60 per cent over a decade, with the differential impact that it must have had on the various groups in the community and sectors in the economy, accompanied by the rise in output and productivity that provides the basis for so much extrapolation into the future, would seem to call for full-scale analysis and for more detailed consideration of policy tools. There is a brief discussion in the *Final Report* (pp. 428-33) of the means of dealing with inflation, emphasizing the primacy of monetary measures in combination with fiscal policy and some selective controls; but the discussion is too brief and general. There is no systematic study of the price movements, over-all and differential; no full-scale analysis of their determinants or mechanism; and, as already indicated, little attention is paid to price prospects in the projections.

Second, while extensive discussion of the distribution of national product, labor force, and capital by industrial source and attachment is provided, almost no attention is paid to the distribution of incomes by type, among various occupational-industrial-economic status groups, and among different size-of-income groups. Granted that the basic data may be inadequate in Canada, as they are in the United States and other countries, yet Canadian national accounts and other sources contain sufficient data—on the distribution of income by type (compensation of employees, income of individual entrepreneurs, property incomes, etc.) combined with the industrial distribution—to shed

[7] After this review was completed, William C. Hood kindly supplied proofs of the one still unpublished monograph (No. 3 in the Appendix). The monograph is partly a detailed review of the financing of economic activity in Canada during the period from 1946 through 1956—with only the broader aggregates for 1955 and 1956; partly an appraisal of the operation of the capital market during the same period. The basic data are a set of money-flow estimates (called national transaction accounts), designed as a supplement to the national income accounts. The resulting matrix is highly elaborate, and the tables that provide the annual entries for the cells from 1946 through 1954 are a rich mine that could be quarried for years. Part II summarizes the conceptual structure of the accounts and reviews the evidence on financing for the economy as a whole for four separate periods: 1946-48, 1949-51, 1952-54, and 1955-56. Part III deals with financing of individual sectors—consumer finance and business finance (including agriculture, small business, and nonfinancial corporations); Part IV, with financial institutions (life insurance companies and other selected intermediaries, the banking system and the money market).

This major study will be reviewed separately in a forthcoming issue of the *Review*.

some light on the flow of incomes to various economic groups in the com munity. The combination of data on personal income taxes with those o flow of personal income to individuals and households and information prc vided by recent sample studies of distribution of income among nonfarn families should make possible some measure of the levels of and changes i the shares of upper-income groups. It need hardly be emphasized that in free society, the levels of and changes in relative compensation of factors an groups in the community affect the relative supply as well as the structure c resources; and the neglect of explicit study of changes in relative compensa tion, past and prospective, seriously limits both the analysis of the past an the projections.

Third, no systematic and detailed analysis of government revenues an expenditures is provided. In its *Final Report,* the Commission indicates tha it deliberately omitted consideration of "the whole question of the cor tinuing relationship of the federal, provincial and municipal levels of goverr ment, and the tax revenues which each should have in order adequately t carry out its responsibilities," and explains the omission as due to the desin not to conflict with the study of this topic "by the officials of the respectiv governments preparatory to a Federal-Provincial Conference on the subject (p. 455). Whatever the reason, there is no study of government revenue co lections and other means of financing government activity, and their possib impact upon production, spending, and saving incentives in the private secto nor is there any analysis of government expenditures, and their possible cor tributions to the various groups in the community or to the various sectors ( the economy. The sheer quantitative weight of government activity, so sul stantially increased in recent decades—as evidenced by a rise in the shar of government purchases of goods in gross national product from about 1 per cent in 1927-29 to 18 per cent in 1953-55, and projected to almost 17 p cent in 1979-81 (monograph 2, Table 7.4, p. 318), seems to warrant detailed and explicit analysis.

Fourth, and perhaps most important, no systematic comparison of suppl with needs, of growth with what growth is supposed to serve, has been a tempted—either in the analysis of the past record or in the projections (e cept in the few cases, like that of social capital, where projections we directly based on needs). Yet it may legitimately be asked whether the growt that occurred in the past fell short of apparent needs in some specific areas ( in some broad sectors; and whether the projected growth, if it is realize will be a tolerable approximation of supplies to the range of the needs of tl community as reasonably envisaged. Without such a comparison, one naturall wonders what growth is for, and whether the frequently cited statistical ra represents *adequate* growth for the major types of individual and collectiv needs.

Associated with these omissions, and also in part with the basic assumptio underlying the projections, is an impression of a kind of "problemlessness Presumably, one purpose of the inquiry, in appraising Canada's prospects ( growth, was to ascertain whether there were any major obstacles to growth what might be considered satisfactory rates and with satisfactory pattern

and to explore policies to deal with such problems. The *Final Report* gives the impression that no serious problems exist; and policy recommendations are specific on minor issues but are rather general when they touch upon a major problem like inflation. To be sure, there are recommendations for: an energy commission to guide policies in that field; an accelerated depreciation rate and other taxation measures to induce greater investment of domestic savings; changes in rules governing investment by financial intermediaries to assure better performance of capital markets; greater "integration" of foreign-controlled corporations, by measures already noted above; more imaginative treatment of the problems of growing urban conglomerates; raising the pay scales in education and other community services; immigration, particularly of skilled labor; straightening out some anomalies in tariff structure and administration. But all these seem minor, in comparison with some major problems that have been ignored.

The gravest problem of Western civilization, the danger of an all-out conflict, is removed by a general caveat; and the related potential problem, of the possible burden of defense outlay on the economy, is also removed by the more specific assumption which points to a government defense expenditure by 1979-81 of less than 4 per cent of gross national product, compared with 7 per cent in 1953-55 (monograph 2, Table 7.4, p. 318). Moreover, the share of government nondefense expenditures is assumed to rise from 11 per cent of gross national product in 1953-55 to only 13 per cent in 1980. The problem of inflation is, as already suggested, largely omitted from the analysis of the past, and removed by assumption from the projections. Any problems connected with income distribution among various groups in the country are, also as indicated, omitted from the analysis and not explicitly brought into the projections. And without systematic comparison of needs and supplies, the adequacy of past growth cannot be tested nor can any problems connected with the failure to supply some needs in the future be discussed. All that we are left with are essentially production problems, in the narrow sense of the word; and given the backlog of useful knowledge, the capacity for accelerated increase in the future, Canada's endowment with natural resources as presently seen, and some plausible growth in the United States, these are bound to be relatively minor.

These observations are not intended to be critical of the Commission's inquiry. They reflect the true situation, viz., that in rich developed countries (like Canada, the United States, and a number of others) the economic problems raised by consideration of growth prospects in terms of supply of factors and demand for products are relatively minor. The major problems are in the realm of international relations—with the Communist and underdeveloped countries; in control of inflation and its impact on the differential position of various groups; and in the use of the government sector, not only for the short run (which is not directly considered in the inquiry) but also to offset any possible *specific* deficiencies of the market mechanism and of the private sector in supplying commodities and services to satisfy some socially important needs.

The comments in this review are perhaps too demanding in their emphasis

on the omissions in the inquiry; and may perhaps understate the value of the analysis and the discussion in the *Final Report* and in the monographs. I would be the first to regret it if the review left this impression, and failed to convey an appreciation of the usefulness of the Commission's published results as a reference series of wide scope and as a valuable attempt at a systematic translation of the analyzed record of the past into a projected, if oversimplified, future.

Two comments are appropriate in conclusion. First, the period over which the studies were completed was very short. The Commission was set up in mid-1955, most of the monographs appeared by early 1957 after a period of about a year and a half, and all but one and the *Final Report* were published by the end of 1957. This is an amazingly short period in which to prepare and publish a series of monographs so impressive in coverage and content. While the brevity of the period may account in part for some of the omissions noted above, it is positive proof of the remarkable production efficiency of this research project.

Second, much of the value of the Commission and its efforts lies in the systematization of knowledge that already exists about Canada's economy, and particularly in the spread of such knowledge over wider circles—a process of education. The enrichment of widely held economic knowledge, the provision of a more balanced framework within which policy decisions can be made, is potentially a more useful service—in purely "practical" terms—than any specific policy recommendations that may be made. And this service can remain effective and increase in value only if the effort is continued and extended. We hope that the present inquiry will be the first in a series, to be followed by others at not too distant intervals—in which the same and additional aspects of Canada's economy will be treated in systematic relation to each other; and that continued wide use of the results of the present inquiry will be made by scholars and students of economic affairs—a use these results richly deserve.

## APPENDIX

### Classified List of Publications of the Royal Commission on Canada's Economic Prospects

All publications are by Edmond Cloutier, Queen's Printer and Controller of Stationery, Hull or Ottawa. The dates of completion are indicated following each title, publication being in the same or following year. No prices are given.

*Preliminary Report*, Dec. 1956, 142 pp.
*Final Report*, Nov. 1957, 509 pp.

A. Studies of Basic Aggregates, Projections, and Components by Type of Use:

1. J. M. Smith, *Canadian Economic Growth and Development from 1939 to 1955*, May 1957, 80 pp.
2. W. C. Hood and Anthony Scott, *Output, Labour and Capital in the Canadian Economy*, Feb. 1957, 513 pp.

3. W. C. Hood, *Financing of Economic Activity in Canada*, including *A Presentation of National Transaction Accounts for Canada, 1946-1954*, by L. M. Read, S. J. Handfield-Jones and F. W. Emmerson, to be published in 1959, about 700 pp.
4. D. W. Slater, *Consumption Expenditures in Canada*, May 1957, xi + 198 pp.
5. Yves Dubé, J. E. Howes and D. L. McQueen, *Housing and Social Capital*, Jan. 1957, 164 pp.
6. R. D. Howland, *Some Regional Aspects of Canada's Economic Development*, Nov. 1957, xi + 302 pp.

B. Industry Studies:

7. W. M. Drummond and W. Mackenzie, *Progress and Prospects of Canadian Agriculture*, Jan. 1957, 424 pp.
8. John Davis, A. L. Best, P. E. Lachance, S. L. Pringle, J. M. Smith, D. A. Wilson, *The Outlook for the Canadian Forest Industries*, March 1957, 261 pp.
9. The Department of Fisheries of Canada and The Fisheries Research Board, *The Commercial Fisheries of Canada*, Sept. 1956, 193 pp.
10. John Davis, *Mining and Mineral Processing in Canada*, Oct. 1957, ix + 400 pp.
11. John Davis, *Canadian Energy Prospects*, March 1957, 392 pp.
12. Urwick, Currie Ltd., *The Nova Scotia Coal Industry*, June 1956, 34 pp.
13. The Bank of Nova Scotia (Dr. Lucy Morgan), *The Canadian Primary Iron and Steel Industry*, Oct. 1956, 104 pp.
14. National Industrial Conference Board (Canadian Office), *The Canadian Primary Textiles Industry*, July 1956, 105 pp.
15. John Davis, *The Canadian Chemical Industry*, March 1957, 182 pp.
16. D. H. Fullerton and H. A. Hampson, *Canadian Secondary Manufacturing Industry*, May 1957, 274 pp.
17. The Sun Life Assurance Company of Canada, *The Canadian Automotive Industry*, Sept. 1956, 119 pp.
18. J. D. Woods and Gordon Ltd., *The Canadian Agricultural Machinery Industry*, Apr. 1956, 47 pp.
19. Urwick, Currie Ltd., *The Canadian Industrial Machinery Industry*, Feb. 1956, 31 pp.
20. C. L. Barber, *The Canadian Electrical Manufacturing Industry*, Sept. 1956, 87 pp.
21. Canadian Business Service Ltd., *The Electronics Industry in Canada*, Apr. 1956, 81 pp.
22. The Royal Bank of Canada, *The Canadian Construction Industry*, Oct. 1956, 232 pp.
23. J.-C. Lessard, *Transportation in Canada*, Nov. 1956, 160 pp. (plus 29 pp. of statistical schedules, unnumbered)
24. The Bank of Montreal, *The Service Industries*, March 1956, 161 pp.

C. Studies of Foreign Trade, Investment, and Policy:

25. D. W. SLATER, *Canada's Imports,* Jan. 1957, 222 pp.
26. R. V. ANDERSON, *The Future of Canada's Export Trade,*[1] March 1957, 338 pp.
27. J. H. YOUNG, *Canadian Commercial Policy,*[1] Nov. 1957, 235 pp.
28. J. G. GLASSCO (of Clarkson, Gordon & Co.), *Certain Aspects of Taxation Relating to Investment in Canada by Non-Residents,* Feb. 1956, 64 pp.
29. IRVING BRECHER AND S. S. REISMAN, *Canada-United States Economic Relations,*[1] July 1957, 344 pp.

D. Other Studies:

30. The Canadian Bank of Commerce, *Industrial Concentration, A study of Industrial Patterns in the United States, the United Kingdom and Canada,* June 1956, 62 pp.
31. The Canadian Labour Congress, *Labour Mobility,* Sept. 1956, 11 pp.
32. The Canadian Labour Congress, *Probable Effects of Increasing Mechanization in Industry,* Sept. 1956, 87 pp.
33. The Economics and Research Branch, Dept. of Labour of Canada, *Skilled and Professional Manpower in Canada, 1945-1965,* July 1957, xiv + 106 pp.

[1] One of a series of three studies prepared under the direction of S. S. Reisman.

# COMMUNICATIONS

## Rent as a Measure of Welfare Change

The definitions of economic rent in current use fall easily into two categories: (1) a payment in excess of that necessary to maintain a resource in its current occupation. Thus, Frederick Benham [1, p. 227] tells us that rents are ". . . the sums paid to the factors which need not be paid in order to retain the factors *in the industry*." While to Kenneth Boulding [2, p. 230] it is the payment to a factor ". . . in excess of the minimum amount necessary to keep that factor in its present occupation." (2) The difference between the current earnings of a resource and its transfer earnings[1]—the latter term signifying its earnings in the next best alternative use [1, p. 328]. For instance, Paul Samuelson [6, p. 593] says, ". . . we should term the excess of his income above the alternative wage he could earn elsewhere as *a pure rent*." Similarly, for George Stigler [7, p. 99] the rent of a factor is ". . . the excess of its return in the best use over its possible return in other uses? . . ."[2]

While the first type of definition is, as we shall see, unavoidably ambiguous, the second type is yet more inadequate. Among other things it would require that, in the choice of occupation, men were motivated solely by pecuniary considerations.

### I. *A Measure of Rent as an Economic Surplus*

For the purpose of revealing ambiguities in the existing definitions of economic rent and of demonstrating the logic of the proposed definition, we shall find it no less convenient and a good deal more suggestive to take our bearings from a more generalized version of the traditional theory of consumer's choice.

Rather than maximizing the utility function $W\{u(x_1, \ldots, x_n)\}$, over the range in which $\frac{\partial W}{\partial x_r} > O$ for all $x_r$, subject to the usual constraint $\Sigma p_r x_r = Y$, where $Y$ is the individual's income [cf. 3, p. 305], we require our individual, in possession of given resources, or assets, to maximize such a function subject to $\Sigma p_r x_r = O$. At least one of the $x$'s is negative in order to indicate a quantity supplied per period by the individual of a good or service and, of course, at least one of the $x$'s is positive to indicate a quantity demanded per

[1] To impart precision to this measure of economic rent the period of adjustment should be specified, as should, also, the area of comparison—within the industry, region, country, or within the world as a whole. But since the inadequacy of this definition of rent prevails irrespective of these distinctions, I shall make no further mention of them in this paper.

[2] In all these cases the writers appear to be using "factor" in the sense in which I shall use the term "resource." And though, generally, I prefer to reserve the term factor for the productive service of the resource, it will avoid possible confusion if instead I adhere to the term productive service.

period of a good or service. The suggested constraint expresses nothing more than the proposition that, in all circumstances, the individual's current earnings are equal to the current value of his expenditure.[3] It is a significant amendment, however, because it brings to the fore the notion of simultaneous determination of the individual's allocation of his productive services and of his earnings in response to a given pattern of prices: an obvious point perhaps, but one frequently ignored in the analysis of the individual's demand and supply curves.

Maximizing the utility function subject to our new constraint, we derive the well-known equilibrium condition $\frac{\partial W}{\partial x_r} = \lambda p_r$ ($\lambda$ being identified as the marginal utility of income) for all goods and services whether their magnitudes are positive or negative—whether, that is, they are demanded or supplied by the individual.[4] Or, dispensing with utility, we can write $\frac{\partial x_i}{\partial x_j} = \frac{p_j}{p_i}$ for any $i$ and $j$.

It should be apparent that, although the substitution effect may be defined in the customary way, there can be no income effect, $\frac{\partial x_r}{\partial Y}$, since there is no necessary correspondence, using our new constraint, between changes in the individual's welfare and changes in his income, real or money. For with the new constraint, money income, Y, is no longer held constant; it is determined along with all the other variables. It may increase, remain unchanged, or diminish, with an improvement in the individual's welfare. In its place, therefore, we derive a *welfare* effect, $\frac{\partial x_r}{\partial W}$. In consequence, the effect on the quantity bought or sold of any chosen good or service of a given change in the set of prices is divided into a substitution effect and a welfare effect.

The implications of this less-restricted formulation, though straightforward enough, are worth recording. A change in the price of any good or service—whether it is supplied or demanded by the individual—changes, in general, the quantities of all goods and services which the individual buys and sells. Consequently it changes the value of his earnings and expenditure. A search for a useful definition of an "incentive good" might begin with the implication that a fall in the price of any consumed good will, *inter alia,* increase or reduce the amount of work done by the individual as a result of the operation of the welfare effect. But this will not be pursued here.

[3] Strictly speaking his spending is equal to current earnings *less* current saving *plus* current dissaving. This could easily be allowed for without any modification of our conclusions. Over time, if his assets grow, his demand for goods and his disposal of productive services will, of course, alter. This problem is, however, common to all such static analysis.

[4] Since we restrict ourselves to the range in which the marginal utilities of all goods and services are positive, the acquisition of goods or services adds to the individual's total utility while the supply of goods and services from the individual's assets or resources subtracts from his total utility. Corresponding to the equilibrium conditions for goods purchased, the marginal utilities of the productive services supplied to the market are proportional to their corresponding supply prices.

Having extended the customary confines of the theory of consumer's choice we may now develop the argument largely in terms of two or three goods or services, but deriving from our hypothesis a more symmetrical construction of the individual indifference map. Since $\Sigma p_r x_r = O$, the price hyperplane passes through the origin of an $n$-dimensional indifference map and is nega-
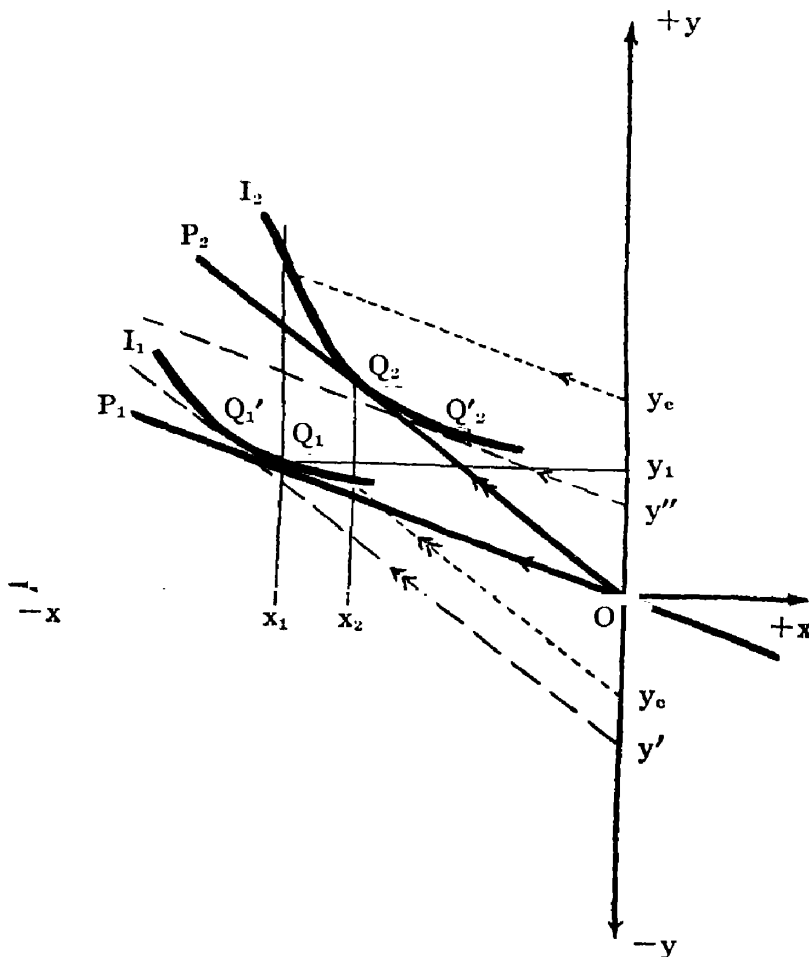
FIGURE 1

tive in slope with respect to all the axes. This means that in order to acquire (surrender) more of one good or service, other goods or services must be surrendered (or acquired).

A two-dimensional cross section of this indifference map is represented in Figure 1. Any distance $Ox$ to the right of the origin measures amount per unit period of $x$ acquired by the individual. Any distance $Ox$ to the left of the origin measures the amount of $x$ per period given up by the individual. Similarly, $Oy$ above the origin measures the quantity of $y$ taken, and $Oy$ below the origin, the quantity of $y$ given up. Inasmuch as rent partakes of the nature of

a surplus, and is to be measured in exactly the same way as consumer's surplus is measured, it is advantageous to consider in some detail the simple problem of the individual supplying $x$, say a single type of productive service, "labor," to the market in return for which $y$ is demanded. Thus, we operate in the north-west quadrant of the figure. And though we may not do so in an $n$-dimensional treatment of the problem, confined as we are to two dimensions we may find it convenient to regard $y$ as all other goods at fixed prices, the only price which alters being the price of $x$, labor.[5] We now seek a precise measure of the difference in welfare resulting from alternative supply prices of labor.

If we construct a price line $P_1$ passing through the origin and tangent to $Q_1$ on the indifference curve $I_1$, the individual is represented as in equilibrium, giving up $Ox_1$ of labor and acquiring in exchange $Oy_1$ of income y. We now perform the familiar Hicksian experiment in order to have the supply effects on all fours with those of demand. The price of $x$ is now increased from $p_1$ to $p_2$, the individual's new equilibrium being at $Q_2$ on the indifference curve $I_2$. The change in equilibrium positions consequent upon the change in the price of labor may be divided into the substitution effect, $Q_1$ to $Q_1'$, and the welfare effect, $Q_1'$ to $Q_2$ (or alternatively the welfare effect $Q_1$ to $Q_2'$ and the substitution effect $Q_2'$ to $Q_2$). Although the welfare effect can, of course, go either way, it should be noticed that a positive welfare effect on $x$, implying an increase in the *demand* for $x$, constitutes a reduction in its supply, which is to say that a positive or "normal" welfare effect of a rise in the supply price of labor, or in the supply price of any good or service, is that of a reduction in the quantity supplied by the individual. The "backward-bending" supply curve of labor is, then, the outcome of a strong positive, or normal, welfare effect, and not a negative, or perverse, welfare effect.

Suppose we are now to measure the increase in welfare following a rise in the price of $x$ to $p_2$, we may follow Hicks' practice [4, pp. 69-82] and distinguish between two preliminary measures: the compensating variation ($CV$), and the equivalent variation ($EV$). The $CV$ is the amount of $y$ which, following a change in the price of $x$, has to be given to or taken from the individual in order that his initial welfare—indicated by the indifference curve $I_1$ in Figure 1—remain unchanged. In this instance, the individual's welfare being

[5] This construction, and its later elaboration, are, I believe, to be preferred to the more common leisure-income diagram apart from the fact that the present diagram is derived directly from the more general condition in which the individual chooses to supply a combination of various goods and productive services to the market in amounts which depend upon the current set of prices: (1) Giving up leisure, a homogeneous good, does not have the same connotation as providing various kinds of services each of which requires a different skill and entails a different degree of hardship for the individual. (2) We need not evoke the artifice of a fixed amount of the good, leisure, say 24 hours a day, with the rather awkward result that an improvement in welfare may be represented along one axis as equivalent to more than 24 hours of leisure a day. In the construction used here, the shape of the indifference curves acts to limit the supply of any productive service furnished to the market, and our measure of welfare changes is in terms only of the good, $y$. Finally (3) the indifference map used here is the correct prior construction to that useful textbook diagram in which a downward-sloping line crosses the price-axis, to the right of which is represented the demand schedule and to the left, the supply schedule.

improved as a result of the price change, $Oy'$ measures the $CV$. For if $Oy'$ were taken from his income he could still maintain his initial welfare position on $I_1$, given that the higher supply price $P_2$ is available to him. The $EV$, on the other hand, is the amount of $y$ which has to be given to, or taken from, the individual to ensure that he reaches the new level of welfare when the change in price does not apply to him. Since in this instance the increment in welfare is positive he is to receive a money equivalent. If he receives $Oy''$ he can just reach $I_2$, the new level of welfare, with the old price $P_1$.

The concept of rent as an economic surplus, it is suggested here, should be measured as a $CV$ or an $EV$ in a manner symmetrical in all respects with the concept of consumer's surplus. In the example above, it arises as the difference in welfare experienced by the individual from the rise in the supply price to $P_2$, $P_1$ being regarded as the most preferred alternative open to him.[6] The rent obiviously becomes larger the lower the initial supply price $P_1$. In the limiting case, $P_1$ will be a no-transactions price tangent to an indifference curve at the point where it crosses the vertical axis.

Since the current definitions treat rent as a surplus which may be appropriated without any effects on the supply of the individual's productive services in his current occupation, it is important to observe that in all cases in which the individual is made to pay or to receive compensation equal to the measures of rent suggested, the amount of the productive service he will then offer will differ from that which he originally supplied at the current price. For example, if, having reached $Q_2$ in Figure 1, he is made to pay the full $CV$, equal to $Oy'$, he will no longer continue to supply $Ox_2$ of labor. Instead he will supply the amount indicated by the equilibrium point $Q_1'$—a larger amount than before if $x$ is normal.

Finally it may be instructive to remove the restriction of a single occupation in our analysis and to consider briefly the case of the supply of productive services to two alternative occupations, A and B, in which, although the individual might choose to work part-time in each if that were feasible, he is obliged, owing to institutional arrangements, to work entirely in the one occupation or the other.

In Figure 2, a three-dimensional indifference map with a vertical $y$-axis and two horizontal axes, $a$ and $b$, crossing at right angles, we cut a vertical slice along the negative $ay$ plane and along the negative $by$ plane as far as the

[6] Though we are working with a single productive service, labor, the notion and the definition of economic rent may, just as in the analysis of consumer's surplus, be extended to several services with obvious modifications. If, for example, the individual is providing two services, $x_1$ and $x_2$, then a rise in the supply price of both services yields a $CV$ rent which is the maximum he is willing to pay—prices of all goods and services other than those of $x_1$ and $x_2$ remaining unchanged—rather than forego these higher prices. This measure remains the same, as we might expect, if we measure each in turn and add them: the rent when the price of $x_1$ rises, all other prices, including that of $x_2$, being constant, *plus* the additional rent when now the price of $x_2$ rises, all other prices remaining constant with $x_1$ unchanged at its new price.

This argument is symmetrical with that of Hicks on consumer's surplus [4, pp. 178-79], but the generalization in the conclusion of this paper goes further than Hicks'.
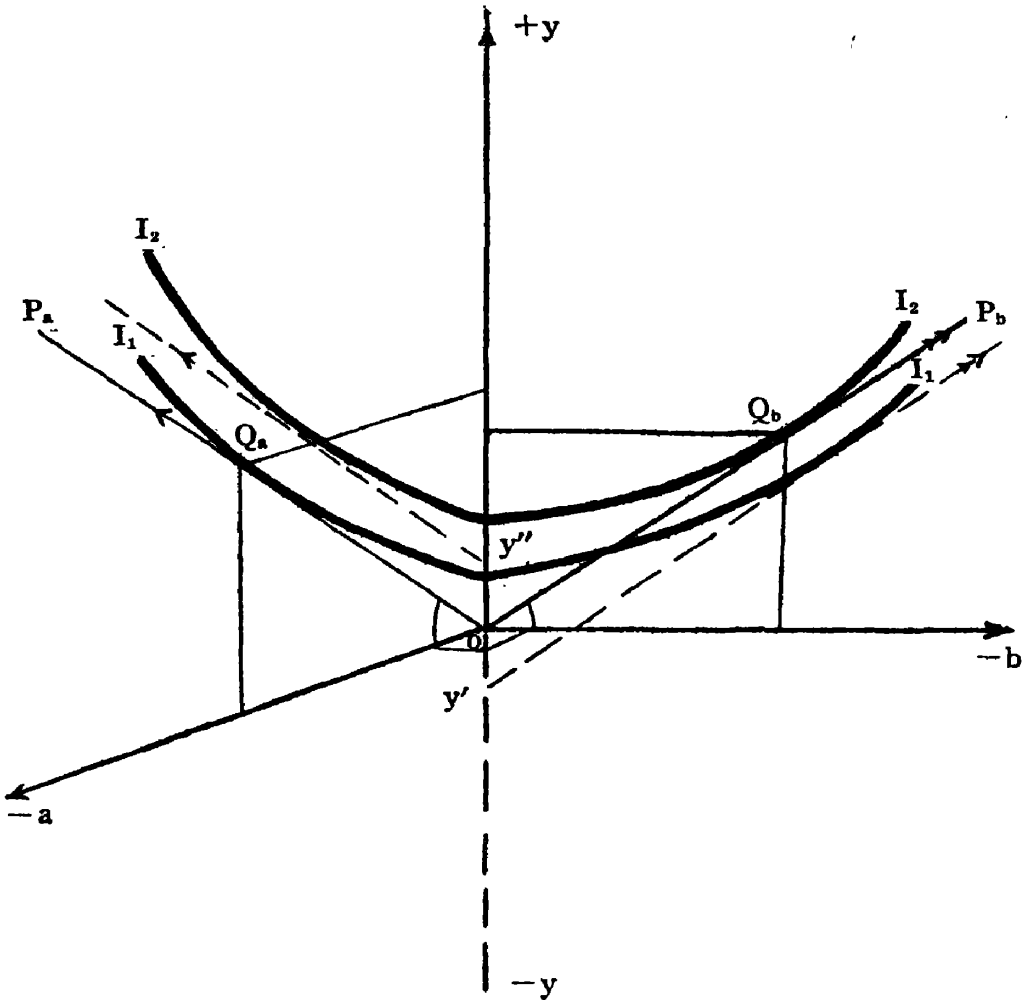
FIGURE 2

$y$-axis and remove the segment. Hence, if we imagine our figure divided vertically into four quarters, we shall be looking into the space left after the removal of the vertical quarter in which $a$ and $b$ are both negative. The upper part of what meets the eye is represented in Figure 2. By removing the vertical quarter referred to, we have removed the possibility of combining employment A and B.

Despite the fact that both the rate of pay and the resultant earnings are higher in A than in B, the individual chooses to supply his services to B, his equilibrium there being at $Q_b$ on the indifference surface $I_2$ compared with the alternative equilibrium position $Q_a$ on $I_1$. Nonetheless, he enjoys a positive economic rent in the lower-paid occupation B which can be measured by the $CV$, $Oy'$—the maximum he is prepared to pay to remain in B when A, at the

existing wage-rate, is the only alternative open to him. It can also be measured by the *EV*, $Oy''$—the minimum the individual must be paid in order to induce him to transfer his services from B to A.[7]

## II. *Comparison with the Marshallian Concept*

Let us now compare our results with Marshall's concept of rent. Though the *Principles* do not contain a formal definition of rent, the sense of most of Marshall's dicta on the subject points to a definition of rent as a surplus above that necessary to elicit the productive services of a resource.[8] This Marshallian definition, essentially that of category (1), suffers from the same imprecision as his definition of consumer's surplus.[9] For one thing, the surplus was treated as if it could be taxed away without affecting the supply of the productive service, which is manifestly false on our analysis. Once this is granted the difficulties are easily perceived. In order to persist with the Marshallian definition we have to interpret it to have reference to some unchanged amount of the productive service; either (a) the amount supplied in the equilibrium position resulting from the price change ($Ox_2$ in Figure 1), or (b) that supplied in the original equilibrium position ($Ox_1$ in Figure 1). In either case we are saddled with an improbable and cumbersome measure inasmuch as we have to compel the individual to supply an amount of the productive service other than that which he would freely choose. For instance, if we adopt (a) we derive what may be called, for convenience, the Marshallian *CV*, equal to $Oy_c$. It represents the maximum amount of money the individual would surrender in order to retain $P_2$ if at the same time he were constrained to provide no more than $Ox_2$ of productive service—$Ox_2$ being the amount supplied at $P_2$ when he is free to choose. As we should expect, the restriction on his choice of quantity reduces the maximum he is prepared to pay for the privilege of the $P_2$ price. In a like manner, if we adopt (b), the Marshallian *EV*, we are left with a measure $Oy_e$. It is larger than the *EV* proper, $Oy''$ since the minimum payment to him must be greater if he is now

[7] In all cases in which institutional arrangements preclude a combination of occupations —the individual having the choice only of putting the services of his resources entirely in A or in B—the coincidence of the four definitions and the Marshallian measure of rent no longer follow from a zero welfare elasticity, for the *EV* and the *CV* now arise from different cross-sections of the individual indifference map.

[8] In particular see pp. 155-62, and pp. 427-30 [5]. Elsewhere in the *Principles* Marshall talks of the additional earnings resulting from superior abilities as a surplus or rent [5, pp. 577-79 and 623-27]. What part of the additional earnings might be regarded as rent on the definition attributed to Marshall cannot be known without determining first what part of the additional earnings is necessary to attract the resource into that occupation. From the point of view of the firm, however, the additional payments for superior abilities must appear as efficiency payments.

[9] [5, p. 124] Marshall was, of course, aware of the slippery nature of his consumer's surplus (though not, apparently, of his economic rent) and tried to cover himself by specifying a change in the demand price for a particular good whose real-income effect was so small in relation to the individual's budget that the marginal utility of money income could be taken, for all practical purposes, as constant. The trouble with this is that it is double-edged: ambiguity is reduced by reducing the significance of what is being measured. Ambiguity disappears entirely only when the price change under consideration becomes zero and there is nothing left to measure.

compelled to provide the original quantity of productive services $Ox_1$ at the original price $P_1$ when his welfare is increased from $I_1$ to $I_2$. There is obviously nothing strictly illogical about such definitions, but on the grounds of plausibility and convenience they are to be rejected in favor of the *CV* and *EV* proper.

If, on the other hand, a Marshallian *measure* of economic rent is taken to be the area above the supply curve of the services of the individual's resource[10] —a measure which seems to correspond with the category (2) definition if the individual's supply curve represents maximum earnings of successive increments of productive services in alternative uses—for this measure to be of any use requires (i) an upward-sloping supply curve, and (ii) exclusion of non-pecuniary considerations. Clearly this Marshallian measure, which is popular in textbooks, is inadequate since it represents no more than a first derivative of the locus of price-quantity equilibria of an indifference map. Nor is this derivative necessarily upward-sloping; it may be backward-bending in contrast to the *marginal* indifference curves which will always be upward-sloping. It appears yet more unsatisfactory if the restriction to pecuniary considerations is removed. We may then discover that differences between the earnings of the resource in its current occupation and those of the relevant alternative occupation are negative, a tribute to the individual's preference for his present occupation.

Hicks has done some admirable work in tracing the relationships between Marshall's definition of consumer's surplus, Marshall's way of measuring consumer's surplus (the area under the individual's demand curve), and the two precise measures *CV* and *EV* which were initially suggested by his indifference curve analysis. Important as these contributions were in clarifying our ideas on this tangled subject, it can be held that the tracing of these precise relationships assumes a far greater importance on the neglected supply side. For it is surely just there that we cannot reasonably suppose that a change in price has negligible effects on the welfare of the individual inasmuch as the supply of any one of his productive services enters significantly into his budget. To the extent it does so, the area above the individual's supply curve, especially in the case of only one productive service, is a much less reliable index of the surplus welfare than the area under his demand curve for any one good.

In the special case in which the welfare elasticity of the supply of $x$ is zero there is a coincidence of the *CV*, *EV*, Marshallian *CV*, Marshallian *EV*, and the Marshallian measure, the area above the supply curve. (This same coincidence obtains when the rent is reckoned as between the current and alternative occupations in the case in which the choice between occupations rests on a purely pecuniary basis.) While in general, a zero elasticity of supply with

[10] [5, p. 811.] Here Marshall graphically illustrates consumer's surplus and producer's surplus for an *industry*. But even if we interpret the industry's supply curve as a marginal curve, the producer's surplus could be identified with the rent of resources in that industry only under restricted conditions. On the other hand, it would hardly be inconsistent with Marshall's view of things to interpret the measurement of the individual's rent in a manner symmetrical with his suggested measurement of the individual's consumer's surplus [5, pp. 125-27] as the area under the individual's demand curve.

respect to price does not entail a zero welfare elasticity, in the particular case in which the former derives from a zero substitutibility *plus a zero welfare elasticity,* these four definitions and the Marshallian measure all come to the same thing. The zero substitutability implies no alternative uses and therefore a set of vertical *marginal* indifference curves. The zero welfare elasticity implies that all the marginal indifference curves will coincide. Ricardian land is a favorite example of a zero elasticity of supply of this sort. Its characteristic is that it has only one use, say wheat production. As a consequence of this characteristic (i) it cannot move elsewhere in response to changes in relative prices (zero substitution effect), and therefore (ii) *all* of a given acreage of land of uniform quality is brought into wheat production in response to any positive price per acre (zero welfare effect).[11]

## III. *Conclusion*

Little further reflection is required to recognize that consumer's surplus and economic rent are both measures of the change in the individual's welfare when the set of prices facing him are changed or the constraints imposed upon him are altered. Any distinction between them is one of convenience only: consumer's surpluses have reference to demand prices, economic rent to supply prices. Furthermore, no consideration of logic precludes our measuring the individual's gain—in terms either of the *CV* or the *EV*—from, say, a simultaneous fall in the price of a good bought and a rise in the price of a service provided.

Indeed, in general, if any one, several, or even all prices change for the individual, some demand prices and some supply prices rising, others falling, the resulting change in the individual's welfare can, in principle, be measured by either of our definitions. The *CV* is an exact measure of the transfer, to or from the individual, following a change in the set of all prices, in order to maintain his initial level of welfare. In this case the amount transferred is measured in terms of any one good, in combinations of various goods, or in a combination of all goods dealt in, always using the *new* set of prices. This is quite possible since, given a set of prices, the amount of any one good is equivalent in value to various combinations of some particular goods or of all goods. More usefully, an amount of money calculated at the given set of prices will suffice to measure the *CV*.

On the other hand, the *EV* is an exact measure of the transfer necessary to bring the individual's level of welfare into equality with what it would have been if he were not, as he is, debarred from the new set of prices. The amount of the transfer is now calculated at the *old* prices, and may be expressed in money or in any combination of goods at these prices.

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[11] The indifference curves in this special case would all be horizontal (signifying zero elasticity of substitution) up to a distance representing the maximum supply of productive service from the given resource. At this distance they would all become vertical and, hence, coincide. Rent however measured would, on this vertical limit, be equal to the vertical distance between the two price lines in question.

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## Shifts in Factor Payments and Income Distribution

The major shift in the inequality of income from 1929 to the end of the second world war and its relative stability since that time[1] can be explained in part by the corresponding pattern of, first, the major shifts in, and later, the relative stability of, the various major types of income payments. The following method measures the quantitative importance of the effect of factor-payment changes on income redistribution. The procedure is illustrated in Table 1. The 1952 distribution is presented in the top section of the table in conjunction with the percentage importance of the major types of income. The lower section of the table shows what the 1952 distribution could have looked like had the major types of income had the same percentage importance that they had in 1929.

The only 1929 data used in the redistribution technique are the 1929 major-type percentages presented in Table 1 in the last row.[2] These are multiplied by the 1952 total income of $212.9 billion to obtain each major-type dollar total (for the 1929 distribution) shown in the next to last line of Table 1. Each major-type dollar total is then distributed among the four income classes on the same percentage basis as that shown by the 1952 distribution for that major type. Because of the nature of the data, it was not only necessary to assume constant dispersion within each major type of income, but to assume that the number of families in each of the four income classes would remain the same after the redistribution.

The distribution of 1952 total family personal income by income classes (based on the 1929 major-type distribution) is now simply obtained by adding, for each income class, the incomes of the different major types. The percentage distribution by income classes is derived from these totals.

The 1929 major-type distribution is more concentrated because dividends,

[1] See Selma Goldsmith, "Size Distribution of Personal Income," *Surv. Curr. Bus.*, Apr. 1958, *38*, 11-12 and Lee Soltow, "The Trend Movement in the Income Distribution in Wisconsin for a Twenty Year Period," *Rev. Econ. Stat.*, May 1957, *39*, 223-25.

[2] The available major-type income-class data for 1952 presented in Table 1 are for nonfarm multiperson families. The total of $212.9 billion is somewhat less than total nonfarm personal income of $261 billion. Major-type percentages which are comparable for both 1952 and 1929 are available only for all nonfarm persons. Major-type percentages for nonfarm multiperson family income in 1929 were obtained by: (1) computing, for each major type of personal income, the ratio of the 1952 percentage to that of 1929 for the income of nonfarm persons; (2) applying each of these ratios to the corresponding major-type percentage for 1952 multiperson family income.

TABLE 1—DISTRIBUTION OF PERSONAL INCOME FOR NONFARM MULTIPERSON FAMILIES BY MAJOR TYPE AND BY INCOME CLASS FOR 1952 AND ITS REDISTRIBUTION BASED ON 1929 MAJOR-TYPE DISTRIBUTION
(dollar data in billions)

| 1952 Family Personal Income (before taxes) | Nonfarm Families (per cent) | Total Family Personal Income | | Wages and Salaries (dollars) | Business, Professional Income (dollars) | Dividends (dollars) | Interest and Rental Income (dollars) | Transfer Payments (dollars) |
|---|---|---|---|---|---|---|---|---|
| | | (per cent) | (dollars) | | | | | |
| Under $4,000 | 32.5 | 14.7 | 31.3 | 23.1 | .8 | .5 | 3.0 | 3.9 |
| $4,000–$5,999 | 33.3 | 27.4 | 58.3 | 47.6 | 3.4 | .6 | 4.1 | 2.6 |
| $6,000–$9,999 | 25.5 | 31.4 | 66.9 | 53.4 | 5.5 | 1.1 | 4.9 | 2.0 |
| $10,000 & over | 8.7 | 26.5 | 56.5 | 30.0 | 12.9 | 6.6 | 6.5 | .5 |
| Total | 100.0 | 100.0 | 212.9 (100%) | 154.1 (72.4%) | 22.6 (10.6%) | 8.7 (4.1%) | 18.5 (8.7%) | 9.0 (4.2%) |
| 1952 Redistribution based on 1929 types | | | | | | | | |
| Under $4,000 | 32.5 | 13.5 | 28.7 | 19.9 | .9 | 1.0 | 5.5 | 1.4 |
| $4,000–$5,999 | 33.3 | 25.5 | 54.3 | 40.9 | 3.7 | 1.2 | 7.6 | 1.0 |
| $6,000–$9,999 | 25.5 | 30.1 | 64.0 | 45.8 | 6.0 | 2.3 | 9.2 | .7 |
| $10,000 & over | 8.7 | 30.9 | 65.9 | 25.8 | 14.1 | 13.7 | 12.1 | .2 |
| Total | 100.0 | 100.0 | 212.9 (100%) | 132.5 (62.2%) | 24.7 (11.6%) | 18.1 (8.5%) | 34.4 (16.2%) | 3.3 (1.5%) |

Figures do not necessarily add to totals due to rounding.
*Source: Surv. Curr. Bus.*, June 1956, pp. 10, 13; July 1956, pp. 10, 12; July 1958, pp. 4, 6.

business income, and interest and rent, the three types of income most unequally distributed, were relatively more important in that year. The mean difference[8] computed from the four income classes is $3,820 for the 1952 distribution and $4,320 for the 1952 redistribution using 1929 percentages

TABLE 2—MEAN DIFFERENCE INDEX AND MAJOR TYPES OF PERSONAL INCOME FOR NONFARM MULTIPERSON FAMILIES, 1929–57

| Year | Mean Difference or Coefficient of Concentration Index (1929=100) | Percentage Importance of the Major Type in the Total Personal Income for the Year | | | | | |
|---|---|---|---|---|---|---|---|
| | | Wages and Salaries | Business, Professional Income | Dividends | Interest and Rental Income | Transfer Payments | Total |
| 1929 | 100 | 62 | 12 | 8 | 16 | 2 | 100 |
| 1930 | 99 | 63 | 11 | 9 | 16 | 2 | 100 |
| 1931 | 95 | 62 | 9 | 8 | 17 | 4 | 100 |
| 1932 | 91 | 63 | 7 | 6 | 20 | 4 | 100 |
| 1933 | 90 | 65 | 8 | 5 | 19 | 4 | 100 |
| 1934 | 92 | 66 | 9 | 6 | 15 | 4 | 100 |
| 1935 | 93 | 66 | 10 | 6 | 14 | 4 | 100 |
| 1936 | 94 | 65 | 11 | 8 | 12 | 4 | 100 |
| 1937 | 96 | 66 | 11 | 8 | 12 | 3 | 100 |
| 1938 | 93 | 66 | 11 | 6 | 13 | 4 | 100 |
| 1939 | 94 | 66 | 11 | 6 | 13 | 4 | 100 |
| 1940 | 94 | 66 | 12 | 6 | 12 | 3 | 100 |
| 1941 | 95 | 68 | 13 | 6 | 10 | 3 | 100 |
| 1942 | 94 | 71 | 13 | 4 | 9 | 2 | 100 |
| 1943 | 93 | 74 | 13 | 4 | 8 | 2 | 100 |
| 1944 | 92 | 75 | 12 | 4 | 8 | 2 | 100 |
| 1945 | 91 | 73 | 13 | 3 | 8 | 3 | 100 |
| 1946 | 90 | 68 | 14 | 4 | 9 | 6 | 100 |
| 1947 | 89 | 70 | 12 | 4 | 8 | 5 | 100 |
| 1948 | 90 | 70 | 12 | 4 | 8 | 5 | 100 |
| 1949 | 89 | 69 | 12 | 5 | 9 | 5 | 100 |
| 1950 | 89 | 69 | 11 | 5 | 9 | 6 | 100 |
| 1951 | 89 | 72 | 11 | 4 | 9 | 4 | 100 |
| 1952 | 88 | 72 | 11 | 4 | 9 | 4 | 100 |
| 1953 | 88 | 73 | 10 | 4 | 9 | 4 | 100 |
| 1954 | 88 | 72 | 10 | 4 | 9 | 5 | 100 |
| 1955 | 88 | 72 | 10 | 4 | 9 | 5 | 100 |
| 1956 | 88 | 72 | 10 | 4 | 9 | 5 | 100 |
| 1957 | 87 | 72 | 9 | 4 | 9 | 5 | 100 |

Figures do not necessarily add to totals due to rounding.
*Source of Data:* See Table 1 and footnote 2.

for the different types of income. Thus it might be stated that the change in the relative importance in the types of factor payments over this period is responsible for each family's income being 12 per cent closer to every other family's income.

Similarly, the mean difference for the 1952 distribution of income has been

[8] This is Gini's mean difference. See M. G. Kendall, *The Advanced Theory of Statistics*, 4th ed., London 1948, pp. 42–46.

computed using in turn the major-type percentages of each of the years 1930-57. The resulting mean differences, expressed as percentages of the 1929 figure, are shown in Table 2 along with the major-type percentages.

It must be remembered that the procedure assumes that total income for each year was the same as it was in 1952 and that the dispersion of income within each major type remains constant, so that only the change due to the shifting importance of types of payments is reflected. Differences due to changes in such characteristics as age, education, and occupation are not considered except at they are responsible for shifts in major types of payment.

It can be seen that from 1929 to 1933, with the drop in importance of the two most unequally distributed types (dividends and business income), an inequality level near that of the postwar period was achieved.[4] Half of this decrease was regained by the start of the war, largely because of the increased importance of business income. The sharp drop in interest and rent had a more limited effect since the inequality of this type is much closer to that of the inequality of total income.

A comparison of 1941 with 1947 shows decreases in the three most concentrated types, and increases in the two least concentrated types. The relative constancy of the mean difference since 1947, as shown in Table 2, reflects the fact that the percentage importance of the factors has been constant except for a shift from business and professional income to wages and salaries.

The radical shifts in importance of the major types of payments from 1929 to 1947 decreased inequality over 10 per cent; and the relative stability of major types since 1947 explains, in part, why the distribution of income has remained constant since that time.

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[4] The coefficient of concentration (*ibid.*, p. 45) can be used as an indication of the degree of inequality for each of the five major types in 1952. The coefficients for the 1952 data of Table 1 assuming each family received all types are: .27 for wages, .63 for business and professional income, .72 for dividends, .36 for interest and rental income, —.13 for transfer payments, and .32 for total income. The negative figure for transfer payments arises because the families in the lowest-income class receive a large per cent of transfer payments. The coefficient of concentration of total income for each year can be approximated by applying the major-type percentages of Table 2 as weights to the above coefficients.

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## Interstate Apportionment of Business Income

The Supreme Court early in 1959 ruled explicitly on the scope of state jurisdiction under the Constitution to tax income from purely interstate business activity. The Court decided that states have significantly broader power than they have been exercising. A nondiscriminatory, properly apportioned state tax on net income can now be imposed on a foreign (out-of-state) business when the activities in the state constitute a sufficient "nexus." The contact necessary to sustain tax, we now learn, is decidedly less than formerly assumed. There is no doubt about the emergence—more accurately the accentuation—of a problem with economic, as well as legal and accounting, aspects.

In principle the extension of state taxes as now permitted would not be cause for concern *if* compliance costs could be kept modest and *if* a proper basis for apportionment were utilized. Yet it seems inevitable that compliance costs will often be high in relation to tax, not infrequently more than the tax itself. One method of helping minimize what is avoidable social waste would be to standardize the formula. The more states using the same formula, the more economically businesses can comply and, in fact, the more efficiently governments can administer the tax. Perhaps now that many more businesses face a problem which heretofore has involved only a relative few, significant political pressure will build up for standardization—possibly even for Congressional action.[1]

For many decades states have sought to allocate, i.e., to divide up for tax purposes, the income of a corporation or unincorporated business among the states in which the firm carries on its activities. Many formulas have been devised and have received at least implicit Court approval. There has been a long and frustrating history of efforts to get agreement on a single formula. Despite some progress toward uniformity, practice still differs widely.[2]

States utilize their freedom to prescribe the formula each prefers. Any large firm will likely find itself computing and paying tax on several bases. It now seems certain that very many more firms must comply with many more laws. The prospect of being subjected to state tax on more than 100 per cent of net income has increased. Yet this is less serious for these firms than the added cost of accounting, legal services, record keeping and storage, negotiation, and litigation. Untold numbers of small and medium-sized firms face a discouraging increase in operating costs—costs most of which for the economy are sheer waste.

The failure of states to achieve greater harmony results in part from the desire of some states to "import" income for tax purposes. Choice of one as against another allocation formula can permit just this result. For example, a state whose residents buy more manufactured goods from outside than they "export" can decree that a large part (or all) of business net income originates where the products are sold, i.e., where buyers live. The place where goods are made is held to be unimportant. Adjustment of the elements of the formula and of their weighting to take account of the nature of a state's economy can make a difference in the state's tax base. Legislatures can reach out and tax "foreigners," corporations (stockholders, consumers, employees, or whoever bears the corporation tax) who have no vote in the state. Any

[1] There does seem to be persuasive argument for national action. There *is* a public interest which extends over the whole economy. There is *not* much basis for arguing that the persons affected can protect themselves in state legislatures. How often will a small "foreign" business have any significant political influence with the legislatures of other states which now have power to tax it? The ability of businesses to protect themselves by shifting the tax to consumers is a topic requiring more analysis than possible here.

[2] The National Conference of Commissioners on Uniform State Laws did agree in 1957 on a model income-allocation statute. The American Bar Association approved. Considerable opposition developed in the business community, however, and apparently no legislature has yet (April 1959) adopted the proposal. Though undoubtedly excellent in many ways it is subject to the basic defect which I discuss below.

state is now free to decree that some condition which does not in fact create income shall be assumed for tax purposes to be the source of income. In view of current and prospective pressure for state revenue and of the invitation the Court has now extended, ambitious, even greedy efforts are not unlikely.

In such cases the rational taxpayer will try to avoid whatever brings no income but which does bring tax liability. He will wisely shift activity from one form to another if by so doing the loss of income (or the extra expense) is less than the tax saving. There is a presumption, I think, that such shifts will involve some loss of real income through poorer allocation of resources. Any gain to this or that state treasury may be bought at much greater cost to the economy.

The most widely used basis for income allocation is the "Massachusetts formula." Property, payrolls, and sales (receipts) are employed and weighted equally.[3] This formula was developed more than half a century ago. But use of the sales (receipts) factor has little—or no—economic justification. Human effort produces income. Use of property produces income. Selling and purchasing, however, do *not* produce income except as human effort and property are involved. Although selling is, of course, a part of the income-creating process, its contribution is represented not by the dollar volume of sales but by the remuneration of the human and material resources that perform the selling function. To assign sales an independent importance is to depart from economically proper apportionment. Worse still, this practice creates an inducement to arbitrary, complex, and wasteful manipulation of sales arrangements. I submit that a formula based on economic reality will accord no separate or distinct place to sales.

Assuming some possibility of standardization, what goal would best serve the public interest? What formula would have the least tendency to distort economic decisions and to retard efficient economic growth? (Such a formula would also possibly be the fairest; but discussions of fairness are likely to be inconclusive.) Income is created by human and material resources. The resources utilized by a business as a whole in producing its income can be measured reasonably well by what is paid for them. Moreover, the places where the resources have been producing during a year can be determined on a consistent, though not completely unambiguous, basis. In numerous cases "separate accounting" for activities in each state is feasible and is now permitted. However, where it is not—and for many firms most affected by recent decisions separate accounting will not be possible—what other basis can serve?

As a starting point it seems acceptable to assume that property and labor contribute to net profit in proportion to what the firm pays for them. That is, more of the income of a business arises in the state in which payrolls are large

[3] There is, however, extensive variation in the factors utilized, their weighting, and in the definition and practical application of each element. For example, some states say that sales are located at the office where they are negotiated, others where the property is at the time of the order, or where the order is accepted, or where the negotiating personnel are located, or at point of delivery, or at origin of shipment—or at some other place. Several methods are also used in determining the situs of payrolls.

rather than small—and in direct proportion to the amounts.[4] The same applies to property. For example, one-eighth of the payroll and one-sixth of the property may be in one state. Then one-eighth of whatever payrolls (labor) contribute to net income, and one-sixth of the contribution of property, will be attributed to the state.

If this procedure is accepted, the problem then arises of determining how much is spent for labor and how much for the services of property—in total and in each state. Payroll data are available. However, figures for annual "spending" on services of property do not exist in any form that is usable for the purposes at hand. Some of the "payment" for the contribution of property takes the form of profit. And we certainly do not know the profit to allocate to each state—that is just what we seek to determine. Capital values are available, however, and these do reflect the values of the services of property.

Thus there is reason for the established practice of computing payrolls on the basis of annual outlays while using capital values for the property factor. However, if payrolls and property are computed on different bases, how can the results be combined? A simple weighting of the fractions seems acceptable. One possibility is to weight on the basis of national income aggregates. Payrolls would then carry about four times the weight given to property. Yet for the businesses involved—very largely manufacturing but also some finance and commerce—property can be more important than it is on the average. While a two-to-one weighting would tend to underweight the labor element somewhat, I am inclined to support it tentatively—and to invite comments and suggestions for more rational treatment.

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[4] An economist might well insist that net profit is attributable to equity capital, other factors being paid as much as they are worth at the margin. I see no basis, however, for apportioning a firm's equity capital among states except on a basis no less arbitrary than involved in the more conventional proposal made here.

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## Principles of Debt Management: Comment

In the June 1957 issue of this journal, Earl R. Rolph[1] undertook to show how the public debt should be managed to minimize the total interest charges while using the size and composition of the debt to achieve any given stabilization goal. Rolph states that "the composition and size of an outstanding national debt is optimal when the marginal utility of each kind of debt instrument is made proportional to its marginal cost" (p. 302). Without regard for whether the author is correct in considering it desirable to minimize interest costs, the purpose of this paper is to show that Rolph has made two analytical errors which may seriously damage his analysis.

He uses the term "moneyness," which all debts have in varying degrees,

[1] Earl R. Rolph, "Principles of Debt Management," *Am. Econ. Rev.*, June 1957, *42*, 302-20.

depending upon their liquidity and lack of risk. The shorter the maturity and the more these qualities are present, the greater the moneyness of the security. Gross national product is assumed to be a function of what I would call the total "money-equivalent" in the economy (which is the sum of all money and near-money in the private sector of the economy, weighted by the degree of moneyness of each). Gross national product may be reduced by removing money from the economy and substituting debt with less than 100 per cent moneyness. The less the moneyness of the debt, the more effective a deflationary tool it is.

A graphic analysis is used to determine the distribution of money and debt maturities needed to maintain a given gross national product (p. 312). Only debts of two maturities are assumed, one short-term and one long-term. Starting with any given distribution of long-term and short-term debt, if the government were to buy back an amount of long-term debt and sell an equal amount of short-term debt to the private sector, the amount of genuine money in the economy would be unchanged. However, a poorer substitute for money would have been replaced by a better one. This would be inflationary. To keep the total money-equivalent constant (and GNP as well), when a given amount of long-term debt is bought back, the total outstanding debt must be increased by selling a greater amount of short-term debt. Thus, as Rolph correctly states, an isoquant of gross national product may be represented by a line sloped negatively greater than 45 degrees. This would be a straight line if the substitutability of the two forms of debt for money were constant. But, as successive substitutions of short-term for long-term debt (into the private sector) are made, short-term debt becomes an increasingly poorer substitute for money as the total debt increases and the money supply decreases.

Having come this far without mishap, Rolph now becomes the victim of his own terminology. He reasons that since short-term debt becomes an increasingly poorer substitute for money when short-term debt is substituted for long-term debt, increasingly greater amounts of short-term debt sales are needed to counteract one unit of long-term debt bought (p. 313, n. 20). He therefore concludes that the curve will be convex to the origin. We started, however, with the premise that the poorer a substitute for money a debt was, the more deflationary its sale would be. Thus, Rolph is saying that the more potent a deflationary tool short-term debt becomes, the more of it is needed to achieve a given amount of deflation. Obviously, he is in error.

To put it another way, as short-term debt becomes an increasingly poorer substitute for money, it comes nearer and nearer to long-term debt in degree of moneyness. As short-term debt becomes more and more like long-term debt, the differential between the amounts of long-term debt bought and short-term debt sold must decrease. Therefore, as short-term debt is substituted for long-term, less and less of it is needed to counteract a given amount of long-term debt. The isoquant of gross national product will not be a curve convex to the origin, as Rolph has drawn it, but concave to the origin, with a negative slope (decreasing in steepness to a lower limit of 45 degrees, as the debt approaches 100 per cent short-term, if the limiting case is equality of moneyness).

A similar set of isoquants of total interest cost are also concave to the origin, as shown by Rolph (pp. 314-15). (At all points, it is assumed, long-term

debt has the higher yield, but increasing the size of an issue raises its yield.) Unfortunately, we now have two concave curves, instead of one concave and one convex. Now, we can no longer simply label the point of tangency bettween the desired GNP isoquant and any total-cost isoquant as a minimal cost distribution. With two sets of curves, both with substantially the same restraints upon them (concave to the origin, with a negative slope falling toward, but never quite reaching 45 degrees), the minimum cost point will be quite troublesome. It is highly possible that this point may lie on one axis or the other. It may also be highly unstable, and alternate between the two extremes. However, the importance of the altered graph lies not in the instability of the minimal cost point, but rather in the stability of the total interest cost as the distribuion changes along an isoquant of gross national product. Obviously, the more similar in shape the two sets of isoquants, the less effect changes in the distribution will have on the total interest cost.

Rolph next turns to the general case, where debts of all maturities are considered. In his Figure 4 (p. 317), with years to maturity on the $X$-axis and yield on the $Y$-axis, he graphs an interest rate ($Y_m$) rising with the length of maturity. A "variable multiplier" (not graphed) is, in effect, ($1 debt)/(effect of $1 debt) for each maturity. If graphed, it would be a falling function. From these two functions, he derives an "economic yield" curve ($Y_e$), which is $Y_m$ multiplied by the variable multiplier. $Y_e$ is, therefore, (interest rate) ($1 debt)/(effect of $1 debt). Thus, $Y_e$ is the average cost of a given deflationary effect. According to Rolph, "Economical debt management calls ideally for making the $Y_e$ curve a straight horizontal line. The interest on a national debt is a minimum when the interest cost of each type of debt instrument per dollar of 'product' (*i.e.*, change in private expenditures) is equal" (p. 317).

Rolph's solution for minimizing total interest cost makes the *average* cost of a given deflationary effect obtained from each kind of debt instrument equal, rather than making the "marginal utility of each kind of debt instrument . . . proportional to its marginal cost," as he has originally proposed. This is clearly not the same. The government may be viewed as a monopsonist buying various amounts of product (depressionary effect) from various markets (different maturities). Such a monopsonist would not minimize his total cost by distributing his purchases so that all prices were the same, unless the elasticities of supply were the same in each market. Therefore, there is no reason to presume that a horizontal $Y_e$ curve minimizes the total interest cost.

The fact that this technique is faulty is relatively unimportant. The inherent similarity in shape of the isoquants of gross national product and total interest cost, discussed earlier in this paper, points to the unimportance of the debt distribution (along a GNP isoquant) as a determinant of the total interest cost. If this is the case, Rolph's analytical procedures, correct or incorrect, have but little economic significance.

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*The author, a graduate student at The Johns Hopkins University, would like to thank Professors Edwin S. Mills and Fritz Machlup of The Johns Hopkins University for their helpful criticism.

## Principles of Debt Management: Reply

Friedman's first criticism concerns the case of a two-dimensional debt structure consisting of long-term and short-term securities. I held that the substitution of short-term for long-term debt in such a way as to leave private expenditures unchanged would require that the total debt increase, and that more and more short-term debt would be needed to offset a unit of long-term debt. Friedman asserts that the latter point is incorrect. Less and less short-term debt would be needed to offset a unit of long-term because the special attractiveness of short-term debt, what I called its moneyness features, diminishes as more of this debt becomes available. Friedman is clearly right in this observation. The GNP isoquant should have been depicted as concave rather than convex. Attention may be called to his demonstration of the precise character of such an isoquant in the limiting case. This finding is a positive contribution.

With two concave relations, a unique solution for a two-form debt structure is no longer assured. Corner solutions become possible; Chairman Martin's "bills-only" doctrine and Henry Simons' "perpetuities-only" doctrine may both be correct. Having made a good point, Friedman goes rather far in claiming (see his final paragraph) that the maturity distribution does not matter, at least not much. His conclusion holds if the relevant GNP and the total interest cost isoquants approximately coincide. How does Friedman know that they will? The interest expense of any actual two-form debt structure may exceed the minimum (or minima) by many times. Empirical questions cannot be resolved so easily.

Friedman's second criticism concerns the general case of a public debt structure of all maturities. He objects to my condition that debt-management officials should aim at making the weighted yields of different maturities (weighted by their comparative effects upon private expenditures) identical, holding that this condition is valid only in the event that what he describes as the elasticities of supply happen to be equal. I interpret this criticism to mean that if the demand schedules to hold government debt of different maturities are negatively sloped, officials should be guided by the weighted yields computed from marginal revenues rather than from market prices in selecting among debt forms to buy back or to sell. Hence, unless the price elasticities to hold different debt forms are the same, the government could save interest expense by concentrating the debt in forms having relatively high elasticities of demand. Such a rule calls for equating weighted yields of different maturities computed from marginal revenues rather than from prices.

As a formal proposition, such a rule would reduce the interest expense more than the one I proposed. But it is half-way solution. The interest expense could be reduced even more by segregating private demands to buy and sell debt and by charging and paying different prices to various classes of debt customers (Pigou's third-degree price discrimination). Conceivably even all-or-none deals could be made with each potential buyer or seller. Minimizing expense, like maximizing profits, is meaningful only with respect to defined constraints. Friedman's solution as I interpret it calls for treating market demand sched-

ules for debt as the constraints. My solution calls for treating market prices as the constraints. The debt structure and size are to be manipulated by reference to existing prices (and hence yields). The altered debt combination will result in changed yields. The process is to continue until yields, as computed from market prices and weighted by the relative effects of different maturities upon private expenditures, are made equal. Friedman's solution requires a similar experiment when the yields are computed from marginal revenues. Hence, the various elasticities of market demand schedules for different maturities must, somehow or other, be ascertained. Such information would be difficult if not impossible to acquire.

The original article was designed to suggest workable rules for the conduct of debt-management and open-market policies. As such, the proposals might be criticized as expecting too much, rather than too little, information and skill on the part of officials in charge. Friedman's solution calls for yet more knowledge. Both solutions are correct on theoretical grounds; others may also be correct, as indicated above.

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### Erratum

Re the review of Eli Ginzberg's *Human Resources: The Wealth of a Nation* (this *Review,* March 1959, pp. 204-5), Eli Ginzberg is not Director of the National Manpower Council but is Director of Staff Studies for the Council. The book was written by him in his capacity as Director of the Conservation of Human Resources Project, Columbia University.

# BOOK REVIEWS

## General Economics; Methodology

*Exacte economie.* By D. B. J. SCHOUTEN. Leiden: H. E. STENFERT KROESE, 1957. Pp. viii, 226. *f* 18.–.

Professor Schouten, of the Catholic Economic Institute in Tilburg, Netherlands, has attempted a reformulation and synthesis of economic theory, including doctrinal history, micro- and macro-theory, growth and cycle theory, and international economics. The bulk of the book consists of ten models, described as: (1) "Quesnay à la Leontief," a system of intersectoral supply, in which labor is superabundant and land is the only scarce factor of production; (2) the growth theory of Ricardo, in which both labor and good land are superabundant and capital is the only limiting factor; (3) the stationary theory of Ricardo, in which good land is scarce and poor land and capital are superabundant; (4) the growth theory of Marx, in which capital is scarce because of excessively rapid technological progress; (5) the Walrasian system, in which substitutability among final products and the influence of prices on consumption patterns make possible the full employment of all factors of production, thus undermining the concepts of absolute scarcity or superabundance of particular factors; (6) the Keynesian system, in which a superabundance of capital entails underemployment equilibrium; (7) a business-cycle model in which lags and expectations generate fluctuations in the volume of production and employment; (8) a business-cycle model emphasizing price fluctuations, in which Tinbergen's relation between investment and profits plays a key role; (9) "the modern price and production theory," in which substitutability among factors of production limits the relevance of the concept of superabundance of a factor; and (10) the theory of international trade, in which freedom and restriction of commodity and factor movements are compared.

According to Schouten, his study is distinctive in using a uniform method and uniform algebraic symbols to bring order into the apparent multiplicity of economic theories and thus highlight their correspondences and divergences. Except in the two cycle models, Schouten adopts what he calls "the method of linear programming." This, however, goes little beyond graphs measuring consumption-goods production on one axis and investment-goods production on the other and representing by straight lines the output limitations posed by the available stock of each factor of production, given the technology. Each model is also cast into a set of equations expressing definitions, expenditure on consumption and investment as related to real income, technological relations between factor input and product output, supply-and-demand equilibrium for factors, and pricing. Finally, each model has an elaborate numerical example illustrating determination of the unknown quantities and prices. The mathematics is restricted to arithmetic and elementary algebra; though the

multiplicity of symbols, equipped with subscripts and superscripts, does present a formidable appearance. Only in appendixes does Schouten venture into the higher mathematics of inversion of the Leontief matrix and solution of dynamic equation systems.

The book amply illustrates the fact, incidentally, that use of symbols does not guarantee precision of thought. Schouten quite casually bandies about words or symbols for such things as "good" and "poor" land, "consumption" and "investment" goods, the minimum basket of consumption goods required for supporting a worker's family, the "total welfare" of two countries considered together—and "superabundant" and "scarce" factors of production. The latter distinction plays a peculiarly important role in squeezing the doctrines of earlier writers into Schouten's preconceived framework. His avowed attempt to organize a presentation of economic theory along the lines of doctrinal history results not only in lopsided theoretical emphasis but also in misrepresentation of the earlier writers. (For example, Ricardo is said to have considered only labor costs in his international-trade model because of a special assumption that not capital but only labor was a scarce factor.) Sometimes it is difficult to tell just when Schouten slides from ostensible paraphrase into commentary of his own. A complete absence throughout the book of references to any specific writings heightens the reader's, and perhaps betrays the author's, uneasiness on this score. Also missing are a bibliography and an index (and the table of contents is not detailed enough to serve as a substitute).

Not only Schouten's whole approach but also some specific comments (e.g., pp. 3-4) suggest that he regards various theories less as steps along (or sometimes astray from) the path of scientific progress than as schematizations of objective conditions supposedly prevailing or foreseeable during the lifetimes of the various economists. He deals with separate *models*, not with a continuing quest to find underlying uniformities in the superficial diversity of economic phenomena and to develop generalizations of broad explanatory and predictive value. His book conveys no appreciation of economics as an evolving, cumulative field of inquiry.

The purely negative impression that this review may so far have created is unintentional. The book is indeed a meritorious performance, demonstrating the author's creativeness and the breadth of his scholarly interests. This reviewer is unenthusiastic chiefly because Schouten's wide-ranging book is not tailored for any particular audience. It is neither for the beginning student nor for the sophisticated theorist. (Appendix 1, for example, contains a survey of standard marginalist theory which is too condensed and sketchy to be of use to either.) The book is not for the economist seeking an introduction to linear programming, not for the mathematician or econometrician, and certainly not for the student of doctrinal history. Some readers may find ideas useful for quasimathematical model-building. Students interested in this sort of thing might do better, however, to start with something like E. F. Beach's *Economic Models* (New York 1957).

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*Common-Sense Economics.* By G. M. TUCKER. Harrisburg, Pa.: Stackpole Co., 1957. Pp. xiii, 289. $3.75.

When a creed is newly born its enthusiasm and intellectual drive often make it an admirable performance. Perpetuated beyond its time, the creed becomes less attractive. When two such creeds deriving from different social periods and reflecting different intellectual moods are consolidated with disharmonies ringing to the ear and the whole unsuited to the times, is anything more lost or futile or forlorn?

Substantially, this is the case with Tucker's book which effects a merger of the social statics of Herbert Spencer, restated in the idiom of an extreme "free-enterprise" credo, and the social philosophy and economic analysis of Henry George with his hostility to landed property and to rental incomes. The book is in form a textbook of economics with reference almost exclusively to American conditions.

Space in the book is allocated between a brief statement of the philosophy of work and wealth (pp. 1-65), an analysis of money and of inflation (pp. 65-95), a vigorous defense of Big Business (pp. 104-38), a short treatise on public finance (pp. 138-224); and social philosophy variously applied to current over-all trends (pp. 224-87).

The reviewer is not unsympathetic to the concerns which have driven the author. And the presentation in the book doubtless has a sparkle and persuasiveness which the conventional text could well emulate. But nonetheless the book is a dangerous one. The parade of dogmatisms does offend and the creed is set forth with little protective foliage of established fact or scholarly endeavor. It is simply not true that "practically all the world's monetary gold" was "buried in Kentucky" (p. 196). Wesley Clair Mitchell did not end some "four hundred pages of dreary reading" by "casting doubt" on the very existence of cycles, "implying that we are searching for something that does not exist" (p. 95). Tucker has confounded the punitive tax which drove out the circulating notes of state banks with the required "backing" of federal bonds stipulated as collateral for issuance of national bank notes (p. 78). Big Business for him cannot offend and "if occasionally a big concern does seem to get the lion's share of one particular market, this influence is counterbalanced a dozen times over by this greater and broader competition" (p. 130). J. M. Keynes is noticed as "a British economist of a kind" (p. 142).

The argument for the single tax raises some interesting issues. Tucker emphasizes that our present property taxation discourages new urban building particularly of rental property. This thesis in more sophisticated form has been recently developed by Morton in his *Housing Taxation* (1955). Transformation of the present property tax into a tax on urban land values would probably stimulate property renovation and improvement and this chiefly in older city-central areas where tax rates are heaviest and where deterioration cumulates. But Tucker does not recognize that this transformation would redistribute tax burdens among local residents so as to make the property tax less progressive in a general sense (or more regressive technically). The fact that business enterprise would contribute less by indirect levies to local government services is a possible advantage which Tucker over-

looked. But unless well-to-do families use frontage and acreage in proportion to their investment in improvements, then the concentration of urban levies on site values would redistribute levies among sites on more regressive lines. Tucker as a libertarian liberal bucks at progressive taxation generally. But most of us would hesitate at increasing regressivity in local taxation.

Over time, of course, a system of taxation of land values would encourage concentrated building on smaller lots. Besides, on impact the tax would virtually expropriate the equity of holders of present vacant lots which would probably revert to public ownership. Tucker continues to chant of land monopolists. He has not noticed that the bulk of our urban and rural land is broken up in small holdings; that urban land values have been greatly cheapened by the street car, bus and automobile; and that net rental incomes make up a small share of national income.

MANUEL GOTTLIEB

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*The Functional Economy: The Bases of Economic Organization.* By BERNARD W. DEMPSEY. Englewood Cliffs: Prentice-Hall, 1958. Pp. xi, 515. $6.00.

Father Dempsey's book is not an economic principles text in the common (or meaningful) use of that term. It is not a work in economics either, as that word is generally employed. *The Functional Economy* is an interpretive work on Catholic social philosophy with respect to the organization of all aspects of society. As a serious work it is subject to Father Dempsey's *apologia* on Catholic proposals for reform in general:

> Catholics sometimes believe that the indifference with which professional economists meet their proposals of reform stem from bias. In a very great number of cases this is certainly not so. What has happened is that the economist has been alienated by what he must inevitably consider sheer utopianism, since it is not accompanied by any reputable economic analysis (p. 330).

In most respects this book will be treated with the indifference noted above and in large part for the reason stated.

> *The Functional Economy* examines systematically the forces and facts present in every real, working economy. The author projects the ideal situation, which intelligent adherence to the enlightened teachings of the Church and constant awareness of the lessons of history can achieve, against the background of historic and existing economic communities. . . . The necessity of and the means for improvement of the economic community, the great challenge that lies in man's call for the restoration of social order, the historical development of man's moral attitude toward economics—all this has been analyzed in the light of traditional philosophy (p. v.).

Unfortunately, the text does not bear out the promise of the Preface. In Father Dempsey's system "All of the persons who make up any society have one supreme unconditional end, God" (p. 367). All have one earthly goal, the "perfection of human personality in this life" (p. 187, 191). All human society must be ordered to that goal, since man lives not isolated but in social groups.

Such perfection in human personality can only be achieved when that "social justice" has been established which requires a proper ordering of all aspects of life (p. 207). Social justice exists in a society when the relationships of man to man, organized community to man, and man to community are just; *i.e.* when commutative, distributive and contributive justice exist (p. 217ff., and Ch. 19). Social justice is the outcome of all human actions, and these in turn are subject to law.

> Human beings live under law, not merely the written law of the civil community, but the natural law of right reason, the divine eternal law, and the super-natural law of the kingdom of Christ. Law, the divine and eternal, governs all human actions, even those which civil law cannot reach, and all human actions, therefore, are related to the common good (p. 369).

The question neither raised nor answered in this legal connection is who or what on earth is the Court of last resort in the temporal, economic area?

In addition to the principle of social justice, "much more important . . . is the principle of subsidiarity" (p. 243). "All communities are subsidiary to . . . the concrete human persons who compose them" (p. 243). The principle of subsidiarity has as the role of government the regulating of the small and (to Father Dempsey) minor portion of man that is political. Such political government must be subsidiary to the groups (principle of functional association, p. 319) that form the good society. And the government's role in economic affairs should be limited to Adam Smith's point of view, for Lincoln's reasons (p. 282), based of course on Mill's "On Liberty"—which is not cited (see pp. 242, 495-96).

In all cases where the work might be considered "economic analysis" something is left to be desired (e.g., pp. 54-55, 99, 200, 426, 430, 439). And the scholastic form of argumentation that is at times employed makes, at best, for needlessly difficult reading.

> Briefly labor is a cause of the whole value of the product: (1) current labor alone, I deny—current and past, saved labor, I concede; (2) is the whole cause of the whole value, I deny—is the partial cause of the whole value, I concede (p. 196).

Another reading difficulty is presented by awkward terminology: e.g., monopolist, meaning any entrepreneur (p. 346); or development, meaning only innovations à la Schumpeter (p. 356). And, in a few cases sources which one would expect are not given (cf. "Summary of Flow-of-Funds Accounts for 1953," a table, p. 50).

In spite of its shortcomings this work serves two interests well: those interested in the Thomastic-Holy See economic philosophy (not analysis), and those new non-Catholic Ph.D.'s interested in employment on a Catholic faculty. I would suggest, also, that those who cite from papal encyclicals and from Aquinas in their courses would make use of Father Dempsey's work.

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*Selections in Economics*. Edited by R. C. EPSTEIN and A. D. BUTLER. 2 volumes. Buffalo: Smith, Keynes and Marshall, 1958. Pp. viii, 247; viii, 216. $4.50; paper, $2.95, each.

These two volumes combined contain 25 selections, averaging about 6,000 words in length. Each selection is either a complete article or a complete chapter from a longer work. Most of the authors are distinguished modern economists.

The editors claim two purposes for their selections; to supplement elementary texts and to interest readers ". . . who seek to bring their earlier and more formal study of the subject up to date." The second purpose is not realized. The first volume does provide an up-to-date treatment of certain phases of macroeconomics, but the most important selections are highly formal. It is doubtful that they could be read with comprehension by anyone who is the least bit rusty in economics. As far as the treatment of microeconomics in the second volume is concerned, there would be nothing new for a student who had not looked at a book in the area since 1930. This section is not only out-dated; it is positively old-fashioned. The four theoretical selections in value and distribution theory all depend on utility analysis and the assumption of pure competition. For these reasons, I will consider the value of the selections for class-room use rather than for the general reader.

It seems apparent that the typical readings book in economics is designed for a "survey" or "problems" course rather than for a "principles" course. The brevity and disputatiousness of the bulk of the selections in most readings books seem to be intended to lend artificial life to an intrinsically boring subject. As the teacher of a real principles of economics course refuses to admit that his subject is inherently dull, this type of book is of little value to him. The ideal outside readings for such a course should clarify or illustrate theoretical points that are difficult for the beginning student, especially when these points are not well developed in the text. It should not set forth debatable conclusions without premises or proof, but should provide a full logical development of each topic treated.

It is apparently the intention of Professors Epstein and Butler to provide just such an ideal supplement for use in the principles course. They have succeeded to a substantial extent in their first volume, which covers income theory, money and economic fluctuations. The selections in the second volume, covering value and distribution, labor, international trade and economic development, are less satisfying. I have been unable to call to mind any widely used principles text that would be substantially strengthened in its coverage of value and distribution by these readings, although this is the weak point of many currently popular texts.

The selections vary widely in difficulty. While one third of them are easy selections from principles texts, another third make no concessions whatever to the beginning reader. Thus the essay by Keynes, which the editors feel to be ". . . far clearer than his exposition of some of the same ideas [in *The General Theory*]" is likely to be quite obscure to American students even after completion of the principles course, for it assumes a better understanding of the internal effects of international balances than most of our students

acquire. Similarly, some of the other selections require a more penetrating understanding of several areas of economic analysis than can be hoped for at any stage of the principles course.

In reproducing the chapter on "Supply and Demand" from Bye's *Principles of Economics* the editors have given new life to old errors in the treatment of decreasing supply price. Mitchell's article on business cycles contains a less important error in his discussion of the acceleration principle, stemming from an implicit assumption that disinvestment is impossible. There are also a number of debatable conclusions and half-truths that have no place in an authoritative work, though many of these are in the popular articles.

The high price of the two volumes, together with the weaknesses mentioned above, limit the value of these selections for use in the principles course. However, the first volume may find wide use in intermediate level courses in macroeconomics or money and banking. One may hope that its sale will be large enough to bring forth a rash of cheap imitations.

V. F. Boland

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## Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

*Videnskab og velfaerd i økonomisk politik.* (Science and Welfare in Economic Policy.) By F. Zeuthen. Københabns Universitets Økonomiske Institut, Stud. No. 1. Copenhagen: G. E. C. Gads, 1958. Pp. 97.

This essay inaugurates a new series of studies to be published by the Institute of Economics of the University of Copenhagen. Professor Zeuthen, the founder of this distinguished school, has for a long time been concerned with problems of practical social policy as well as with the advancement of economic theory. This book is an attempt to bridge the mental gap that usually separates these two fields of investigation.

The central problem is that of applying scientific method in a field so far dominated by subjectivity, dogmatism and ideologies. Is it possible to rationalize policy by applying experimental and logical methods? The many aspects of this problem are thoroughly, though concisely, discussed. Old and new welfare theories, starting with Bentham, are re-examined. The Paretian stumbling block of the dichotomy between the scientifically valid efficiency theory and the nonscientific distribution doctrines is once more discussed, and overcome by referring to common sense, which legitimates interpersonal comparisons of utility. Production and exchange on the one hand, and distribution on the other, are dealt with separately, before being merged again in the discussion of practical problems in the last chapter. Combining efficiency and distribution criteria in solving policy problems involves compromises whenever the two sets of criteria are incompatible. Distribution policy should be designed in a way to promote efficiency or, at least, to minimize any loss of production; conversely, a policy aimed at increasing productivity should have regard for the distribution effects. The problem of centralization versus decentralization in policy-making is discussed and solved in a nondogmatic way. The author

then discusses R. Frisch's and Tinbergen's attempts to rationalize policy-making by using decision models.

The author concludes, à la Robertson, by pleading for the use of common sense in addition to science in dealing with practical problems. To be useful for policy, science must build on plausible, not on arbitrary, assumptions. On the other hand, to be successful in the long run, policy decisions must be founded on sound analysis.

Although this book makes no new contribution to welfare theory, nor to the solution of concrete problems, it contributes mature wisdom, which is a comparatively rare service.

ROGER DEHEM

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*Investment in Innovation.* By C. F. CARTER and B. R. WILLIAMS. New York and London: Oxford University Press, 1958. Pp. ix, 164. 15s.

This book, a sequel to *Industry and Technical Progress* by the same authors, is sponsored by the Science and Industry Committee appointed by the Royal Society of Arts, the British Association for the Advancement of Science, and the Nuffield Foundation. Although it draws its facts from a study of British industry, its conclusions ring true for the United States as well.

The Appendix at the end of the book might well be read before the first chapter, because this appendix outlines the orthodox theory which the book seeks to improve. Here is set forth the classical theory on saving and innovation as worked out by Adam Smith, Ricardo, Mill, Marshall, Pigou, Knight, and others. A very interesting account it is.

New inventions, new products, new methods, new scientific knowledge, all these things give entrepreneurs an opportunity to invest in new capital goods with the hope of high profits. This book would have been of great interest, therefore, to J. B. Clark, with his theory that business profits, the fourth distributive share, are the reward for successful pioneering. It would also have been of interest to Schumpeter, with his theory that the exploitation of great inventions one after another is what causes business cycles. It would likewise have been of interest to Keynes, with his theory that planned investment may differ from planned saving; and also of interest to Hansen with his theory of secular stagnation. And today the book is of especial interest to the Board of Trade, who financed its publication, because the Board wants to know what if anything can wisely be done by the government to control the busness cycle.

The book asks questions like these: Where do new ideas come from? What sort of ideas are most wanted? How are they sifted? Who makes the choice in the end? How is allowance made for risk and uncertainty? Is there any shortage of ideas? Is there any slowness in making use of them? Is there any shortage of money to exploit them? What rate of return is needed to get an innovation accepted? Do corporations with great research laboratories (like Bell Telephone, General Electric, and du Pont in the United States) have a customary cut-off point between acceptance and rejection? Does this cut-off point change with changes in the rate of interest?

In answer to the last question, the book cites a long list of case studies to prove that the rate of interest is usually of small importance compared with the outlook for profits, the volume of unfilled orders, the firmness of prices, the burden of taxes, and the amount of excess capacity in the industry.

Would it have been digressing too far from the main theme if the authors had gone on to show that certain other kinds of investment—in contrast to investment in innovation—are indeed very sensitive to changes in the rate of interest? They could have mentioned investment in private housing financed by home mortgages, and they could have listed public works like schools, streets, sewers, waterworks, bridges, subways, etc., financed by municipal bonds that boost the tax rate on real estate, and they could even have included long-term Treasury issues used to refund the floating debt, for in all these cases the flow of new issues is very sensitive to the current price of bonds and the success of recent offerings.

The authors, by asking the right questions and going to business managers themselves for the true answers, deserve our hearty approval. Their book is a real success, because it adds a great deal to our understanding of the free enterprise system and how it actually works.

JOHN BURR WILLIAMS

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*Studies in the Mathematical Theory of Inventory and Production.* By KENNETH J. ARROW, SAMUEL KARLIN, and HERBERT SCARF. Stanford: Stanford University Press, 1958. Pp. ix, 340. $8.75.

The authors of this book are to be commended for carrying out a highly competent and rigorous analysis of certain inventory and production problems. The mathematical level of the book is such that the vast majority of the readers of this journal will find it difficult if not impossible to follow.

The problems analyzed are quite general. The authors search for policies which minimize the sum of certain costs associated with various levels of production and inventory when confronted by stochastic or deterministic demand. Cost minimization is equivalent to profit maximization, since prices are considered as fixed and not controlled by the decision-maker. Among the costs considered are those of producing or ordering, costs of storage, and costs of stock-outs. The solution of these problems is of considerable practical as well as theoretical interest.

Unfortunately, from the standpoint of the economist, the authors have made little effort to relate their results to economic theory, except for a brief portion in the introductory chapter drawing the parallel between inventory theory and the transactions, precautionary, and speculative motives. The relevance of the authors' analyses for microeconomic theory receives extremely little attention. Although this is not the purpose of the book, the economist cannot help but be disappointed at the lack of concern with problems of interest to him. The pure mathematician also finds little of great interest to him. However, applied mathematicians, mathematical economists, and operations researchers should find the book interesting and relevant. For the nonmathematical reader, the second chapter provides a description of the nature of the problems considered and the techniques used to solve them.

Part II analyzes optimal production and inventory policies in deterministic processes. It deals with problems involving production over time with increasing marginal cost, production smoothing and optimal production plans without storage. Some of the equations contain familiar results such as the difference between the marginal costs in two periods equaling the cost of carrying stocks from one period to the other (p. 63). However, many of the cases contain phenomena usually not considered in traditional economic literature, such as the requirement that initial inventory plus cumulative production must be at least equal to cumulative sales at any point in time.

Part III is concerned with optimal policies in stochastic inventory processes, demand being the primary stochastic variable. The following example may serve to give some of the flavor of the analyses. Assume that one wishes to determine the optimal stock level for a single period when demand is stochastic. A literary economist might suspect that the optimal policy would involve equating expected marginal cost and expected marginal revenue. In most cases, his suspicions would be confirmed. However, mathematical analysis includes, in addition, various pathological cases (e.g., among others, a zero or infinite optimal stock) which do not lead to an unique solution, bringing to light the hidden assumptions often implicitly made by the economist. It is to be hoped that the same rigorous microscopic analysis may be extended to other areas in economic theory, although such rigor is necessarily achieved only at the expense of much tedious detail.

The final portions of the book are concerned with operating characteristics of inventory policies, expecially policies of "simple form," which are optimal under many plausible sets of circumstances. Their conclusions are of interest from the point of view of business applications, although even some of these "simple" rules are presented in a more complicated form than most that have reached the application stage.

The book ties together many loose ends of analysis in inventory and production theory. It is recommended reading for those with a good mathematical background who are interested in problems in this area.

T. M. WHITIN

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*Inventories and the Business Cycle with Special Reference to Canada.* By CLARENCE L. BARBER. Toronto: University of Toronto Press, 1958. Pp. xii, 132. $3.50.

From the time of the Classical School's pronouncements concerning the relationship between circulating capital and economic crises, economic analysis has made great strides; but the activities about which economists theorize have increased greatly in number and complexity—possibly as much or more than our advances in theoretical understanding of what goes by the name of business cycle. Inventories have long been suspected of being one of the crucial elements in the problem of cycles, so we are fortunate in having another contribution towards an understanding of their rôle in economic fluctuations.

The present volume originated, as have many notable works, in a doctoral dissertation. Professor Barber starts with a doctoral candidate's summary of the literature and of the diverse findings in his special field. He

then goes on, in keeping with current methodological procedures, to develop a model relating investment in inventories to equilibrium levels of income. The model is based, essentially, on Keynes' famous formulation of the relationship of income to consumption, investment and saving. This is the major theme in Part I, but even in this part Barber's discussion breaks away at times from a strict preoccupation with his models, and he gives some penetrating comments on the problem of inventories as seen from empirical evidence.

In Part II, on "Inventory Fluctuations in Canada, 1918 to 1950," the author describes certain industries and their inventory experience, and then he examines the entire economy and inventory data. He describes different forms of inventory and differences in major sectors of the economy such as manufacturing and agriculture. He brings out the possible divergence between the value and the volume of inventories, showing how different accounting formulæ affect inventory values and profits. He shows that inventories are only one form of investment and gives its relative significance in the Canadian economy. He mentions the inventory investment-bank loan relationship; and he indirectly raises the issue whether an increase in inventories may not be the effect as well as a cause of the change in the level of economic activity. He also shows some interesting differences between the Canadian and the United States economy in cyclical behavior and in inventory policy. The latter seems a wonderful field into which to probe further.

The number and intricacy of the factors impinging on inventories, which the author clearly brings out, show that much more spade work will have to be done in this field; and, to the reviewer at least, indicate that it may be a little premature to spend too much time building macroeconomic models for inventory-business-cycle analysis. We still have a lot of agonizing work to do in formulating the questions we want answered and in bringing some order out of the complex diversity of phenemona. Barber's study will be an aid to others and should lead to further work.

RICHARD C. BERNHARD

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*Risparmio e ciclo economico*. By GIANCARLO MAZZOCCHI. Milan: A. Giuffrè, 1957. Pp. 168. L. 1200.

This volume on savings and business fluctuations is concerned with the interesting problem of the way in which cyclical movements may be modified by one form of personal savings, the contractual. This category which appears to be showing secular growth, as Goldsmith has shown in his monumental *Study of Savings in the United States*, is regarded by the author as the sum of life insurance premiums, contributions under private pension plans, and mortgage payments. (Instalment payments for consumer durables, which now commonly run as long as three years, are excluded.)

Mazzocchi's analysis, based on data and technical literature which are largely American and British, leads him to the conclusion that substantial contractual savings, coupled with the investment practices of the institutions which receive such savings, increase the economy's instability. On the savings side, an increase in the ratio of contractual to total savings reduces the marginal propensity to save which, given the multiplier, tends to increase

the amplitude and duration of fluctuations. Such fluctuations are not moderated but actually intensified by the investment practices of what we call financial intermediaries, which favor or are required by law to place their funds in debt instruments. Capital users wishing to maintain a given equity-debt ratio are guided in their use of debt capital mainly by the level of retained earnings. Because the latter are themselves markedly subject to fluctuations, however, savings of the contractual sort move irregularly into investment.

Suggested remedies are chiefly of the fiscal kind. The author does not count on relief by way of monetary policy, because saving is held to be too interest-inelastic. Instead Mazzocchi seems to lean heavily on periodic recourse to a spending tax, under which the authorities could encourage saving during expansions and spending in recessions. On the investment side, he would like to see enlarged facilities for supplying risk capital, particularly to medium and small businesses, in order mainly to minimize the role of unstable retained earnings in connection with investment plans.

Though Mazzocchi presents a closely reasoned and lucid exposition of the subject, which is only sketchily treated above, this reviewer feels that his analytical account is one of a long list of efforts which needs to be rethought in the light of Friedman's basic critique of the consumption function.[1] Contractual savings would appear to take on a different significance, for example, if they were viewed as deriving in some measure from transitory components of income rather than from permanent income. Policy prescription, particularly that of the compensatory variety, seems premature in the light of the unresolved analytical questions.

VIRGIL SALERA

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*Vpliv dohodkov in cen na raven potrošnje prebivalstva v Sloveniji.* (The Influence of Incomes and Prices upon the Level of Consumption in Slovenia.) By VLADO FRANKOVIČ, ALENKA RISMAL, and VIDOJKA KOZAK. Ljubljana: Economic Institute of Slovenia, 1958. Pp. 140.

In October 1957, the Yugoslav People's Assembly adopted the resolution on Perspective Development of Personal Consumption. It recognized the adverse effect of a low level of personal consumption upon the productivity of labor. The resolution called for an increase in the share of national income devoted to personal consumption, and for development of more accurate measurements of fluctuations in real wages and personal consumption. The present book is a contribution to the accomplishment of the latter task.

The study is mainly concerned with: (1) income and price elasticity of demand for main foods as well as for broad categories of personal consumption (food, housing, etc.); (2) the structure of consumption at different levels of income; and (3) minimum cost of food necessary to satisfy qualitative and quantitative physiological requirements.

The analysis is based upon budgets of four-member families in Slovenia, with an income range of 15,000-30,000 dinars. Workers', clerical, and

[1] Milton Friedman, *A Theory of the Consumption Function*, Nat. Bur. Econ. Research Gen. Ser. 63, Princeton 1957; and the published comments thereon, especially Robert Eisner, "The Permanent Income Hypothesis: Comment," this *Review*, December 1958, *48*, 972-90.

agricultural families are treated separately. In determining the combined effect of prices and incomes upon the consumption, the function:

$$(1) \qquad p = \frac{a}{c} + bd \quad (p = \text{consumption},\ c = \text{prices},\ d = \text{incomes})$$

was selected. Income and price coefficients of demand elasticity for 1955 and 1956 were determined by keeping prices and incomes constant. The effect of the prices of substitutes (Slutsky-Schultz theorem) was not considered because of the complexity of mathematical operations, for the authors believe that the results obtained would not have been more significant, considering given economic conditions.

The coefficients of income and price elasticity for various foods are less than unity in most cases. The outlays for food are influenced more strongly by income than by prices. Outlays for clothing are characterized by low income and price coefficients because of relatively low consumption and high prices. The relevance of coefficients for housing is obscured because of the acute shortage of housing and because of noneconomic rents.

The structural analysis reveals a relatively low level of consumption, reflected mainly in a high percentage of food expenditures, which in Yugoslavia take 54.9 and 52.5 per cent of total expenditures (for workers' and clerical families respectively in 1956); in Austria, 49 per cent (for all population in 1954-55); in Switzerland, 38.2 per cent (for all population in 1955); in the United States, 28.8 per cent (for urban population in 1950); in Norway, 27.1 per cent (for all population in 1952-53).

The final two chapters deal with the analysis of the minimal cost of food, considering the physiological requirements. They present the initial results of research, still in progress, conducted by the authors at the Economic Institute of Slovenia. First the characteristics of a typical family were determined on basis of data obtained in the survey; then the daily physiological requirements of this family were found from the tables of the Food and Nutrition Board (U.S.A.), which were adapted to conditions in Slovenia. The basic problem is presented in the question: What type and quantity of food should the four-member family have bought per day, in 1956, in order to satisfy its physiological needs with a minimum of expense? The problem was solved by linear programming, the result giving a rather unvaried menu. To approximate habitual consumption, additional conditions, permitting the inclusion of other foods, were added in two subsequent programs. These additions, of course, increased the cost. Comparison of results obtained in the third linear program with the actual food consumption of surveyed families showed that the existing diet provided sufficient calories, but that it was unsatisfactory from a nutritional standpoint, particularly in the case of the low-income families. A further comparison of food expenditures of total population with the physiologically required minimum showed that actual expenditures will not purchase this minimum.

The study is a valuable contribution to the literature in the not widely explored field of Yugoslav consumption.

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*Ricardian Economics—A Historical Study*. By MARK BLAUG. Yale Studies in Economics, No. 8. New Haven: Yale University Press, 1958. Pp. x, 269. $5.00.

Ricardo was one of the most amiable and obstinate of economists who ever lived. He could hear disagreement with the bland indifference with which saints listen to heretics, and he could receive praise in the way they are said to listen to hymns. Neither meant much to him. He had a set of ideas, and it was the product of his formidable powers of analysis and observation. Mr. Blaug has written a good book about it, and about Ricardo's followers, their opponents, and the ways the system made itself felt. He has stayed close to the texts, and his method is sympathetic and searching: What were the economists trying to do, and how were particular ideas related to their intention?

Ricardo's was to explain the laws regulating the distribution of income. He meant, Blaug explains, the causes of *changes* in distribution as population increases and the productivity of land declines. More labor time is needed in order to produce a given amount of food. Real wages may fall in the short run, but in time will rise to the subsistence amount. On marginal land, the rate of profit diminishes, and the rate elsewhere must fall also. Rent, the difference between the outputs of equal inputs of labor-and-capital, increases. Over the long period, income is redistributed in favor of landlords, real wages are constant, and the rate of profit declines. "The heart of the Ricardian system consists of the proposition that the yield of wheat per acre of land governs the general rate of return on invested capital as well as the secular changes in the distributive shares."

It was the declining rate of profit which would cause the distribution of income to change. A complete explanation required an accurate statement of the labor theory of value and an invariable measure of value. Few who read Ricardo saw the place of those ideas in his system. They enabled him to show that additional investment in agriculture altered the terms of exchange between it and other industry in favor of the landlords. When M'Culloch (who cuts a better figure here than in most histories) proclaimed the harmony of self-interest, Ricardo demurred. "Are the interests of the landlords and those of the public always the same?" he asked.

If one accepts Blaug's statement of Ricardo's intention (as the reviewer does), one must conclude that there has been much said about Ricardo which is inconsequential. He did side with Say and believe that full employment was automatic, but the point was extraneous, and there is no excuse for feeling superior or becoming indignant about the "tyranny" of his system. There have been growth models put together out of elements culled from the *Principles*. They are quite synthetic, and impute a purpose to Ricardo which would have bewildered him. He would have been surprised even by his reputation as a champion of *laisser faire*, because policy was not his guiding purpose.

In that area, Blaug is not at his best. He is reluctant to accept the fact that Ricardo did not believe in free trade, although he describes Ricardo's position very exactly; and he attaches less importance than it deserves to the fact that Ricardo's followers opposed the campaign to repeal the corn laws. The normative ideas of the Ricardians are not as easily integrated as their positive ideas are. About Blaug's treatment of the latter, there is little with which

one can disagree. There are a few indefensible assertions, but they can be set down to haste or polemical zeal.

His book is a fair statement of the system in its positive aspects. Now let us have equally good statements of the others. We need a biography of Ricardo, and if an economist will not do it, let someone like M. St. J. Packe use the material. He told us many things about Mill which were illuminating (*The Life of John Stuart Mill*). Studies of the economists as political figures will help us to understand their ideas about policy. We also need fresh viewpoints like that of Walter C. Weisskopf, in *The Psychology of Economics*, which analyzes the premises of the classicists as well as the reasoning from them. Then let someone put his mind to a vocabulary of economics (a Fraser for the present) as T. D. Weldon has done for politics. Blaug's book will have an important place in such a collection.

William D. Grampp

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*Theory and History, An Interpretation of Social and Economic Evolution.* By Ludwig von Mises. New Haven: Yale University Press, 1957. Pp. ix, 384. $6.00.

With the author's indulgence, an advance estimate of the general professional reception to this work would indeed be low. In the case of many within the economic fraternity a quick, instinctive association between the author's name and his extremist position in the spectrum of contemporary economic thought may be sufficient to cause indifference. The limited interest of professional economists in the subject under consideration would also support a low estimate. However, despite these and other possible factors, a largely negative reaction would be most unfortunate. Obviously, a lack of serious interest in this treatise could not advance and deepen our thinking on some of the most fundamental intellectual issues confronting American minds today.

As a sequel to his tome on *Human Action,* in this absorbing study von Mises attempts to grapple with the fundamental issues of guiding thought. Looked at in its entirety, the work is not merely an intepretation of social and economic evolution; it represents von Mises' personal philosophy based upon years of study, scholarship, and cultured experience. Whatever may be said of his economic "classicism," in contrast to some others of like mind he brings into his expositions a wealth of cultural and historical insights. Although he will not agree with the reviewer's terminology, in the present study he displays dimensions of thought and embraces a broad orientation reflecting the stature of a social scientist with a formal economic background. Von Mises' lucid and often incisive contributions cannot but be readily welcomed. They serve both as a sobering counterweight to many perpetuated socio-economic fallacies and a vibrant stimulus for economists to explore more intensively related disciplines of thought.

The range and nature of subject matter covered in this work make it essentially a philosophical treatise rather than, as the author to some extent believes, a strictly scientific contribution. In examining, for instance, the differences between natural and social scientific methods, the question of ends and means, or the relationship between materialism and determinism,

the author is thinking philosophically, not scientifically. He subtilizes fairly well many such conceptions. But when it comes to a firm and grounded distinction between science and philosophy, none can be found; and confusion on this point persists throughout the entire work. To speak in one place of the "discursive reasoning of the sciences of human action" (p. 4) and, in another, of philosophical interpretations as being "discursive and scientific" (p. 323) indicates a loose handling of terms. When he speaks of philosophy building on "the foundations laid by science" (p. 275), he clearly shows a defective understanding of the independent discipline of philosophy. Science serves philosophy, to be sure, but so does the ever-expanding reservoir of common experience. The linearity and sensory observations of scientific thought as against the inherent discursiveness of philosophical thought and its intellectual penetration into the intelligible nature of things seem to elude the author.

This inability or perhaps reluctance to sharply define the two disciplines is crucial to a critical analysis of von Mises' dominant thesis and his supposedly novel introduction of the two terms "thymology" and "praxeology." His thesis is really a simple one. Indeed, it essentially and boldly projects into the full context of historical process what this reviewer in his work on *Veblenism* has defined as the formal object of economic science. Our chief concern, von Mises contends, is with the choosing of means suitable for the realization of man's ultimate ends. These ends depend entirely on judgments of value which are subjective, voluntaristic, and scientifically indeterminable. We are told that choosing means is a "matter of reason" and choosing ends "a matter of soul and will." Repeated in other words time and time again, the further point is made that: "With almost negligible exceptions, all people want to preserve their lives and health and improve the material conditions of their existence" (p. 270). Capping this philosophical scheme is the individualist utilitarian view that what results in benefits is good and what doesn't is bad. Applying the formula to social systems, one is to conclude then that von Mises' position on pure capitalism, i.e. without governmental intervention, is the true one because of the beneficial results produced by that type of system.

The simplistic character of this philosophical scheme is as unsatisfactory today as it was in John Stuart Mill's day. The existential ends of man include also self-preservation in terms of personal honor, self-perfection, enlargement of experience, and social fellowship to promote common utility. Doubtless, suitable means are important to realize ultimate ends, but a rational knowledge of good ends is equally important for the judicious determination of suitable means. And the good in general is the perfection that is proper to a thing. Such knowledge unavoidably necessitates the systematic study of metaphysics. It helps von Mises little to assert that man cannot do without metaphysics and then, quite erroneously, claim that it has no significance for "a logical examination of scientific problems" which, in this work, are almost entirely philosophical in character. The nature of man, which is common to all men, lends itself as an object of rational inquiry just as any other being in existence. Mere reiteration of subjective "feelings, tastes or preferences" by the author doesn't brush this truth aside.

The author's defense of the individuality of man, the primacy of ideas and reason, and the basic freedoms is impressively stated. His criticisms against positivism, historicism, behaviorism, and panphysicalism are logically sound. Notions associated with these movements have afflicted the social sciences, including economics. But the treatment offered by von Mises is no more formidable than were the unsuccessful attempts of the nineteenth-century liberals to stem the tide of scientific materialism. Were he familiar with the full structure of Western thought, whence these ideas on man, reason, and freedom flowed historically, his arguments would be strengthened considerably by more basic ideas rooted in metaphysically perceived reality.

On this point the book shows a gross unfamiliarity with the *Philosophia Perennis* formed in the medieval period. Inaccurate statements on the origin of the idea of the common weal (p. 30), the relationship between collectivist philosophy and medieval realism (p. 250), and the beginning of a stress on the problem of liberty and bondage by seventeenth century philosophers amply attest to von Mises' blind spots. There are many more, some of an exaggerated and wild nature. What he calls thymology has formed the content of philosophical psychology long ago; what he calls praxeology has also for centuries formed the empirical basis of social ethics. New terms do not solve essentially old problems. And socialism, for which, significantly, the utilitarian doctrine has been ably used, is not the only possible outcome of our efforts to preserve the good of the market economy and to reconstruct our institutional arrangements on the basis of durable principles of socio-economic order.

LEV E. DOBRIANSKY

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*Historia rozwoju ekonomiki.* (A History of the Development of Economics.) By EDWARD TAYLOR. Poznan: Panstwowe Wydawnictwo Naukowe, 1957/58. 2 vols. Pp. xii, 258; 385.

The intellectual ferment which accompanied the October 1956 events in Poland was heavy with promise of valid contributions to knowledge, not least in the field of economics. The results to date have been disappointing. A striking exception to the run of mediocre economic journalism is Professor Edward Taylor's two-volume textbook on the *History of Economics*, of which the first volume appeared in October 1957, and the second in February 1958. A third volume devoted to the development of socialism, has been promised. The first two volumes are an eloquent tribute to the tradition of the University of Poznan as a center of Polish economic thought, and to the scholarship and academic integrity of the author. Here is a clear, incisive, well-balanced and comprehensive manual worthy of the attention of students of the subject, and crying out for translation into at least one of the world languages.

Taylor held the chair of economics at the University of Poznan from 1919 till 1949. In that year he fell victim to the purge of the allegedly "reactionary" instructors carried out by the communist government, and was not reinstated until the fall of 1956. Prior to his compulsory retirement and at the request of his students, Taylor in collaboration with one of his assistants, J. Tetzlaw,

prepared a revised script of his lectures in the history of economic thought, containing the substance of thirty years of scholarship and pedagogical experience. His retirement from active teaching in the years 1950-1956 provided him with occasion to thoroughly re-examine, expand, and rewrite the original lectures in book form. Although it bears the marks of its lecture parentage, the result is a refreshingly clear account of the development of economic thought from ancient times to the present day. This is not just another textbook on the subject; it ranks among the best manuals on the subject currently available on the international market.

The peculiar circumstances of their inception account for a number of the unusual features of the first two volumes. The "economic underworld," all that ballast of ideologically tinged socio-economic movements, is ruthlessly eliminated from this portion of the book. The removal of the untouchables of Marxist economics (and, by extension, of the whole host of utopian socialists and early collectivists) seems to have been a prerequisite for an objective treatment of the bulk of economic theory. This method has a surprisingly purifying effect on the exposition of the logical structure, evolutionary process, and continuity of theoretical economics. The voices-off-stage, significant as they may be, have been put on a separate record. The reader's attention is in this way concentrated on the world of ideas, on the disinterested pursuit of an explanation of economic phenomena, with only slight distraction from the historical facts of the epoch in which these ideas originated. Whether such procedure is to be considered as the book's weakness or strength is irrelevant in view of the circumstances under which it had been written and published. One example of the method may be of interest: born in the midst of a communist world, the book refers to communism once in all its 643 pages, and this only in connection with the ideological stand of Professor Oscar Lange.

Taylor attempts, successfully, to combine the two methods of presenting the history of economic ideas: he both outlines the logical development of particular economic problems, and presents a coherent chronological arrangement by authors. Major stress is laid on the post-1870 period. This is, without doubt, the most interesting and thorough part of the work, reflecting a great effort to convey clearly and concisely, yet without sacrifice of the essentials, the original contributions to the science of the last eight decades. As a contribution to the understanding of the issues involved, Taylor's exposition of the economic thinking of this period stands well nigh unique.

JAN S. PRYBYLA

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*Ekonomi och religion.* By KURT SAMUELSSON. Stockholm: KF's Bokförlag, 1957. Pp. 175. Sw. Kr. 11.

This essay is a vigorous attack on Max Weber and the propositions first advanced in *The Protestant Ethic and the Spirit of Capitalism.* It argues, first, that Protestant thought had little or nothing to do with the "spirit of capitalism" and, second, that there was no significant correlation between religious changes and the post-Reformation development of capitalism.

After an interesting recapitulation of various contributions to the half

century of debate over the Weber thesis, Samuelsson examines the attitudes of the reformers toward wealth and economic activity. He concludes that puritanism, far from being the source of the capitalist spirit, was antithetic to it. Most of the arguments of this section have been advanced by earlier critics of Weber and nearly all of them were either anticipated by Weber or answered in the notes to the 1930 English edition of *The Protestant Ethic.*

Samuelsson does not seem to understand the distinction, clearly formulated by Weber, between the immediate intentions of the reformers and the ultimate consequences of their ideas. His citations of the sermons of Fox and Wesley on the dangers of wealth miss the point that such sermons became necessary when relatively poor men had become rich in the practice of their religion. No evidence supports his contention that the reformers trimmed their doctrine to suit capitalist audiences or that businessmen changed their religion to fit their economic practice. Such allegations suggest an inadequate acquaintance with early Protestant history and imply an underlying conviction that religious attitudes could not have had much influence on economic behavior.

Previous discussions of the Weber thesis have, according to Samuelsson, been beside the point because they failed to ask whether there was in fact any correlation between Protestantism and economic progress. He embarks on a comprehensive historical review to demonstrate that the answer is negative, but his country-by-country analysis brings forth nothing new, and his interpretations are not impressive. England and Holland are shown to have experienced economic progress before the Reformation, but there is no discussion of varying rates of economic progress. The most prosperous periods in Switzerland and Scotland appear to have come after the decline of Calvinist influence, but this would be consistent with Weber's conception of the role of puritanism. Only a naïve view of historical relationships could accept the conclusion that such "discoveries" have undermined the foundation of the Weber thesis.

Samuelsson seems to believe that the idea of a relationship between piety and progress originated with Weber rather than in the comments of contemporary observers and subsequent scholars. His attempt to demolish the "starting point" of the Weber thesis is unconvincing, although he does unearth a typographical error in the Weber-Offenbacher statistics of Protestants in institutions of higher education in Baden.

In view of the sweeping nature of Samuelsson's charges, it is appropriate to recall the limited and precise aim of Weber's original essays. He sought to explain how an attitude (the spirit of capitalism), which had been ethically suspect in highly capitalist centers of the fourteenth century, could have been regarded as the essence of moral conduct in backwoods Pennsylvania in the eighteenth century. His conclusion that the development of Protestant religious thought had much to do with the change in values will survive the challenge of Samuelsson's provocative essay. Weber's subtle and erudite discussion of his limited theme still excels most of his critics even on points they think they have discovered.

J. WILLIAM FREDRICKSON

*North Park College*

## Economic History; Economic Development; National Economies

*Possibilities of Economic Progress*. By A. J. Youngson. New York: Cambridge University Press, 1959. Pp. x, 324. $6.00.

Professor Youngson has done a good deal more than contribute just another tome to the current outpouring on economic growth. His study is a landmark pointing the way by which the accretion of knowledge on economic growth must occur. He has thoughtfully combined the theoretical literature on economic growth with the insights derived from four case studies in economic history. The vast literature on economic growth has provided something less than a satisfactory body of hypotheses on the subject. It is likewise evident that economic history has contributed little to the field. The difficulty is that economists concerned with growth have paid far too little attention to the historical experience of economies, and that economic history has provided all too little of use to the student of economic growth. Yet clearly economic history should be the basic research field for economic growth if progress is to be achieved. Youngson's study gives an indication of the promise of such an approach. It also inevitably reflects the current shortcomings in these two fields.

Part I includes an able discussion of the difficulties involved in the meaning and measurement of economic growth whether conceived in welfare or productivity terms. In successive chapters he develops a series of hypotheses about economic growth with attention focused on three aspects: (1) invention and the adoption of new ideas (particularly as they lead to further innovation and investment); (2) the supply of enterprise; and (3) the "drift towards massive production" (primarily as a consequence of improved transport and economies of scale).

The four case studies in Part II on the acceleration of economic progress are: Great Britain, 1750-1800; Sweden, 1850-1880; Denmark, 1865-1900; and the Southern United States, 1929-1954. In Part III the author summarizes some of the conclusions that emerge from the application of the hypotheses developed in Part I to the case studies in economic history. While recognizing the diversity and certain unique aspects in each case, the author suggests that three important conclusions emerge. The first is the decisive role that involvement in the international economy played in each instance (in the rest of the United States in the case of the regional study of the American South). The second is the importance of agricultural reform, and the third is the complex interdependent pattern that development takes. Youngson also has some general comments drawn from the four studies on sources of capital and population and labor force changes.

In the final chapter some suggestions for the present are offered, drawn from the previous material. The author concludes that countries seeking accelerated development should: (1) think carefully about extending their international contacts (despite the risks involved) rather than conceive of development in national terms; (2) conceive of the role of government less in terms of undertaking specific development projects and more in terms of improving "the general conditions and equipment of economic life"; and (3) devote particular attention to agricultural development.

As noted above the shortcomings of the study are inherent in the state of the fields of economic growth and economic history. Until a better understanding of the interrelationships involved in economic growth is developed it is hard to be convinced that the strategic elements upon which attention is focused are indeed the critical ones, and the literature on economic growth is strikingly deficient in exploring these interrelationships in the past development of economies. The historical evidence suffers from the fact that it is too skimpy to bear the burden of proof and reflects the fact that economic historians have been derelict in not developing either the statistics or the generalizations about the development of economies which are needed for such case studies. Progress along the lines Youngson envisions initially entails fundamental research in both directions. His study points the way.

DOUGLASS C. NORTH

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*Economic Planning in Underdeveloped Areas: Government and Business.* By EDWARD S. MASON. New York: Fordham University Press, 1958. Pp. viii, 87. $2.50.

In this second series of Moorehouse I. X. Millar Lectures, Professor Mason has not lighted a very clear path for those interested in an answer to the question: How should the United States proceed in its efforts to raise per capita incomes around the world? Perhaps this is as it should be, but one cannot help but wonder if more light, as well as heat, would not be provided by the development of a definite stand. I am convinced that this approach would be much more helpful to the political leadership of the underdeveloped nations. I doubt that one can be all things to all people, and I am afraid that Mason attempts to be in these lectures.

The first and second lectures point out in general terms the underlying conditions in countries currently seeking rapid development from a low-income level. These prevailing conditions are then related to the pre-industrial revolution conditions in England, Japan, and the United States. The differences highlighted in this comparison are pessimistic, as they should be. The points included are the helpfulness of the legal system, the per capita income, the level of education, and government efficiency. The evaluation does not consider other comparisons that are also pessimistic. Examples that come to mind are birth rates and death rates, population base, consumption propensity, and political stability.

The third and fourth lectures consider government initiative in general and in Southeast Asia. The points developed in these two lectures give the impression of a strange indecision or ambivalence. On page 51, government is given a dominant role because of "technological and psychological requirements . . . and probable sources of funds. . . ." When actual government performance is discussed, such statements are made as "inevitable limitations of governmental capacity to manage and control" (p. 69); referring to India and Pakistan, "Whatever has been or can be said of the private sector, its record to date, if the projections of the various plans are to be taken seriously, has been substantially better than that of the public sector" (p. 75); and, again, referring to these two countries, ". . . despite recent increases in taxation, tax contributions to development will be small or negative" (p. 76).

Reconciliation, of a sort, of the dichotomy is made in the next to the last paragraph, where it is concluded that "large governmental participation in Asian development programs [may be] a temporary phenomenon associated with early stages of economic growth. . . ." Why this might be the situation is not explained beyond noting that it happened in Japan.

The title is much too broad, for concrete examples of the current development problems are drawn almost entirely from South and Southeast Asia. This was perhaps unavoidable because of the limits set for the lecture series, but a greater effort to select concrete examples giving an insight into cultural, environmental, and economic differences would have contributed much to justify the title and to increase true reader understanding.

However, these lectures do provide, in capsule and understandable form, a view of many of the relationships and current ideas (including basic reference sources) associated with the acceleration of economic development in low-income areas. This is the most useful aspect of the volume and certainly justified its publication.

R. W. LINDHOLM

*University of Oregon*

*Saggio sullo sviluppo economico dei paesi arretrati.* (An Essay on the Economic Development of Backward Countries.) By VITTORIO MARRAMA. Turin: G. Einaudi, 1958. Pp. xii, 396. L. 3500.

This book serves, *inter alia,* as a critique of the best writings dealing with the economic development of underdeveloped countries. Its value as a reference as well is incidental. Though not a profound treatise, Marrama's work nonetheless represents an authoritative contribution to the subjects of economic development theory and policy.

Part I analyzes basic concepts such as underdevelopment, backwardness, and poverty; real per capita income, productivity per man-hour, and other indicators of the well-being of a people and the development level of their country. The problem then brought into focus is: Underdeveloped countries are characterized by average low per capita income and a high concentration of total income in the hands of a few.

Part II deals with factors contributing to economic backwardness. After noting the important extra-economic forces influencing development, such as customs and habits of peoples, their history, religious beliefs, and political and social systems, the author goes to the heart of his study. This concerns three decisive economic factors which are characteristic of underdeveloped countries: limited capacity to accumulate capital; limited capacity to import; and improper utilization of available investment resources and import capacity.

Underdeveloped countries lack the adequate infrastructural capital assets (roads, railways, electricity-generating plants, etc.) essential for their development. Moreover, the few manufacturing enterprises that may exist in these countries are isolated (i.e., outside of a general industrial environment) and do not benefit from external economies. Thus, the ratio of capital to output is greater in the underdeveloped than in the advanced country. Consequently, a greater investment increment is needed to insure a given increase in output. The low level of productivity, income, and savings renders capital accumulation difficult, and external assistance is therefore necessary to help to break the vicious circle.

Capital accumulation required for development in such countries depends primarily upon imports of equipment, etc., acquired with export earnings. Most underdeveloped countries are exporters of primary products, violent short-run fluctuations in the prices of which influence the countries' capacity to import. Prices of primary products have also suffered a secular decline. These backward countries have experienced a diminution of their capacity to import from decade to decade.

The final decisive factor that has contributed to the backwardness of these countries has been the utilization of investment resources and import capacity for unproductive purposes. The high rate of population growth necessitates diversion of a disproportionate share of resources to investment in dwellings. Since the marginal propensity to consume is high, the total of net savings is low. Both public and private investment of meager resources tends to be imitative of rich countries and goes into such unproductive projects as ornate public buildings, country clubs, luxury resorts, etc. The imitative principle applies also in the consumer-goods field, "interdependence of consumer preferences." The inhabitants of poor countries inordinately import luxury goods, travel abroad on expensive tours, and maintain savings in idle balances.

Fundamental economic development policy goals and measures for their attainment are discussed in Part III. The primary goal should be that of utilizing available resources so as to accelerate income formation. Among alternative avenues of investment those that promise increased production over the short term, or those with a low ratio of capital to income should be selected. Stimulation of savings is essential to capital formation, but since savings are insufficient in underdeveloped countries, capital must also be obtained from abroad through the World Bank, the Export Import Bank, etc.

The problem of increasing import capacity should be attacked by stabilizing world market prices for primary products through international control of supplies and, on a national level, by improving quality and types (diversification) of primary products for export. Other effective policies are utilization of seasonal and hidden unemployed in productive public projects, industrialization to provide substitutes for imports, and agrarian reform. Finally, policy should aim at redistribution of income without hampering the propensity to save. In backward countries this is necessarily identified with the reform of regressive tax systems.

Part IV considers various programs for rational economic development; the difficulties arising in their choice and implementation; an evaluation of their balance-of-payments and income-redistribution effects; the advantages and disadvantages of imput-output analysis for devising practical programs; criteria for comparing the income-producing capacity of various types of investment; and the problem of establishing priorities for implementing individual projects. Finally, an Appendix is devoted to a consideration of the economic development problems of Southern Italy as a case study. The author employs theoretical tools and statistical data with the skill of a mature and experienced researcher. The clarity and realism of exposition make the reader aware of the many factors which qualify the findings of abstract analysis. For example, he attacks the doctrinaire orthodoxy of neo-Malthusians who place too much emphasis on overpopulation as a negative factor contributing to

underdevelopment. Their formula for developing backward countries, namely "accumulation of capital" coupled with "birth control" is much too simple. They fail to identify the true obstacles to development. Excess population is but a symptom of the failure of backward countries to use economic resources properly. Highly industrialized countries have absorbed population increases without lowering their standards of living. Moreover in the 19th century these countries experienced a greater rate of population increase than did the backward or poor countries.

On the whole, this volume is a worthwhile addition to the evolving literature on the economics of development. It places at the reader's disposal the benefit of the author's research over the period of a decade on location in many backward countries of the world. The mass of difficult technical material is well organized and cast in an unusually facile style. It should be read by every economist and sociologist concerned with economic development problems.

NICHOLAS M. PETRUZZELLI

*Washington, D.C.*

*Sviluppo economico e produttività del capitale.* By AUGUSTO GRAZIANI. Naples: Eugenio Jovene, 1957. Pp. 136. L. 1250.

Augusto Graziani has added another thoughtful well-reasoned study to the growing list of volumes on economic development and the productivity of capital. Unfortunately there is nothing very new about what he has to say.

The author has attempted to find out how the productivity of capital varies in the long run in conjunction with the general theory of economic development. He considers short-run variations and business-cycle fluctuations only briefly. The treatment is orderly and well planned. After a couple of introductory chapters, Graziani develops a model of economic development, discusses two types of investment, the place of natural resources in development, the impact of technological changes, the effects of changes in distribution and the theories of Marx, Ricardo, Keynes and Hansen in this area. He concludes that "in the period of modern economics, the productivity of capital shows no tendency to decline" (p. 122). This is hardly a startling discovery. Also he says (as did Marshall) that the productivity of capital and its progress in development must be studied dynamically, not statically. Then, like Marshall, he proceeds to treat the problem in the usual static manner.

Graziani makes some helpful comments about the exhaustion of raw materials (always excepting land, which he considers inexhaustible). He says modern technology steps in with some newly discovered material, which replaces or improves upon the older raw material. Also in commenting on the stagnation theory, he says, "Although the theory of economic maturity is logically correct [?], it is based on two hypotheses that cannot be found in contemporary economic reality[!]" (p. 119).

The statistical evidence is put into an appendix. Graziani knows the literature very well and his statistics are mostly American. The reviewer has the feeling that the study is a skillful exercise in analysis, but that it is somewhat disappointing in its results.

HENRY S. MILLER

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*British Economic Policy Since the War.* By ANDREW SHONFIELD. Baltimore and London: Penguin Books, 1958. Pp. 288. 85¢.

Ever since the publication of Harrod's *Are These Hardships Necessary?*, the question of the adequacy of the level of capital formation has been in the forefront of all serious discussion of Britain's postwar economic problems. Andrew Shonfield, who is economic editor of the *Observer*, has made it the central issue of his highly polemical essay *British Economic Policy since the War.* Shonfield's basic theme is that Britain's postwar difficulties stem largely from the fact that her productive capacity has not grown sufficiently fast in the postwar period. This, in turn, is attributable to three factors: a misguided and disproportionately large defense program; postwar sterling area arrangements which permitted a (virtually) unimpeded outflow of capital from Britain to outer sterling-area countries; and the tendency of the postwar governments to cut back on the investment program whenever restraint in domestic expenditure was required to stay an intolerable drain on the reserves. The first two factors allegedly deprived the investment sector of resources while the third exercised a depressing effect on the marginal efficiency of capital. A large part of Shonfield's essay is devoted, therefore, to a plea for a reappraisal of policies in these areas to create a more hospitable environment for investment.

Shonfield describes his essay as concerned with "politics looked at from an economic viewpoint" (p. 11). To the extent that he emphasizes the economic costs of political decisions his characterization is appropriate. This reviewer has qualms, however, concerning the manner in which he arrives at his conclusions. By and large, Shonfield thinks it is sufficient to indicate that the economic costs of certain policies are high to condemn them. This procedure, however, is inappropriate. A valid analysis requires the development of criteria which would permit us to measure the marginal economic costs of political decisions against their marginal noneconomic advantages. In the absence of criteria of this nature, it is not permissible to conclude that because the military program or the sterling area arrangements tended to depress investment, these policies were inappropriate. As important as a high level of capital formation is, it cannot be assumed a priori that it must take precedence over everything else. While Shonfield may be correct, his conclusions remain mere assertions and do not logically follow from his premises.

The assertive nature of Shonfield's essay is also apparent in his blanket condemnation of British postwar governments for their general policy of restraining investment during crisis periods. He displays no awareness of Harrod's and Nurkse's analyses of this problem. Apart from the fact that capital formation was more readily controlled by the government than other components of domestic expenditure, the desire to maintain reasonably full employment was an important reason for operating on the investment sector. Given the nature of the resources required to produce capital goods for domestic use, domestic consumption goods and exports for which an extensive demand existed, a much larger reduction in consumption expenditures would have been required to effect a given improvement in the balance of payments than in investment demand. This is not to imply, of course, that the decision to restrain investment is beyond criticism. Perhaps too much emphasis was

placed on the maintenance of full employment in the postwar period. The only point I wish to make is that the problem is much more complex than Shonfield suggests in his essay.

Despite these criticisms, Shonfield's essay deserves a wide audience. It is a well-written, provocative and intelligent commentary on important problems facing Britain. And this is considerably more than can be said for much of the current literature directed to the elusive "intelligent layman."

ELLIOT ZUPNICK

*The City College of New York*

*Canada's Economic Development, 1867-1953.* By O. J. FIRESTONE. Income and Wealth Series VII. London: Bowes and Bowes, 1958. Pp. xxii, 384. 45s.

This volume is the result of a venturesome undertaking by Dr. Firestone assisted by two of his colleagues in the Department of Trade and Commerce in Ottawa, T. R. Vout and A. W. A. Lane. It is an attempt to interpret Canada's economic development over the 86 years since the confederation of the British North American colonies in terms of national product, national wealth and growth in the population and labor force. Since most of the relevant official estimates begin with the year 1926, Firestone and his colleagues in Ottawa prepared new and admittedly rough estimates of the following aggregates: (1) Births, deaths, marriages, emigration and net family formation on an annual basis from 1867 to the 'twenties; (2) Gross national product by industry in current and constant dollars for one year in each decade from 1870 to 1920; (3) Gross national expenditure and its major components for each year preceding the decennial census from 1870 to 1920 with the exception of 1880, again in current and constant dollars; (4) Various new price series required to deflate the national accounting aggregates; (5) Various supplementary series including the value of reproducible capital in key industries.

Conceptually, all of these aggregates are obviously useful in the interpretation of the country's economic development, but I do think the author errs in attempting such an interpretation on the basis of these pioneering quantitative results. I would have preferred a bare presentation of the estimates and a more extended discussion of the sources and procedures. The estimates are Firestone's significant and valuable contribution and their revision will indicate, as Simon Kuznets states in the preface to the volume, ". . . that Dr. Firestone and the others have provided stepping stones for further work in the field—an indispensable objective for an activity in which only cumulative and cooperative effort can yield significant results" (p. xv).

Users of these estimates will want particularly to read Sections 11 and 12 (Part III, pp. 235-326) in which the limitations of the data, the sources and the chief methods used are described and the basic tables presented. Part IV, which concludes the volume, is an historical review of earlier income and wealth estimates for Canada which should prove very useful to other investigators of Canada's growth.

Rather than attempt to discuss differences in the interpretation of Canada's economic development, it is more useful here to indicate two or three of the

major errors or inconsistencies—or, at least, what appear to me to be errors or inconsistencies—in the estimates.

According to the estimates, national income per capita in Canada increased at an average annual rate of 1.59 per cent from 1870 to 1890, 1.95 per cent from 1890 to 1910, but at a *zero* rate from 1910 to 1930 (p. 171). I find this hard to believe. There are official records of wages rates and of the cost of living from 1913 on. These data show an average annual rate of growth in real wages of 1.70 per cent from 1913 to 1930. This comparison suggests that Firestone's benchmark estimate for 1910 may be too high. His next benchmark is 1920 and his series ends in 1925. It is *assumed* that the series is consistent with the official estimates which begin in 1926 and the two discrete sets of estimates appear in numerous tables without comment on the problem of levels up to 1925 and since 1926. I am, therefore, particularly inclined to distrust comparisons of years before 1926 with years after.

Firestone's benchmark estimates of gross national product for 1870, 1880, etc. are based upon decennial censuses. Annual interpolations cannot add *new* information as to the *levels* of the series. Levels are determined as accurately as possible by the benchmarks. Annual interpolations that are then averaged can change the apparent level at different points in time significantly and undesirably. Firestone's method of interpolation is wrong in principle. Interpolators were constructed for each major component—consumption, domestic investment, etc.—and then combined with fixed weights. Among the results which I would question is a level of GNP in constant dollars at a considerably higher level during the latter 1870's than during the earlier '70's (p. 276), although Firestone himself refers to "a severe economic slump from 1874 to 1880 . . ." (p. 146).

There are also some major anomalies in the population estimates. The official record of birth registrations dates from 1920. Firestone's annual estimates for the years 1870 to 1920 are too low. The benchmarks are again tied to the decennial census. The estimated absolute level of births in each year preceding the census is equal to the reported number of children age 0 to 1 plus half of the reported deaths to children age 0 to 1 in the 12 months preceding the census. Both totals are undernumerated in the census by at least 15 per cent. My guess is based on the following:

Firestone shows a crude birth rate for Canada of 33.7 in 1870. As a matter of firm record, the crude rate for the Roman Catholic population of Quebec in that year was 45 per thousand. Weighting them as 1 of 3 in total population implies a crude rate of 28 per thousand for the rest of the population. The Canadian rate has been that high in recent years. In an unpublished paper "Long Swings in the Growth of Population and in Related Economic Variables," Simon Kuznets shows a crude rate for the United States in 1870 of 43.5 per thousand.

As a further check one can estimate the crude rates for the province of Quebec on Firestone's method for 1870, 1880 and 1890 and then subtract from this estimate the recorded births of the Roman Catholic population of Quebec. This yields total births for the remaining population of the province, who were 14 to 16 per cent of the total, of 246 in 1870, minus 2900 in 1880,

and minus 1425 in 1890. Since Firestone applied his method to all but the Roman Catholic population of Quebec, the underestimate originating in the underenumeration of the census applies to all but the Roman Catholic population of Quebec. By a similar method one can check Firestone's implied crude death rates for non-Catholic Quebec and the rest of the country. The rate for 1870 is 15.6 which appears to me to be too low. The Canadian rate in 1920 was 13.3. Kuznets' estimate for the United States in 1870 is 25.7.

One would expect the underenumeration of total deaths in the census to be relatively less than of deaths to infants and of children age 0 to 1. This expectation is borne out by Firestone's estimates of net migration which were obtained as a residual. For example, his estimate of the net out-migration from 1870 to 1900 is 175,000. This estimate is much lower than the estimate of 472,000 obtained by Nathan Keyfitz ("The Growth of Canadian Population," *Population Stud.*, 1950-51, *4*, 47-63). Keyfitz used English life tables to project Canadian life tables to the earlier census years.

These few comments will serve to illustrate the hazards of taking individual series too seriously. Some of the series do stand up very well in the uses that can be made of them. In addition to the stimulation of further work in this field and subsequent revision and improvement of the estimates, Firestone's estimates, like all contributions in this field, have another valid use. If a wide variety of these historical data appears to support particular hypotheses, one's willingness to entertain them seriously is strengthened even though the individual series are known to be in various ways imperfect. In the words of the Preface, "One can only express deep appreciation of the contributions thus made to the stock of our knowledge on the quantitative aspects of economic growth. . . ."

KENNETH BUCKLEY

*University of Saskatchewan*

*The New Economy of China*. By GYAN CHAND. Bombay: Vora & Co., 1958. Pp. xiv, 429. Rs 16.00.

Described as a "factual account, analysis and interpretation" of the economic development of mainland China, this book represents the observations of an Indian economist who was head of the Economic Section of the secretariat of the Indian cabinet in 1948-51. The author attempts to discuss in sixteen chapters such topics as land reform and the rural economy, the marketing and producers' cooperatives, industrial expansion, labor, commerce and communications, money, banking and finance, Soviet aid, and the population problem. The entire account is meant to produce a "thorough understanding of the processes at work" in the Chinese economy.

In spite of its broad scope, the book can hardly be recommended either as a source of reference or as an unbiased and comprehensive study to introduce the nonspecialist to the Chinese economy. In the first place, there are certain noteworthy omissions both of subject matter and of facts, examples of which are, *inter alia*, the serious impact of losses to Manchurian industry, through the postwar stripping by Soviet troops, on China's economic rehabilitation and monetary stabilization (Ch. 14) and the reappearance of price instability

following the outbreak of the Korean war (Ch. 12). The Manchurian losses, estimated at $2 billion by the Pauley Commission, would, if mentioned, cast a different light on the author's estimate of 4.22 billion rubles of Soviet aid to China (pp. 359-60). More serious, however, is the lack of any informed discussion of the size of the Chinese national product and its allocation between investment and consumption. Although the book was published in April 1958, most of its rather scanty statistical information refers to the period before 1954. Moreover, granted that it is most difficult to separate the political from the economic in the case of this subject, it is nevertheless somewhat surprising to find a scholar naïvely describing the Chinese scene as one in which "democracy as a spirit of true fellowship and cooperation is a reality" (p. 399), or that "the historical fact is that without the Marxist party the benefits, which the present regime has undeniably conferred upon the people, would not have in fact accrued to them" (p. 388). An experienced economist though he is, he has apparently taken at face value much of the official interpretation he received during his six-month stay in China.

The redeeming merit of the book lies solely in its being an interpretation of Chinese economic development through Indian eyes. Even though the author purposely refrains from drawing explicit comparisons between the two countries, for the Western reader the following points are well worth consideration. First, Gyan Chand believes that in China the development of handicraft cottage industry may continue to expand along with that of factory production and that the former will not suffer from the competition of the latter because they serve different consumer markets and because the output of handicraft production is channeled through the trading cooperatives. "The whole program is now being put into operation on the assumption that most of the consumer goods, particularly in the rural areas, are to be produced by the crafts . . ." (p. 188). More recent developments, such as the mushrooming growth of the "back-yard blast furnaces" and the expansion of native industry in the rural "communes," show that the utilization of these industries has been increasingly extended to the production of producer goods. Needless to say, such developments would help to alleviate the problem of rural underemployment. Secondly, in the author's opinion, notwithstanding the Communist rejection of the Malthusian population theory as a direct negation of the Marxist faith, it is necessary to engage in a critical re-examination of population policy. This re-examination is recommended not merely to Communist China, but also to Australia, Canada, the United States and certain Latin American countries (p. 373). The implicit suggestion as a solution of China's population problem is, of course, emigration. If this is a provocative thought, one might note further the author's belief that through trading with the Soviet Union under long-term agreements China has been freed from the effect of fluctuations in world prices and has concurrently been assured of expanding markets for her exports. The Soviet Union, it is said, has been prepared to gear her production "especially to meet the specific requirements of China" (p. 356). Finally, the author asserts that the success of China's economic development to date is largely due to an increase in the vertical mobility of talents (p. 398) and the

large-scale participation by the population in the various facets of economic development (p. 395). Both conclusions are probably true. But the disturbing thought is his contention that all this would not have happened but for the Communist revolution.

YUAN-LI WU

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*Colonial Planning—A Comparative Study*. By BARBU NICULESCU. London: George Allen & Unwin, 1958. Pp. 208. 18s.

This reworking of a doctoral dissertation is a comparative study of economic planning in areas that are or recently were colonies. It deals primarily with British territories, rather briefly with French, and quite skimpily with Netherlands and Belgian colonies. Its coverage is very broad. In about 165 pages of actual text, some seventy-odd territories are "touched upon" and the "development plans of some sixty of these territories have been taken into account" (pp. 13-14).

The book's scope is wide also in that it deals with many aspects of the subject. After a one-chapter introduction, Part II, "Background to Planning," discusses in a largely conventional way a number of problems and possible policies related to colonial economic planning. The topics examined seem to be chosen rather randomly from among a large number that would be of equal interest. Part III consists of a useful one-chapter historical survey treating "The Growth of the Idea of Development Planning for Colonial Territories." In Part IV, "Planning Machineries," the planning process is divided into four stages: (1) collection of the required data; (2) "setting up of planning organizations" or "deciding on priorities" (defined both ways presumably because the planning organizations decide on priorities); (3) implementation of the plan; (4) preparation for the new plan—a transitional stage linking one plan with the next. Part V, grandly entitled "Analysis of the Plans," is concerned chiefly with the problem of priorities in development. The discussion of priorities is centered upon the relative merits of social welfare and economic development expenditures, even though the author subsequently (p. 182) finds it "difficult to accept the existing dichotomy into *economic* and *welfare* planning as economically valid. . . ." The rest of the section touches upon the length of the planning periods, the sources of funds, and rather commonplace discussions of communications and agriculture. The concluding Part discusses briefly some noncontroversial achievements of the plans and some criticisms which, since they are dealt with on such a general level, the author finds difficult to appraise.

In his concluding chapter the author seems to recognize the two major shortcomings of his book. First, the analysis is rather pedestrian; one looks in vain for stimulating interpretations or penetrating insights. "The analysis attempted in the course of this book cannot be said to have revealed any striking new facts or interpretations in connection with development planning in the colonial territories surveyed" (p. 177). Second, the enormous range of the book seems to require generalizations so broad that they have little signifi-

cance. "Plans, their contents, and their effects have varied from colony to colony. General appreciations have therefore of necessity to deal with rather vague overall concepts" (p. 179).

On the other hand, the book provides a serviceable general picture of colonial economic planning. Its discussion of the administrative problems encountered and their effect on the nature of the plans is excellent (Ch. 3 and *passim*). The book is useful for those interested in the economic development of one (or more) of the present or recent colonial areas.

SAYRE P. SCHATZ

*Hofstra College*

*Population Growth and Economic Development in Low-Income Countries*. By A. J. COALE and E. M. HOOVER. Princeton: Princeton University Press, 1958. Pp. xxi, 389. $8.50.

This is another of the major studies of population change in the countries of Asia carried on by the Office of Population Research of Princeton University. Unlike the previous works, which have been historical and empirical, this one is primarily valuable for its theoretical model. India is essentially used as a laboratory test of the theory—which is at the end also briefly tested on Mexico as a country with a quite different starting population base, and with different policies, in order to show its relevance under other conditions. The authors carefully and rightly disavow that the book is either "an appraisal of Indian plans, or a prediction of the future rate of economic growth." (The subtitle of the book—"A Case Study of India's Prospects"—is therefore misleading.) In the course of the test the authors develop various projections of India's population to 1986 and beyond, and present an excellent summary and analysis of India's second five-year plan and prospects for the future growth—but these are really by-products of the theory.

The theory may be summarized briefly although necessarily crudely. (The authors themselves provide such summaries frequently.) Let us assume that the low-income countries start with high birth rates and high death rates which keep the population in a low-income equilibrium. Under the impact of the recent health-improving developments, such as DDT, it is possible to reduce the mortality rates, not slowly (as former theories implied) but very rapidly, and before or simultaneously with industrialization and urbanization. Previously, it was assumed that the decline in fertility lagged behind that of mortality, creating a population explosion; but that gradually a new equilibrium was established at which the greater population was maintained at a higher level of per capita incomes. Now, in the low-income countries the speed of the decline in mortality rates and the delayed reaction in fertility rates is such that the rate of population growth becomes very rapid—on the order of 2 to 3 per cent per annum. During the period of rapid population growth, assuming no migration, there is a diversion of resources from directly productive investment to investment in late-yielding social expenditures to support the growing population—which in the early period is largely below working age. If fertility rates could be reduced, more might be devoted to either direct productive investment, or to higher per capita welfare (health and education)

expenditures. Thus lower rates of fertility have as direct, short-run consequences higher investment, higher per capita incomes, and improved well-being of the population; and as a longer-run cumulative result, continued higher rates of growth of per capita incomes than in the case of higher fertility. This may be crucial for the long-run ability of the country to support the rapidly growing population under either fertility assumption. (In these low-income countries the stimulating effects of population growth from more births as a demand creator in the Keynesian sense, is either nonexistent if the population is already very large, or is outweighed by its effects upon investment.)

As a test of this model, the authors develop a series of population projections for India on the basis of past trends (reinterpreted) and certain assumptions with respect to future mortality and fertility rates. The projected mortality rates are based conservatively upon the experience of other countries, especially Ceylon, which have introduced health programs similar to those already introduced or planned for India. The fertility estimates are on three different assumptions: maintenance of present rates, an immediate decline in fertility, and a delayed decline. These are then used to project total population growth and age composition. (These population estimates, which have been available in India in an earlier draft for several years, have already had the effect of raising previous projections in India for planning; the discussion of the Indian third five-year plan assumes a rise in population of 2 per cent per annum.)

It is assumed of course that India will be able to sustain the projected rates of population growth at some level for a period of time—at least the thirty years to 1986. The authors, on this assumption, and assuming technology about constant and a closed economy, analyze the effects of the varying rates of population growth upon India's economic development. The second five-year plan is used as the basis of this analysis. (Unfortunately events move so rapidly in India that the second plan is no longer a satisfactory basis for projection—however this is not too important as a test for the model, although it would be very important if the primary purpose of the book were a projection of the Indian economy. For example, one area of discussion in this book in which the second plan is a misleading guide is the analysis of the community development programs and agriculture. Use of the second five-year plan as a basis for this analysis makes projections of trends in farm output somewhat too optimistic; this of course reinforces the implications of the theoretical model.)

The economic analysis contains very interesting discussions of certain general problems of underdeveloped areas such as capital-output ratios, investment and saving, and shows an imaginative use of the Cobb-Douglas function for income projection purposes—all useful in general analysis of other economies. This analysis is used to project investment and income in India over the thirty-year period. It is conclusively shown that with lower fertilities a greater proportion of national income would be spent as investment for growth and this results both in higher per capita consumer income, and a higher rate of growth in total output and per capita income during the period. The authors have deliberately assumed, in their economic projections, a closed economy for India. In the long run, but possibly even before 1986 if the projec-

tions of agricultural output prove to be too optimistic, and certainly beyond it, this may prove quite unrealistic. The discussion of Mexico shows that similar conclusions hold in a smaller developing economy with a greater volume of foreign trade than India, and with very different starting conditions.

The authors are to be congratulated on the very neat construction of a theoretical framework and the testing of that framework in a real situation. For the lay reader it may be too logical and dry in its construction and style. However, as an example of methodology, it is almost a model of economic-demographic analysis; in substantive terms it will be necessary reading for anyone interested in the relationship between population growth and economic change.

GEORGE ROSEN

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*Le libéralisme économique et les pays sous-développés: études sur l'évolution d'une idée.* By FRÉDÉRIC CLAIRMONTE. Geneva: E. Droz; Paris: Minard, 1958. Pp. 361. 30 sw. fr.

Dr. Clairmonte has written a scholarly, provocative volume on the relation of classical *laissez-faire* economic theory to the critical need for industrialization of the underdeveloped countries of Asia, Africa, and Latin America. He characterizes as underdeveloped those countries that lack modern technology and the socio-economic superstructure that is built upon that technical base. In so doing, he has many precedents, but theoretically he begs the question as to what general factors or criteria determine underdevelopment. He believes no adequate, general theory of economic growth has as yet been presented.

With considerable expository skill and an intense dislike of British industrial capitalism, Clairmonte portrays "classical economics" from Ricardo to Marshall and Pigou as an honest rationalization of British middle-class economic needs during the nineteenth and early twentieth centuries. He eulogizes the strong defense of nationalism, protectionism, and industrialization by Alexander Hamilton, Matthew Carey, and Friedrich List. List is given the highest praise for his attacks upon British economic doctrines, his role in bringing the *Zollverien* into existence, and his interpretation of history as a dialectical process.

As part of his case against the classical economists and their followers, Clairmonte gives a devastating account of the harm done to India during the eighteenth and nineteenth centuries by British innovations in manufacturing, agriculture, and land-ownership. A good deal of this indictment seems sound. Yet I think the evidence and analyses of other authorities justify the conclusion that British rule in India brought a differential diffusion of modernization: progress in certain key sectors of Indian economy and society, stagnation or retrogression in certain others, with a net long-term increase in real national income that was accompanied by so large a population increase as to keep the per capita real income down to either a stationary level or a slow rise over many years. Moreover, considerable responsibility for the retarded

economic progress in India rests with the native population of India for having preserved institutional obstacles to economic progress and social mobility.[1]

Clairmonte demonstrates the intimate relationship among foreign trade, foreign investment, and the progress in industrialization of the major Western European and American powers in the third section of his book. He admits that foreign investments resulted in the increased welfare of Europe, the United States, Canada, and Australia. Yet he maintains that western capitalists did not tackle the crucial problems or elicit the latent human talents of the native populations of Africa and Asia. This time he singles out nineteenth-century China as an example of how Western factory goods could come in too speedily and injure the handicraft industries of the non-European poor countries. Here a mass of evidence supports his main charge. But Western enterprise also did much to modernize China's industry, transportation, and education and eventually to raise the Chinese standard of living over the previously incredible low level.[2]

In the last quarter of his study Clairmonte rightly attributes the death of *laissez-faire* as the dominant economic theory and the world-wide spread of state-interventionism to the economic, political, and social changes following the first world war, the nationalistic rivalries of the 1920's, and the great depression of the 'thirties. He regards the emergence of the Chamberlin-Robinson theories of monopolistic, imperfect competition and Keynes's *General Theory* as the Western academic recognition of the inadequacies of classical economics in the realms of theory and practice. But he feels that the case for state-interventionism in the underdeveloped countries of Africa, Asia, and Latin America is far stronger than in those highly developed countries where full employment is the main concern of the governments. The crying need of the poor countries, in his eyes, is for industrialization, population control, national integration, and planning on a physical basis. He believes an enlargement of the public sector of these economies is an economic necessity and thinks that the governments of these former colonial areas will not welcome private foreign investments because these might result in high profits for foreign capital and would hinder the drive towards socialism.

The author is conscious of the difficulties in the realization of his project in the Middle East and Latin America from corrupt monarchies and anti-industrially oriented aristocracies; yet he is hopeful of progress. I fear, however, that there are more dangers arising from his emphasis upon speedy industrialization and upon the state as the chief instrument of economic change than he is conscious of. State enterprise and planning are necessary, but it is equally vital in these regions to build up an independent middle class in industry and agriculture if the perils of past despotic governments are not to

[1] Vera Anstey, *Economic Development of India*, 4th ed. (New York, London, Toronto, 1952), pp. 433ff.; Kingsley Davis, "Social and Demographic Aspects of Economic Development in India," and D. R. Gadgil, "Indian Economic Organization" in Simon Kuznets *et al.*, eds., *Economic Growth: Brazil, India, Japan* (Durham, N.C., 1955), pp. 263-315, 448-63.

[2] Cf. G. C. Allen and A. G. Donnithorne, *Western Enterprise in Far Eastern Economic Development* (New York, 1954), pp. 241ff and Marion Levy, "Contrasting Factors in the Modernization of China and Japan," in Kuznets, *op. cit.*, pp. 496-536.

be revived.[3] But there are limits also to foreign government loans from the rich to the poor nations, and private capital investment will have to be relied upon for many reasons, political and economic. I wish that the desirability of emulating the Danish producer cooperatives in backward areas had been stressed and that the problem of world-trade adjustment between the rich and poor countries had been analyzed at greater length.[4] Nevertheless, we are indebted to Clairmonte for a valuable exploration in the history of economic ideas and institutions.

SIDNEY RATNER

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*Poverty and Capital Development in India.* By D. K. RANGNEKAR. London: Oxford University Press, for Royal Institute of International Affairs, 1958. Pp. xii, 316. $6.75.

A record and appraisal of public and private investment in postwar India, Mr. Rangnekar's study includes four chapters on agriculture, two on industry, two on transport and communications, and one each on housing, the level and pattern of aggregate investment, investment and progress under the first five-year plan, and the investment pattern and priorities under the second five-year plan. Chapters dealing with different sectors of the economy are chiefly statistical, with theoretical and other argument interspersed from time to time. In the much more readable final section, analysis of policy plays an increasingly important role, being central to the discussion of the second five-year plan launched in 1956.

Pessimists and critics of Indian economic policy will find much in the book to support their views. Thus, with respect to record and prospects: In spite of a substantial rise in industrial output per capita, average real income was probably still (1957) below the prewar level (pp. 100, 277). Total agricultural production appeared to be lower than before the war; the rise in reported output in recent years has been chiefly attributable to more favorable monsoons and better statistical coverage (pp. 40, 47). Moreover, the rise in the industrial production has not brought with it an increase in factory employment. Urban unemployment has been growing and population pressure on agricultural land has become greater than ever (pp. 80, 104-4, 256). Housing has lagged behind the growing population in both rural and urban areas; even the cities' middle classes are worse housed than some years ago (p. 180 ff.). There are heavy arrears in railway maintenance and replacement (p. 175). If India is to break through the vicious circle of poverty, savings must rise from the 6-7 per cent of national income achieved under the first five-year plan to at least 8-9 per cent, a formidable figure for a poverty-stricken country with a rapidly rising population (p. 216).

Rangnekar's criticisms of Indian policy include: Although the first five-year plan rightly assigned first priority to agriculture, it did not draw up a

[3] Cf. Karl A. Wittfogel, *Oriental Despotism* (New Haven, 1957), pp. 413ff.

[4] Cf. A. O. Hirschman, *The Strategy of Economic Development* (New Haven, 1958), pp. 185ff and C. P. Kindleberger, *Economic Development* (New York, 1958), pp. 238ff.

"sound order" of specific priorities, understressed transport and communications, and put too much emphasis on grandiose, spectacular projects involving excessive costs (pp. 26-27, 110-14, 175-77). The second five-year plan, expressing the ambitions of India's "articulate classes," is much less realistic than its predecessor in that it puts even more emphasis on the spectacular, concentrates on manufacturing (especially large-scale, heavy industry), assumes a rate of saving that almost certainly cannot be achieved, and relies on a degree of foreign aid that Indian policies make quite unlikely (pp. 249-76). Instead of pushing nationalization and reform measures intended to introduce a "socialistic pattern of society," the government should concentrate on providing the necessary "social overhead capital" (an enormous task in itself), encourage private enterprise to undertake industrial and other investment, and so modify taxes and restrictions as to give private enterprise both the means and the incentive to move ahead (pp. 117-22, 240-41, 273-75, 280-81). The private enterprise to be encouraged should include foreign as well as domestic business, since India badly needs foreign capital and expertise and receives very little under its present program (pp. 225-26, 255).

Since the book discusses so many problems, it obviously cannot thoroughly explore them all. Some of Ranknekar's rather sweeping condemnations of Indian policy, however, arouse counterquestions. For example: To what extent would funds which taxes now channel into public investment otherwise go into consumption or forms of investment not especially desired by the planners? What policies could India pursue which, in the present state of the world, would attract large sums of foreign private capital? As Rangnekar points out, this is in part a problem of Indian domestic politics, involving the ambitions and sentiments of India's "articulate classes." But the task of attracting foreign capital might not be simple even if Nehru were free to shape policy as he pleased and were not himself imbued with socialist and nationalist ideas.

The book's major shortcoming, an inadequate factual base, is not attributable to the author: The preface points out that much of the study's detailed information stops with 1951-52, and numerous other passages refer to the absence of data and the "guesstimate" nature of some published statistics. So long as Indian series remain thus faulty, intertemporal comparisons and judgments of economic progress or decay must be very broad or highly tentative. The folly of attempting precise comparisons between Indian and non-Indian levels of real income, investment and wealth is even more obvious, and Rangnekar is cautious when he compares India with other lands (pp. 2-4, 215).

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*Japan's Postwar Economy*. By JEROME B. COHEN. Bloomington: Indiana University Press, 1958. Pp. xiv, 262. $6.50.

Mr. Cohen's record of scholarship on Japan makes the appearance of a new work by him an event to be warmly welcomed. Readers of his past studies[1] will

[1] *Japan's Economy in War and Reconstruction*, Minneapolis 1949; *Economic Problems of Free Japan*, Princeton, N.J. 1952; as well as numerous articles.

not be disappointed in his latest work, *Japan's Postwar Economy* which covers the decade, 1946-1956, a decade in which Japan staged an amazing comeback.

In his preface Cohen describes his study as "a brief analysis of the nature of and factors responsible for Japan's economic recovery and of the economic problems which confront Japan." Actually, however, the focus is on the latter, the "economic problems which confront Japan." One chapter is devoted to analyzing the recovery, five to the major economic problems before the country. Inasmuch as trade is pivotal to the Japanese economy, Cohen spends three additional chapters delineating the character of traditional and probable future trade relations between Japan and the United States, Japan and Southeast Asia and Japan and the Communist Bloc.

The questions which Cohen sees as crucial for Japan are (p. 26): "How to produce more food and fewer people; How to employ 800,000 more persons each year; How can Japanese export prices be made competitive with those of West Germany and of other countries; How can Japan sell a billion dollars more of exports abroad; How can Japan achieve stable economic growth without inflation; How can substantial additional industrial expansion be achieved without bringing on recurrent balance of payment crises."

He builds his analysis on detailed factual data. (Unfortunately, footnotes are at the back of the book so that one is never-endingly turning pages.) Study of the data leads him to feel qualified optimism as to Japan's ability to overcome the formidable problems before her. His analysis is persuasive. In only one area does his presentation seem to this reviewer inadequate. Energy sources are obviously of critical importance to the argument. Cohen considers in turn hydroelectric power, petroleum and coal. Japanese coal production has been singularly slow to recover, and he takes as a datum that little can be looked for from added output of this industry. Undoubtedly he has a good basis for this position but inasmuch as the output of domestic energy figures prominently in his analysis, the reasons for the poor recovery and the probable poor future showing need to be stated.

In the reviewer's judgment, the argument of the book would have been clearer if the first chapter "Asia, Japan and the West," intended to provide setting, had been omitted. The chapter is essentially a collection of facts, and when one does not have the guideline of an argument, one can get quite hopelessly lost in factual data. Further with respect to organization, Chapter 11, entitled "Countervailing Forces" (contervailing to the reform forces of the Occupation), would appear to need another location in the book or it might have been given independent status, such as a journal article. In this chapter, Cohen paints in broad strokes the institutional framework of the economy the contours of which are beginning to emerge now that some time has passed since the ending of the Occupation. Inasmuch as this institutional setting obviously affects the "answers" which the economy is giving, this chapter could well have been placed at the beginning. "Countervailing Forces" does not purport to be a comprehensive summing up of Occupation efforts. It treats in summary form the demise of the military, the changes in the bureaucracy and the *zaibatsu* and the emergence of labor as a political force.

The Appendix contains a most useful reading list on Japanese economic development and current economic problems. Persons who followed events in considerable detail during the Occupation period but have not been in a position to keep up with the current literature will find the bibliography on current materials especially useful.

In 1958 there appeared not only Cohen's book but also G. C. Allen's *Japan's Economic Recovery* (London: Royal Institute of International Affairs, Oxford University Press). Although the two titles would suggest that the authors were treating the same subject (and in the same year), actually there is little duplication. Cohen's study is an analysis of future trends; Allen's work, a summing up of developments in the major sectors of the economy, is descriptive.

Analyzing the possible, probable course of an economy is no small assignment. All of the fields of economics are called into play. Cohen has performed the task with distinction.

ELEANOR M. HADLEY

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*Zagadnienia socjalistycznej industrializacji Polski.* (Problems of Socialist Industrialization of Poland.) By ANDRZEJ KARPINSKI. Warsaw: Panstwowe Wydawnictwa Gospodarcze (State Economic Publications), 1958. Pp. 239.

A young Polish economist provides the first assessment of the policies, implications and results of Poland's 6-year plan, 1950-1955. Karpinski accepts the policy of industrialization on the ground that it ensures a faster possible rate of development of the economy than the alternative concentration on agricultural production; for under Polish conditions the latter would require a considerable intensification of agricultural methods, which would itself be dependent on an industrial background. Existence of an agricultural overpopulation at the outset of the 6-year plan showed also the need to create alternative sources of employment where the marginal productivity of labor would be higher than in agriculture. Thus a policy of industrialization serves ultimately to raise the standard of living of the population, although, particularly during the first stages of industrialization, the relation cannot be one of direct casuality.

The 6-year plan achieved a substantial expansion of total production, and created a basis for further development. Industrial production rose during the period according to Karpinski by at least 120-130 per cent (as against 170 per cent in the official estimate based on gross production), an average growth of 15 per cent per year, which is higher than that of any West European country. The Polish rate of growth in the production of particular commodities, when compared at an equal stage of absolute production, was also higher than that in the Western countries. This achievement was, in Karpinski's view, the result of a high rate of investment possible under conditions of socialized ownership of means of production, and of a concentration on investments in industry to the extent of about 46 per cent of all investments.

The basis for further industrial development has been created by an expansion of key industries: metalurgical, machine-making and chemicals which between them accounted for 51.7 per cent of the total investments in industry.

Karpinski is not, however, preoccupied mainly with listing the achievements of the 6-year plan. In fact, much of his book is devoted to a critical analysis of its mistakes and of difficulties encountered. Instead of the heralded increase of real wages by 40 per cent, they showed virtually no rise between 1950 and 1955, while during the period 1951-1953 actually some decline took place. The plan did not bring about the results expected partly because of much higher costs of investment than those planned: the planned outlays covered only 62 per cent of the planned volume of investments. Industrial production rose at a rate slower than laid out for it in the economic plan. As causes of this inadequate performance, originating partly in too "mechanical a transplantation of Soviet experience" to Poland, Karpinski lists the following:

1. Too high a share of national income was devoted to investment, particularly at the low Polish level of income. The share of gross investment in national income in 1950 prices rose from 22.7 per cent in 1949 to 38 per cent in 1953. Because of the use of regulated low prices for investment goods, the actual share of accumulation in the national income might have been even higher.

2. Improper agricultural policy. While a rise in agricultural supplies is a precondition of industrial development, the policy of collectivization caused serious difficulties and either declines in production or declines in marketable surpluses. At the time when the plan envisaged a rise in agricultural production by 50 per cent and industrialization extended the market for agricultural commodities, the policy of forceful collectivization resulted in abandonment of farms, virtual elimination of private investment in agriculture, and in land being left uncultivated, all this when greater intensification of agriculture was called for. New investment in agriculture was thus left solely to the state, reducing the funds available for the industrialization drive, and causing a general all-round insufficiency of investment in view of the 6-year-plan goals. Karpinski draws from this an interesting conclusion that in an underdeveloped country the solution of the industrialization problem should precede the collectivization of agriculture.

3. Unwarranted dispersion of investment efforts, in part due to the need for state investment in agriculture, and in part shown in the pursuance of too many investment goals in industry. The plan was originally self-contained, embraced a whole investment cycle. Owing to higher than planned costs, to additional and unplanned investments in armaments industry, and to a slow gestation of new investments many projects on which the plan relied for its effects had to be postponed to the following planning period, or even to the 1960's, leaving the investment cycle uncompleted. A sample of completed investment projects mentioned by Karpinski shows an average delay of about two years. In practice completion of an investment project brought also smaller than its planned performance. Karpinski demonstrates that the gestation period of investments was longer in Poland than in the Western countries, and he places the blame for this outcome upon lack of sufficient concentration

of the investment effort and the harmful procedure of uniform reductions in investment funds allocated to a large number of projects, rather than greater concentration of investable funds. He also blames excessive bureaucratism which limits the initiative of investors, an acute scarcity of construction materials, and a low standard of organization of investment work.

4. Excessive concentration of investments in heavy industry. Within the price system implicitly accepted by Karpinski, characterized by low prices of producer goods, the economic effectiveness of investments, measured by the value of investment necessary to bring about a unit value increase in annual production, was lowest in heavy industry and highest in consumer goods industries. As the result of faulty planning and of the armament drive actually over 85 per cent of investment funds devoted to industrial development were directed to heavy industry, at the cost of development of consumer goods industries.

5. Excessive reliance on the construction of new establishments, instead of the enlargement of old ones which, on the average, showed greater economic effectiveness. Exceptions to this general observation constitute enterprises in which technological considerations make expansion of existing plants difficult (e.g., steel mills), or enterprises which use rapidly changing technology.

6. Errors in employment policy, consisting in (a) locating new establishments in labor-deficit areas, (b) precipitating a decline in handicraft and cottage industries, suited to small localities which had characteristically a surplus of labor, (c) causing too rapid an outflow of labor from agriculture to industry, with adverse social and economic effects, and (d) using extensive methods in raising the level of total production, rather than striving to increase the productivity of labor. Karpinski shows that in many new plants labor productivity was substantially below that in old plants.

7. Faulty premises employed in the development of foreign trade, primarily to the effect that the importation of raw materials would decrease with progressing industrialization, that the increase in exports could rely on agricultural products, instead of on a broad export program, and that equilibrating of the balance of payments could be achieved through an extensive anti-import program rather than through the development of exports.

The area of Karpinski's study is broad, his statistical coverage adequate, and his discussion illustrated by many revealing examples. His discussion of the errors of the 6-year plan leads him to the formulation of useful proposals for future economic policy, while he at the same time avoids doctrinaire thinking. Some of Karpinski's conclusions run parallel to changes in the economic policy embodied in the current 5-year plan. Karpinski uses, naturally enough, official Polish statistics which sometimes yield different indicators than Western concepts and statistical methods, and he displays too uncritical an attitude towards the official explanations of economic phenomena (e.g., unemployment statistics), but in the main his analysis and conclusions are the product of a thoughtful effort to explain the immediate past for the benefit of the immediate future.

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*Finansy i sotsialisticheskoye stroitel'stvo, 1917-57.* (Finance and Socialist Construction, Collected Articles.) Moscow: Gosfinizdat, 1957. Pp. 357.

The book under review, published to commemorate the 40th anniversary of the Soviet regime, includes 7 articles. The introductory and basic article, "Soviet Finances for 40 Years," presents a general picture of the development of the financial system of the USSR. This article is of particular interest because it presents and discusses figures pertaining to the military budget of the Soviet Union. It gives an idea of how grossly perverted are the Soviet data on military expenditures. It is well known that the published Soviet budget presents only the direct military expenditures, listed under the Ministry of Defense. These have decreased from 108.8 billion rubles in 1952 to 96.3 billion rubles in 1958, while their relative weight in the total budget decreased from 23.6 to 15.3 per cent. By comparison with countries of Western Europe and the United States, the proportion of the budget in the USSR destined for military use is extremely low.

Actually, however, both the military expenditures and the percentage they constitute of the total budget of the Soviet Union are considerably higher, being in large part included in other sections of the state budget. We do not know precisely how large are the concealed expenditures slated for military purposes. We do know, however, that they exist under such headings as education, various economic and socio-cultural undertakings, or simply under "other expenditures." For example, prior to the second world war (1937-40), of the total amount of capital investment allotted for industrial development, about 30 per cent went into war industries (aviation, ammunition, armament, etc.).

Increasingly large sums are being expended on atomic and other scientific research. Although these activities are directly associated with military potential of the Soviet Union, the necessary funds are included under the general heading of "scientific research." During the past 18 years the budget for this category increased from 1.1 to 15.0 billion rubles, or from 0.6 per cent (1940) to 2.4 per cent (1958) of the total state budget.

Another important factor is that in the Soviet Union there is but one budget that covers the needs of a Union Republic (the largest administrative unit) as well as the budget requirements of a rural Soviet. It includes many expenditures which have no place in a national budget of other countries, such as the financing of various economic and commercial undertakings. Once this factor is taken into account, the 1958 military expenditures of the USSR (excluding the budgets of the Union Republics) amounted not to 15.3 per cent but perhaps almost triple this amount.

During the past 8 years, any decrease in the stated military expenditures of the Ministry of Defense of the USSR may have been matched by an increase in concealed military expenditure items, or simply by a decrease in the purchase price of armaments and military equipment. Although we may only presume that this is the case, it is important to remember these peculiarities of the Soviet financial system.

The next 3 articles discuss the role of finance in the development of Soviet

industry, agriculture and the socio-cultural fields (education, science, medicine, social security). The fourth article stresses the increasing role of the budgets of the Union republics, since the adoption of the reforms which placed a large share of the administrative responsibility for industry and construction directly on the Union Republics. Of the two final articles, "Monetary System of the Soviet Government" and "International Currency and Financial Relations," the latter is undoubtedly of greater interest, since it includes the latest data on the allocation of loans and credits by the Soviet Union.

Theoretical questions, such as price formation and price policy in a planned economy, interrelationship between the budget and national income, and other aspects of Soviet finance are either omitted or discussed very superficially in these articles. Soviet publications concerned with social problems usually are of little interest and value. This book is no exception.

T. Sosnovy

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## Economic Systems; Planning and Reform; Cooperation

*System jugoslowianski z bliska.* (The Yugoslav System at Close Range.) By Wlodzimierz Brus and Szymon Jakubowicz. Warsaw: Polskie Wydawnictwa Gospodarcze, 1957. Pp. 78.

In December 1956, a group of Polish economists spent several days in Yugoslavia, apparently in order to find out as much as possible about the Yugoslav economic model and its possible application to Polish conditions. Two of the participants have published their impressions in this small volume, the first part of which treats the system as a whole, with the second part concerned exclusively with the problem of workers' councils. The volume was written exclusively for Polish consumption soon after the political upheavel in October 1956.[1]

The author of the first part, Professor Brus, is at present one of the leading spirits in the campaign to liberalize the Polish economy. With minor reservations, he does not hide his admiration for the Yugoslav model, and he devotes by far the largest part of his section to description of the mechanism of the Yugoslav central economic plan. Unlike the detailed plans of the Soviet bloc countries, the Yugoslav plan contains only a set of targets to be eventually realized as a result of economic processes generated by a combination of measures chosen by the planners. The means employed include differential depreciation, interest, and turnover and income tax rates, allocation of profits in enterprises, wage scales and distribution of investment funds. The independent enterprises must then work out their own plans based on these data. Brus emphasizes that the system does not "subject the economy to spontaneous market processes." The plan provides the general framework within which the individual enterprises are free to choose alternatives. The choice is not ab-

[1] For a recent description of the Yugoslav economy see "Economic Planning and Management in Yugoslavia," *U.N. Econ. Bull. for Europe,* Nov. 1958, *10* (3).

solutely free since the system of economic incentives embodied in the plan creates sufficient pressure upon the enterprises to make their plans comply with the central planners' preferences.

It is this lack of rigid centralism and of direct controls that appeals mostly to Brus. He realizes that any socialist economic model must somehow combine central planning with the decentralization and freedom of choice. In his opinion, the Yugoslavs manage to connect both elements in the right proportion, leaving enough freedom to the enterprises to operate on their most efficient level and yet preserving enough control to eliminate any tendency towards monopoly. Enterprises can select their own suppliers, both at home and abroad, and plan their operations on the basis of market demand. Brus is particularly impressed with the foreign trade policy and with the fact that the level of domestic prices is not completely divorced from world prices, as it is in Poland. At the end of 1956, all prices but those for some basic foodstuffs and for important producer goods were apparently the outcome of free-market forces.

Brus provides a useful analysis of the relationship between output, profits and the level of wages. Originally, worker earnings were entirely dependent on profits. This system was soon abandoned because of the great differences in profitability among enterprises and consequently in earnings in the same occupation and industry, and because of the existing inflationary pressures. Since then, wages have been divided in two parts of which only the smaller part is determined by profits. Basic wages are quite rigid, wage differentials are very low and work norms are revised drastically every few months. According to Brus, this system weakens incentives to acquire skills, to increase productivity and to get rid of unproductive workers. One interesting point is the absence of a tendency to underestimate planned output in order to achieve a high rate of overfulfillment and high premiums. Since part of the wage fund is connected with profit, it is the final result that counts, and any overfulfillment of the plan has a relatively insignificant effect on wages. The reverse effect occurs in Poland.

The final part of Brus' volume deals with his appraisal of the Yugoslav model. He tries to answer here the question why, in spite of the measures listed above, relatively little progress has been made in certain respects, particularly in average consumption per head. He explains this in three ways: First, the rapid rate of industrialization, with over one-third of the national income diverted to investment, has had the same negative repercussions as in the Soviet-bloc countries. Second, the international situation until 1955 caused a break-off of commercial relations with the Soviet bloc and forced Yugoslavia to spend on defense up to about 18 per cent of its national income. Third, "the lack of a correct policy in agriculture" apparently compares very unfavorably with the rest of the economy.

The author specifically disagrees with the Yugoslav policy on only two points. He favors fixed prices for producer goods, arguing that violent movements in these prices are bound to affect the prices of final products, thus threatening to destroy the equilibrium in the relatively free market for con-

sumer goods. He also comments on the lack of differentiation between industries of nation-wide and local importance that results in often irrational distribution of investment funds and subsidies.

Brus ends by listing three elements of the Yugoslav system which he would like to see incorporated in the Polish model: the central plan influencing the economy not through direct controls but through economic incentives; real independence of enterprises; and the system of workers' councils. For many years the Poles believed that a planned economy could only operate on the basis of highly centralized controls of the Soviet type, which meant considering the Yugoslav model as a "betrayal of socialism." This opinion, the author concludes, was patently wrong.

The second part of this volume, written by Dr. Jakubowicz, goes over much of the ground covered by Brus, in addition to providing fairly detailed study of the Yugoslav workers' councils. His study does not contain any particularly useful analysis and can properly be classified as a pamphlet glorifying the councils, as was the fashion at the time of its writing.

The section written by Brus can serve as an interesting, although hardly extensive, description of the Yugoslav model, presented in true scholarly fashion. It should contribute to knowledge of Yugoslavia not only in the East but also in the West.

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## Business Fluctuations

*The Relationship of Prices to Economic Stability and Growth: Compendium of Papers Submitted by Panelists Appearing Before the Joint Economic Committee; Hearings Before the Joint Economic Committee, May 12-22, 1958.* Washington: Supt. Docs., 1958. Pp. xiv, 712; v, 427. $2.00; $1.25.

In 1957, the Joint Economic Committee of the Congress decided on "a full-scale investigation of prices and price-making processes in relation to economic stability and growth." This decision resulted in a compendium of papers by 47 leading economists published in May 1958. Later that month, most of the contributors appeared before the Committee for a series of panel discussions of the papers submitted. Despite efforts of the Committee staff to direct contributions over a broader range of topics, the discussion continually adverted to the behavior of the price indices during the 1957-58 recession. In consequence, the papers—and even more so the panel meetings—furnish an interesting cross-section of professional opinion as to the unorthodox interaction of demand and prices during this period.

Without too much simplification, it can be said that four viewpoints emerged. The first held the continuing increase of prices over the past two or three years to be attributable wholly or in the main to forces originating on the demand side of the market. This viewpoint considered the proper weapons for maintenance of price stability to be monetary and fiscal policy, perhaps sharpened by greater experience, perhaps (in the opinion of some mem-

bers of this group) supplemented by more specific credit controls. Most adherents to this view foresaw no serious clash between reasonably defined goals of full employment and stable prices.

The second group, although usually in considerable agreement with the first, felt that the supply response to increased demand was not simply an induced response but also a monopolistic one. This group wanted monetary-fiscal policy reinforced by stiffer enforcement of the Sherman Act, and sounded the call for "more competition" in order to maintain the compatibility of full employment and stable prices.

The third group, although agreeing with the second that supply-side forces demanded attention, questioned explanations framed in terms of simple monopolistic conspiracy. They questioned, that is, explanations drawn from conventional price theory, preferring to build their analysis around some version of the "administered price" concept. Having accorded more autonomy to supply forces, members of this group tended to be more sceptical of the ability of restrictive monetary-fiscal policy to maintain price-level stability without excessive social cost.

The fourth group was most openly hostile towards the Federal Reserve's restrictionist policies. The emphasis here was not on structural or institutional supply-side features, but on the dangers of a near-obsession with a stable price level, in disregard of considerations of equal or greater importance—productivity and growth.

The most forthright analysis of price-level behavior was that of Milton Friedman, who argued from a qualified version of the quantity theory: the dominant influence on the price level is exerted by the stock of money per unit of output. Although conceding minor roles to other factors, Friedman argued that there is a close link between money-stock and price changes in short as well as long periods; and he gave the money stock the dominant role in this relation. But Friedman insisted that there may be a considerable, and unappreciated, lag—over a year—before the full effect of any money-stock change is felt, thus explaining recent deflationary phenomena in terms of the delayed impact of 1956-57 tight money policies. The key to price behavior thus lies in monetary policy—but not discretionary policy. Although in general approving Federal Reserve policy since 1951, "I am myself inclined to believe that in our present state of knowledge and with our present institutions, even this policy has been decidedly inferior to the much simpler policy of keeping the money supply growing at a predesignated rate month in and month out with allowance only for seasonal influences. . . ."

If the money stock exerts its influence over prices by being spent, then presumably this is a Group 1 or demand-side interpretation (although this designation is the reviewer's, not Friedman's). Excess money demand was stressed also by Friedman's colleague, C. F. Christ. But Christ did not advocate nondiscretionary policies; on this point, O. H. Brownlee seemed almost Friedman's only supporter. Herbert Stein concurred with Friedman to the extent of attributing recent price-level rises to a lagged supply response to earlier demand.

Arthur Smithies foresaw serious danger of an unrestrainable price-wage

rise mainly in an overambitious attempt to guarantee full employment; occasional minor recessions, presumably inevitable even with the best of monetary-fiscal policies, would thus furnish the most effective check. The Group 2 contributors, such as Neil H. Jacoby, showed greater concern over demand-originating inflation. Jacoby's anti-inflationary program demanded (in addition to more effective monetary-fiscal policy) vigorous antitrust enforcement, gradual withdrawal of government price-fixing and price-support programs, and lower tariffs. G. W. Ensley, E. G. Nourse and G. L. Bach all stressed the maintenance of competition via antitrust enforcement. But S. N. Whitney, whose paper analyzed the consequences of a series of past antitrust convictions, found little evidence of any significant change in price behavior attributable to a conviction. R. A. Musgrave considered any thought of a return to competition, as an anti-inflationary device, utterly unrealistic.

The most provocative viewpoint offered was probably that of M. J. Bailey, who insisted that administered prices are practically nonexistent in the unregulated sector of the American economy, that the subject is not a proper concern of public policy, and that Congress ought not to waste time in its study. As to the unimportance of administered prices, Bailey had some support from his Chicago colleague, A. E. Rees. Unfortunately, Rees was not present when this support was most needed, at the panel discussion when Senator O'Mahoney, who appeared to regard the Bailey viewpoint with something less than unqualified approval, gave voice on the subject. A later panelist summed up Bailey's argument as "both moderately right and substantially wrong." Part of this argument, sometimes overlooked, was that an administered price is something different from a simple monopolistic price. In this, Bailey was identifying himself with Group 3 rather than Group 2, and he may well have been more than moderately right. But in then adding that monopoly prices are important, administered prices unimportant, he returned to Group 2.

The most comprehensive Group 3 argument was that of Gardner Ackley, who interpreted recent price behavior as a by-product of jockeying for income positions as between labor and capital. Ackley insisted that account must be taken of the administered-price phenomenon, and that "mark-up inflation" is an intellectually respectable form of analysis. Administered prices are to some degree insulated against market demand forces, and to this extent can live a life of their own. Althought he insisted that mark-up inflation is more than simple cost-push inflation, Ackley's argument seems consistent with points made by Ruth P. Mack and subsequently cited in several panel discussions. Mrs. Mack observed that most crude-materials prices still fluctuate freely, but that prices beyond the crude-materials range seem to work on a ratchet. An increase in raw materials prices sends succeeding prices up. But an increasing accumulation of overhead costs relative to direct costs in fabricating industries provokes a fear of price reductions; hence fabricated-good prices do not fall when raw materials prices fall. Strongest support for the Group 3 position (although with differences in emphasis) seemed to come from those who had been involved in statistical or other empirical price-involving work: Mrs. Mack, J. W. Kendrick, B. G. Hickman, R. F. Lanzillotti, and A. R. Oxenfeldt.

No deep conflict between Groups 3 and 4 was necessarily involved, save as to the points most in need of emphasis, and several contributors took intermediate positions. The Group 4 leader was Richard Ruggles, who argued that price indices and unemployment figures are seriously inadequate as all-around measures of performance. Ruggles felt (as did many participants) that the inability of the Consumer Price Index to take full account of quality improvements means that it must overstate the true degree of price increase. The increasing proportion of overhead labor (supervisory and technical) whose employment continues even when output drops means that unemployment figures fail to reveal the full extent of output loss. Despite technological change and expanded capacity, there have been significant increases in output per manhour in only two of the past five years. Denying the argument that a mature capitalist country must submit to a relatively low growth rate, Ruggles argued for increased investment in productive plant and equipment and an emphasis on growth. The much-praised automatic stabilizers choke off rises as well as falls—"we are in fact in the position of being strangled by automatic stabilizers." Continuation of present behavior would mean that "any growth that is achieved must be crowded into a small space of time; all the rest of the time will be recession, recovery from recession, or levelling off prior to going into recession." In answer to those who fear that increased investment demand would add fuel to already-burning inflationary fires, Ruggles pointed out (as did some others) that high productivity is an anti-inflationary counter to the increasing wage demands that may prevail in any circumstances; and high productivity comes when the economy operates close to capacity.

Virtually all contributors paid at least lip-service to the goal of price stability. Some viewed the prospect of secular price increases with alarm. Others, Ruggles included, found it tolerable—not desirable, simply tolerable in comparison with the alternatives to price rise and their consequences. Estimates of tolerable increase seemed to range from 1 to 3 per cent annually. Several Group 3 or 4 economists felt that some form of public administrative body might be needed to deal with wage-price problems. Ackley tentatively suggested a "watchdog commission." A. G. Gruchy proposed a "negotiational-consultative body" along Scandinavian lines. Abba Lerner advocated a body possessing more explicit powers: an agency to furnish administered regulation of administered prices.

An arbitrary squeezing of viewpoints into four pigeonholes may do injustice to individual participants; yet it serves to illustrate agreements and disagreements. As to the broad area of disagreement, two points seem noteworthy: (1) In the main, these are not normative disputes, conflicts as to the proper goal to be pursued. They are disputes as to the way things work. (2) They concern a topic which has been the traditional preoccupation of economists for over a hundred years: price. It would appear that there is analytic work still to be done in the analysis of price determination and price behavior.

ROMNEY ROBINSON

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*Can Inflation be Controlled?* By HAROLD G. MOULTON. Washington: Anderson Kramer Associates, 1958. Pp. xii, 302. $4.95.

The objective of this volume, in the words of its author, president-emeritus of The Brookings Institution, is ". . . a thorough-going reappraisal of the forces responsible for changes in the general level of commodity prices" (p. 8).

Dr. Moulton starts with a review of the state of confusion extant on the subject of prices and money. In spite of the inroads made by the income approach, most textbooks deal with the price level as a monetary phenomenon explained by some version of the quantity theory. Economists, senators, members of the Board of Governors and others serve up explanations of inflation which range through such causal elements as the abandonment of the gold standard, trade unions, government deficits, demand-pull, cost-push, and debt monetization.

Moulton finds that most of these explanations neglect the way in which money comes into existence. Goods and money are integrally related and one cannot increase without the other. The thesis of this volume is that price changes must be analyzed through the forces that increase or decrease the ratio between factor costs and output. Rising costs are the only factor which exerts *pressure* toward higher prices. Prices are set on the basis of costs established before goods come into the market.

The adherents of the demand-pull explanation of inflation, the author finds, are on very weak ground. An examination of the differential behavior of wholesale and retail prices demonstrates that the pressures come first on basic raw materials, and are transmitted by costs, not by demand.

The author finds support for these views in an examination of price behavior in such diverse experiences as the American Revolution, the Civil War, the French Revolution, the period following the first world war in Germany and both world wars in the United States. Further evidence, both analytical and statistical, is set forth in an appendix which makes up almost half the book, where the author examines such matters as the early quantity theory, Irving Fisher, and various attempts at statistical verification of the relationship between the price level and the quantity of money.

Moulton argues that long-term peacetime trends in the price level are shaped primarily by the cost-increasing effect of upward changes in money wages rates and the cost-reducing impact of improvements in productivity. Only if the latter exceed the former can there be price stability. Efforts to balance government budgets, to control interest rates, or influence the quantity of bank reserves will have little or no effect in the absence of a balance between wage rate increases and productivity. The only way to control inflation would be to control wage negotiations, but this would violate forces that ". . . are an inherent part of the private enterprise system" (p. 179). Both rights and human interests are involved, and, it may be inferred, in Moulton's view these are more important than the resulting inflation. The book ends with the hope that labor, management-stockholders, and consumers will all share equitably in the gains of technological progress.

In spite of an over-brief treatment of a great many significant points, this book is a contribution to monetary theory. At a minimum it is a useful offset to those who have recently become enamored with the rediscovery of money.

JESSE BURKHEAD

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## Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

*Gosudarstvennyi bank SSSR: Kratkii ocherk k sorokaletiiu Oktiabria* (The State Bank of the USSR: A Brief Outline on the Fortieth Anniversary of October.) By V. F. POPOV and others. Moscow: Gosfinizdat, 1957. Pp. 254. Rbl. 9.35.

Since the annual flow of Soviet publications on money and banking and related matters is large, interest attaches to this particular volume chiefly because of the circumstances under which it appears. This is, in effect, an official history of the State Bank (the principal short-term lender and principal source of money creation in the Soviet economy). Its authorship is credited to a group of the Bank's employees, writing under the direction of V. F. Popov, chairman of the Bank. As its subtitle indicates, it is published in commemoration of the October Revolution. It appears at a time when secrecy has been relaxed for sectors of the economy which would seem vastly more sensitive than that of banking. All of these circumstances suggest that the publication of this work could have provided the occasion for the release of comprehensive quantitative data on the Bank's operations.

Disappointingly, and with exceptions to be noted, the occasion has not been taken. Rather, like publications in the field for many years past, this work consists almost entirely of institutional and descriptive material: the organizational structure of the Bank, its lending procedures, modes of rendering payments, currency circulation (the chapter on this subject contains most of what is said on the Bank's planning and control functions, which are otherwise little emphasized), the Bank's operations as fiscal agent of the budget, its handling of international settlements, accounting and operating procedures, etc. Moreover, while the institutional information provided here is handily arranged either for a brief introduction to Soviet banking practices or for easy reference by the specialist, it is generally available in more extended discussions in other sources.

With respect to quantitative data, it should be first understood that detailed balance sheets for the Bank ceased to be published in 1932, complete but highly consolidated accounts were published from 1933 to 1937, scattered fragments were released until the start of the war, after which scarcely a single absolute figure was released until figures for total loans again appeared in the early 1950's. Hence the anticipations which this book arouses and largely—though not quite entirely—disappoints. There are some new figures here, principally on pages 58-59 and in a statistical appendix.

Most revealing are several postwar figures for deposits held at the Bank by

enterprises; these are so small as to suggest that the enterprise sector has remained as chronically illiquid as it was before the war. Figures on deposits of collective farms reflect the post-Stalin rise in farm incomes; a single figure for deposits of individuals indicates that these remain insignificantly small. On the critical issue of the size of the Budget's deposit holdings, which are known to be very large, the source reports merely that balances of "the financial-credit system" in 1957 were 11.2 times the prewar level (when loans were 4.4 times the 1941 level). No new data on currency emission are given here, although the Minister of Finance has felt free to report elsewhere, also in 1957, that currency at the end of the war was 4 times the prewar level and by mid-1949, after the 1947 conversion, was only 30-40 per cent above prewar. Also on the liability side, useful figures are given for what are essentially items in transit. On the asset side, loans are here shown to have increased by little more than 8 per cent from 1941 to 1946, which raises again a long-standing question of the source of the wartime currency expansion, beyond that which can be accounted for by budget deficits. Extremely detailed data on the distribution of the Bank's loans by purpose and by sector suggest likely explanations of recent fluctuations in the volume of loans, including a 22 per cent increase in 1956 alone, and indicate clearly that the much-heralded changes in lending procedures decreed in August 1954 have had little quantitative impact. Nothing is said of reserves, capital accounts, or total assets, and no hint is given on the handling in the Bank's accounts of the 1947 conversion or of the inventory gains and losses known to have been shifted to the Bank as price levels changed.

In general, the amount of new information given here is large relative to that previously released but small relative to that required for a reasonably complete understanding of the Bank's operations. It is enough to be tantalizing but too little to be satisfying. Perhaps one should, nevertheless, be thankful for small blessings.

One other deficiency of the volume merits comment: its almost total lack, except for some obscure implicit theorizing in the chapter on currency circulation, of anything which could be called analysis. A few years back, this would have been in no way noteworthy. But from some time after Stalin's death, Soviet writers on monetary matters began to raise the kinds of questions which present themselves immediately to the Western observer: What criteria justify the present division of responsibilities, for financing the economy, between the Bank and the Budget? What function is served by the budgetary surpluses accumulated as deposits at the Bank? (One reviewer of a textbook published in 1956 complimented its author for merely raising the latter question, though surpluses have been run since 1924 and large surpluses since 1933). What accounted for the inflation that persisted into the late 1940's? The answers offered have been generally faltering and naïve, but the reawakening of a spirit of inquiry which they indicate is intriguing and even heart-warming to watch. That no such spirit is evident in the book at hand may be no more than a reflection of its official character.

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## Public Finance

*Classics in the Theory of Public Finance.* International Economic Papers No. 8. Edited by R. A. MUSGRAVE and A. T. PEACOCK. New York and London: Macmillan, 1958. Pp. xix, 244. $6.00.

This volume is a collection of late 19th—early 20th century contributions to the classical problem of public resource use in a fully employed economy, which has recently been regaining a central position in public-finance theory and policy after a long period of displacement by the problem of stabilizing effective demand. All sixteen selections (except one from Edgeworth included because of special relevance) are original translations from works of continental European writers: ten from the German (including work of the Swedes Wicksell and Lindahl), four from the Italian, and one each from the French and Dutch. The selections are arranged chronologically beginning with extracts by Wagner published in 1883 and terminating with excerpts by Ritschl published in 1931. A 10-page introduction by the editors very helpfully identifies and traces the main trends of thought running through the various contributions and relates them to the concurrent development of general economic thought.

The most interesting and perhaps most significant aspect of the *Classics* is that many of the issues of methodology and policy which concerned the contributors are familiar as components of recent discussion. (Cf., for example, Gerhard Colm, *Review of Economics and Statistics,* Nov. 1956; Julius Margolis and R. A. Samuelson, same journal, Nov. 1955; J. K. Galbraith, *The Affluent Society,* Ch. 22 and passim.) Some of these issues and the treatment they received one-quarter to three-quarters of a century ago are, in brief, as follows:

1. What is the comparative productivity of normative and positive theories, and how construct the latter? Most of the authors in the main do take a normative approach. Exceptions are Wagner in his explanation of the growth of public expenditures, Stein in his historical treatment of taxation, and Goldscheid who vigorously claims public finance for sociology and criticizes the cart-before-the-horse approach of setting policy norms prior to understanding the function of public finance in state and society.

2. Is the indivisibility of public goods—that is, providing for one provides equally for all—a centrally important feature? In general, yes.

3. Are individual preferences the main criterion for allocating resources to the public sector? A resounding "No!" from Ritschl, for whom the state serves communal needs subjectively felt by competent authorities and by individuals who spiritually identify with the community; others must be coerced. Montemartini considers the product of government to be not service at all but coercion. Barone clearly rejects individual valuation as a basis for taxation. Edgeworth and Cohen Stuart, exemplifying the ability-to-pay approach, split off tax policy from expenditures and attend exclusively to the former, elucidating via marginal utility analysis the implications of minimum, equal and equal-

proportional sacrifice for tax-rate progression. Others, by implication affirmative, are discussed in (4).

4. Can the quantity of public goods and their cost distribution be such as to permit an optimum want satisfaction comparable to that for private goods and is there a workable means of attainment? Mazzola, having early introduced the indivisibility feature, argues that quantity consumed not being individually variable, correspondence between marginal utility and price requires differential prices (taxation), which he concluded (wrongly) would eliminate consumer surplus. Whether the equilibrium force could overcome disturbances from the political sphere is unclear. Sax, restating a forty-year-old position, accepts that taxes should vary with the money-worth of public goods to the individual (whose initial valuation, however, necessitates a spiritual bond with the community).

In a long and wise essay, Wicksell, responding to Mazzola and Sax renders an all-right-in-theory-but-not-in-practice verdict. The individual, whose benefit depends on the payments of others and insignificantly on his own, would bid nothing for public goods. Therefore he suggests that all proposals be submitted in an expenditure-*cum*-tax form and be adopted only by unanimous or approximately unanimous vote.

Lindahl in a rigorous analysis views the evaluation of public goods as analogous to private pricing of joint products, the supply schedule of government services to one taxpayer-demander depending on their total cost and the proportion covered by others. At equilibrium, the level of expenditure and its cost distribution is determined so that for each taxpayer the marginal gain equals the marginal cost. The analysis, initially developed in the context of a community of two taxpayers possessing equal political power but unequal income, is considered extensible to large numbers and in principle valid for cases of unequal power (but cf. Musgrave, *Quarterly Journal of Economics* Feb. 1939, and Samuelson, *op. cit.*). Individual preferences become effective through the vote and the fact that fiscal decision-makers always take the viewpoint of some average citizen.

5. Should taxation be used for wealth redistribution independently of cost distribution? Edgeworth, yes, but not to the income leveling extreme implied by the minimum sacrifice doctrine; Wagner, yes, as part of the social welfare concept; Wicksell, a cautious yes; Lindahl, yes for central but not for local government. Wieser, never.

Aside from speculation on the form and substance of intellectual progress, the primary reaction to the book is one of gratitude to the editors and the Association for making these works available for what is probably the first time to most economists in the field—including the reviewer. The teacher can certainly derive much utility from its use as a source of supplementary reading, particularly if he sees pedagogical merit in a development-of-thought approach to the subject.

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*Public Finance*. Edited by R. W. LINDHOLM. New York: Pitman Publishing Corp., 1959. Pp. xviii, 798. $7.25.

This book was written by the Committee on Public Finance which consists of 66 professors of public finance from all over the nation and from all types of educational institutions. An attempt was made to write a definitive textbook in public finance, or certainly one that might be preferred by a majority of teachers in that field. While the committee method is not usually considered the ideal method by which to write the ideal textbook, the Committee on Public Finance has been very successful in integrating the material and presenting it with clarity, precision, and a common style. Undoubtedly the Committee benefited from the experience and criticisms of the earlier Pitman book, *Money and Banking* (1957), written by a 59-man Committee on Money and Banking, and reviewed in the December 1958 issue of the *American Economic Review*.

The emphasis of this book is on balance and comprehensive coverage rather than on controversy. Theoretical analysis is combined in proper proportion with the practical realities of the subject matter. Yet none of the recent theoretical developments in the field of public finance and fiscal policy is omitted or overlooked.

As for the earlier book, separate committees were established to plan and to write each chapter, but each chapter benefited from the mutual criticism and cooperation of other members and committees of the project as well as from the editorial staff. While the quality of the individual chapters varies somewhat, the over-all result is a rather constructive and challenging piece of work.

The book is divided into the following parts: Introduction, Expenditures, Taxes and Revenues, Public Debt, Fiscal Administration, and Fiscal Policy. The Introduction discusses the financing of the public as contrasted with the private sector of the economy, and the role of the government in a market-oriented economy. Part II devotes 10 chapters to all of the major federal expenditure programs as well as to the general patterns of public expenditures. Part III in 9 chapters covers all the important federal, state, and local tax sources as well as the criteria for allocating taxes by level of government and the subject of shifting and incidence of taxation. Part IV covers the relationship of the public debt to the money supply and also the economic effects of public borrowing. Part V devotes 4 chapters to the formulation, administration, and execution of the federal budget, the planning and control of public expenditures, state and local administration, and intergovernmental fiscal relations and problems. The last part in 5 chapters discusses fiscal, expenditure, tax, and debt policy, and also the basic patterns and historical evolution and application of fiscal policy.

All in all, the book is thorough, complete, and readable. The data are current generally through 1956 and 1957. The discussion questions at the end of each chapter as a whole are good.

The length of the book is not particularly objectionable because parts can

if necessary be omitted in the conventional introductory course in public finance. Then, too, sufficient material is presented for certain follow-up courses in that area. However, a good bibliography at the end of the book, or better still, at the end of each chapter or part of the book, would have been of distinct advantage to the teacher and especially to the student.

The book has a number of novel features, only a few of which can be mentioned here. For example, Chapter 2 has an excellent discussion of the determination of governmental powers and responsibilities, and the distribution of these by level of government; Chapter 21, of the desirable size for the money stock or supply as it relates to fiscal policy and debt-management problems and objectives; Chapter 26, of the direct, indirect, and implied federal limitations on the power of state governments to tax. Chapter 29 has a unique discussion on the formulation of tax policy for each level of government.

Needless to state, with 66 co-authors, the book will have many adoptions. Apart from that fact however, the book warrants the attention of all serious students of public finance and fiscal policy.

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## International Economics

*Globe and Hemisphere: Latin America's Place in the Postwar Foreign Relations of the United States.* By J. FRED RIPPY. Chicago: Henry Regnery Co. in coop. with Foundation for Foreign Affairs, 1958. Pp. xi, 276. $6.00.

This is a topical book on what to do about Latin America by a veteran student of the area. More exactly, it is a series of brief historical essays that, as happens almost always, shows the historian to be more interested in prescribing for the future than recording the past. The "lessons of history," of course, are what the historians would teach us, history or no history, but it's a pity they do not meet issues head-on, working openly in the light of reason, weighing arguments pro and con. The direct approach might at least provoke a good debate that unfortunately is not encouraged by any number of asides strewn among the interstices of a loosely constructed series of historical narratives.

It is not to be denied, however, that there is some interest in reading the various episodes in recent inter-American relations that make up the bulk of this book. Its 14 essays include three on past U.S. investments in Latin America; two of general scope on our economic aid to these countries; two on specific programs of assistance (the inter-American Highway and the canal zone projects in Panama and Nicaragua); one on our massive aid program in Bolivia; separate chapters on the problems of rubber, sugar, and fisheries; an introductory essay on the Western Hemisphere concept; one on cultural relations; and a final one entitled "myopic drifting" that returns to the theme

of foreign aid along with an obiter dictum on how such aid allegedly fosters statism in the receiving countries. There's something for almost all tastes, written in an easy style, and rich in documentation and tabular material. Very often a healthy muckraking spirit enlivens proceedings, especially in the episodes of Nicaragua and the "rubber-planting fiascos in tropical America." In fact, the author shines in narrating little-known fiascos, but his foreign investment chapters, which would ordinarily be of most interest to economists, do little more than repeat an oft' told tale. He would also be more convincing were he less *ad hominem* and more *ad rem,* treating proposals on their merits, not on the supposed motives of their sponsors.

The author's main point is that we should keep a place for hemispheric commitments within our global ones. At the outset he announces his opposition to expending "more energy and larger resources in shaping the destiny of the peoples of the Old World than in improving the fortunes of the New" (p. 1). To the obvious objection that our national security lies in alliance with the Old World, he answers that "Maybe the defense of Europe can soon be left largely to the most efficient and dependable European nations with their intermediate-range missiles. Then the United States could gradually resume its traditional policy of emphasis on the Western Hemisphere . . ." (p. 239). But maybe not? Surely our foreign policy should not rest on the success of one kind of missile. More importantly, he believes that the Western Hemisphere concept preserves a military basis: "But the Western Hemisphere still serves as the 'inner fortress.' Though more exposed and less impregnable than it used to be, it is still the inner fortress" (p. 28). Also: "And let it never be forgotten that this Western Hemisphere, where our ancestors had a chance to make a new start, is our inner fortress and the only base of operation we have; that all would be lost if it were weakened beyond repair" (p. 200). Of course, all this is very doubtful, but it's a matter more for generals than economists.

Rippy would support foreign aid only if it were tied exclusively to our national security. Hence on aid programs: "The programs should be restricted to countries of supreme importance to national security, which should be their prime, if not their sole, motivation" (p. 230). Again: "Only such expenditures as are strictly involved in safeguarding the security of the United States can truly be described as outlays serving the national interest" (p. 238). But this is not to be taken as a plea to multiply aid to the countries of our so-called inner fortress: "The conclusion that [what] the hemispheric neighbors received in fiscal years 1956-57 was about as much as they deserved in the circumstances appears to be justified" (p. 233). The correct policy would therefore seem to be a drastic cut in foreign aid whose allegedly heavy burden on the taxpayer is indeed deplored several times. Such a line of reasoning would also suggest that Rippy is one of the last of the isolationists; in a short review one cannot say more than more power to him in challenging accepted notions, but his isolated salvos in fact carry little firepower.

The author is aware that Pan-Americanism means more than military security based on propinquity. He writes that "Yet the Western-Hemisphere

concept in its full flower seems to have embodied ample common convictions, ideals, and objectives to bind the Americas together in a unique practical relationship. Perhaps the concept was always partly fictitious; but faith and trust have a way of transforming fiction into fact" (p. 199). So wishing does make it so, after all. Or does it? Who still believes, looking south as well as north, that the New World is the home of free government and the Old World the mother of despotism? If common political systems were a necessary basis for alliances, which is belied by our special relation to Spain and Yugoslavia, then our most intimate ties would be with Western Europe, not Latin America. Too much is often made of a community of ideals in the Western Hemisphere when reality falls short of ideals, by yards in the one case, miles in the other. Meanwhile there is a movement under way (in the talk about a "common market," for example) to bring together more closely the twenty Latin American countries. This movement has some roots in the past, and it will be interesting to see how it prospers in the face of growing American ties across the Atlantic.

THEODORE A. SUMBERG

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*Financing Free World Trade with the Sino-Soviet Bloc.* By RAYMOND F. MIKESELL and JACK N. BEHRMAN. Princeton: Princeton University Press, 1958. Pp. viii, 109. 25¢. No charge outside U.S.A.

This small volume is the latest in the series of paper-bound Princeton Studies in International Finance. While as the title indicates it is concerned with the methods by which the "free world" and the Sino-Soviet bloc have financed their mutual trade, much of the book deals with the theory and practice of bilateral agreements, since this type of agreement has predominated in East-West trade. In 1955, for example, 71 per cent of free world-bloc trade occurred between countries with bilateral agreements; in 1957, there were roughly 240 bilateral trade or payments agreements between these two groups of nations.

What has motivated such a large amount of bilateralism? In the case of Western European countries, the bilateral agreements are largely a residue of severe currency problems of the earlier postwar period and are declining in number. The less-developed "free world" nations have a variety of motives. Bilateral agreements with the bloc provide them with opportunities to: dispose of temporary surpluses of primary commodities; sell in more stable (both as to price and quantity) primary-product markets; obtain credits for import surpluses as well as technical and financial assistance; sell exports despite overvalued exchange rates. On the other side of the fence, bloc motivation is both political and economic. Commercial advantage has clearly been dominant in the expansion of bloc trade with Western Europe—but this expansion has become increasingly "multilateral." In the case of bloc trade with the less-developed nations, there are also strong economic factors: the need for raw materials; and the opportunity in many cases to obtain these materials in exchange for manufactured goods which might not be acceptable in more de-

veloped countries at world prices. Bilateral agreements also provide the bloc nations with greater bargaining power in dealing with these "weaker" trading partners and also tend to orient the latters' trade toward the bloc. The usefulness of bilateral trade and payment agreements for exerting political pressures is, of course, obvious. While the Sino-Soviet countries have not always, nor perhaps often, conceived their bilateral trade agreements with the political factor in mind, they have used them to some political advantage in a number of instances documented by the authors. Special cases of this are the series of long-term credits to less-developed nations negotiated in the bilateral framework since 1955.

Much of the raw material upon which this book is based was gleaned by the authors from the unclassified files of the Department of Commerce and from information provided by the State Department regarding bloc trade agreements with other nations. The bulk of the book is devoted to analyzing and classifying these data in order to reconstruct the salient characteristics of bloc-free world trade. Among other things, the authors cover payments agreements, commercial and banking practices, agencies conducting trade, pricing practices, trade and commercial practices, and so forth. Their analysis is competent and interesting. Of especial interest was the presentation of case studies to illustrate generalizations.

One generalization which particularly struck me, having just recently read some Russian materials on the advantages of Soviet-type bilateral trade, was that bloc bilateral trade has been both (1) subject to wider fluctuations than unplanned trade among nonbloc nations and (2) wide of targets regarding the value of trade. Soviet literature leaves the opposite impression on both counts.

This is a useful, interesting and (in my opinion) unprejudiced empirical study in a field in which economists and politicians are too prone to prejudiced deductive generalizations. It should be read by everyone concerned with East-West trade. It contains, incidentally, in addition to the officially listed 109 pages of text, a 145-page unpaginated appendix summarizing the agreements of Soviet-bloc countries with the West.

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*Soviet Economic Aid: The New Aid and Trade Policy in Underdeveloped Countries*. By JOSEPH S. BERLINER. New York: F. A. Praeger, Inc. for Council on Foreign Relations, 1958. Pp. xv, 232. $4.25.

A descriptive analysis of what goes on behind an iron curtain of secrecy and propaganda is no easy task. And a study which by its nature requires a good deal of conjecture, estimation, and guesswork is not without frustration. Yet the challenge of such an endeavor must have been attractive enough to invite Professor Berliner to write the story of Soviet aid to underdeveloped countries.

The book deals with Soviet "aid and trade" programs in the Middle East,

South and Southeast Asia, Africa, and South America in the four-year period between 1953 and 1957. The first half of the study analyzes the rationale of the Soviet program and the nature, type, and relative magnitude of the Soviet and the Communist bloc commitments. The remainder of the book is concerned with an evaluation of the program's benefits to the Soviet economy, the advantageous position of the Soviet Union in this new game of diplomacy, and its capability to continue its aid program. Several charts and tables provide supporting data on bloc credit and trade agreements with selected underdeveloped countries. The bibliography presents a fairly comprehensive list of the major works on the the subject. Notable exception: Klaus Knorr's brief but pioneering study, *The Ruble Diplomacy.*

According to Berliner the Soviet aid and trade program began in 1953 as a result of a major tactical shift in Soviet foreign policy. Until Stalin's death, Soviet policy supported local Communist parties in their opposition to the emerging "bourgeois" governments. Western economic aid was denounced as a capitalist-imperialist device to exploit the underdeveloped areas. The post-Stalin leaders of the USSR, however, discontinued this policy and decided to support "bourgeois nationalists" in their quest for rapid economic development, political independence and military neutrality. Underlying this change in tactics was an effort to influence foreign policies of the newly independent countries of Asia and Africa in favor of the Soviet Union.

Communist bloc aid to undeveloped countries takes a variety of forms: (1) the provision of medium- or long-term (5-20 year), low interest (2-2½ per cent loans and credits to finance capital-goods imports; (2) technical assistance in building and installation of industrial plants; and (3) training of local technicians and skilled workers for the operation of industrial projects. Since, however, the latter types of assistance are normally paid for by the recipients, Communist aid is in effect favorable business loans that ought to be repaid at a later date. In this respect it differs materially from foreign aid offered to the underdeveloped world by the United States.

The value of Soviet bloc credit agreements with 16 underdeveloped countries between 1953 and 1957 is estimated at $1,581 million to be drawn upon over a "number of years." Of this total only $342 million is estimated to have been actually delivered between 1953 and 1957. The total Soviet "deliveries," therefore, hardly measures up to the $2,597 million of U.S. grants-in-aid and credits actually given to the same countries between 1945 and 1957. Compared with the total U.S. aid and loans to the entire underdeveloped world of some $12.8 billion in the same period, Soviet aid ranks even lower. Three-quarters of the $1,581 million credit is extended by the Soviet Union alone, and 90 per cent of it is concentrated in five countries: Yugoslavia, the United Arab Republic, India, Afghanistan, and Indonesia. The magnitude of the bilateral technical assistance agreements between the bloc and aided countries is estimated to be about $6.5 million or roughly the same amount contributed by them to the United Nations' expanded technical assistance programs. This is to be compared with the U.S. government pledge of $72 million to the

same United Nations agency. The total value of gifts offered by the bloc is believed to be under $20 million.

The Communist bloc members consider commercial trade as an integral part of their policy of economic aid. Thus, the extension of long-term credits is generally accompanied by barter trade agreements. As a result, bloc trade with its underdeveloped partners is believed to have risen by more than two and a half times between 1953 and 1955. Despite this remarkable increase, the volume of commercial trade in 1955 was still below its 1948 level in real terms, and lagging far behind the trade of the same underdeveloped countries with the free world. Interestingly enough, although the USSR has provided the bulk of trade credits, the lion's share of the expanded commerce has thus far accrued to the East European satellites and not to the Soviet Union itself. The trade of Soviet Union with all non-Communist underdeveloped countries in 1955-56 accounted for only 3-6% of total Soviet foreign trade—a phenomenon which is indicative of Russia's continued fear of dependence on nonbloc nations.

Economically, Russia gains from trading certain kinds of manufactured commodities (in which it has comparative cost advantage) for certain types of primary materials which it needs. These advantages are now greater than they were in the early part of the century for two basic reasons: (1) a change in relative cost ratios which has occurred as a result of Soviet industrialization in the last 30 years, and (2) the possibility of providing many outlying Soviet regions with imports from neighboring countries at lower transportation costs. These economic benefits, however, are only a small part of Soviet gains. The real Soviet gains from its aid and trade program should be found in its "spreading good will" throughout the underdeveloped world and its increasing direct or indirect political influence in foreign policies of the underdeveloped countries.

Why has the Soviet Union succeeded in gaining such good will and influence with so little material sacrifice? Berliner suggests a variety of political, strategic, and emotional factors. Economics, again, plays a minor role. The advantage of "novelty" is all-important. The West has no longer a monopoly in foreign assistance. Underdeveloped countries can now play Russia against the United States. Anticolonial and anticapitalist biases in the socio-religious traditions of Asian and African people, too, favor closer affinity with Russia than with the West. Attractive terms, absence of apparent political strings, barter deals, acceptance of local currencies and commodities, and the administrative flexibility of the Soviet aid machinery are other points in Russia's favor which the United States government should seriously consider in its aid program.

The author considers Soviet capacity to extend economic aid to be fairly large, although he does not expect Soviet aid to rise substantially in the years to come. Soviet exports to all underdeveloped countries in 1956 amounted to only 1/20 of 1 per cent of its gross national product. Even if the Soviet government were to give underdeveloped countries as much economic aid as does

the United States, it would still spend not much more than ½ of 1 per cent of its 1956 national product. Assuming an annual increase of 5 per cent in Russian productivity, an aid program as large as that of the United States today would absorb slightly more than 10% of the additional annual production. This point, however, should not be overemphasized. Should the aid program be concentrated, as it very well may, on the export of machinery and particularly metallurgical equipment, an export program of even modest magnitude will make heavy inroads into Soviet production at least in the short run.

*Soviet Economic Aid* is written in a lucid and readable fashion. Combining objectivity of treatment and technical sophistication with informality of style, the study is of interest not only to professional students of Soviet-American economic rivalry, but also to all intelligent readers who are concerned with this new aspect of the cold war. The real contribution of this study to the field of economic diplomacy, however, rests to a large extent on the manner in which the author delves into the various facets of his inquiry, narrows the range of possible alternatives, chooses likely hypotheses, tests them against available facts and statistics, and reaches some important logical conclusions.

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## Business Finance; Investment and Security Markets; Insurance

*Principles of Finance.* By CARL A. DAUTEN and MERLE T. WELSHANS. Cincinnati: South-Western Publishing Co., 1958. Pp. xii, 596. $6.50.

The authors intend this text for a one-quarter or one-semester survey course in finance, covering the finance functions and the principles, institutions, instruments and procedures that have been developed to carry them out. Presumably it is aimed at those in schools of business administration who have found the traditional course in money and banking unsuitable as a basic requirement. The book is divided into six parts. The first four are devoted to financing in the private sector of the economy, covering short- and long-term sources of funds for business, special areas (agriculture and international finance), and short- and long-term sources of funds for the consumer. The latter two parts are concerned with the monetary system, including the impact of Federal Reserve and Treasury actions and with interrelations between the money supply, prices and production. The text is essentially descriptive in nature.

There is no question that in coverage this work is very complete and will provide the student with a comprehensive picture of the field of finance. The presentation is clear and straightforward; and some chapters, particularly those on merchandising and facilitating agencies for long-term financing, consumer financing, and Treasury powers affecting the money supply, are very good indeed.

However, in a survey book of this type, emphasizing breadth rather than depth of coverage, two very important considerations are (1) organization and (2) selection and emphasis of materials. It is in these areas that the reviewer must express some reservations. On the question of organization, setting up the business enterprise, the consumer, the farmer and the foreign trader as the focus of dicussion may have certain advantages. It certainly facilitates the presentation of descriptive material, but it practically eliminates any well-rounded analysis of portfolio management in our major financial institutions. As one illustration, in the case of the commercial bank, the instructor will nowhere find mention of the role played by secondary reserves in maintaining bank liquidity. The latter is discussed in less than a sentence (p. 485).

Just as serious are imbalances in selection and emphasis. For example, commercial finance companies and factors receive an inordinate amount of space in relation to their minor roles as sources of business credit; much more space is given to types of insurance policies than to the application of insurance company funds (Ch. 11); there is a minimum allocation of space to open-market operations, given their importance as an instrument of credit control (Ch. 23); the stress in "Financing Agriculture" (Ch. 14) is apt to give the student a false impression of the importance of governmental sources of credit in this field. Further, though there is considerable discussion of the income and financial status of households using consumer credit, there is no analysis of differences in the terms and availability of credit to large, medium and small business enterprises.

In sum, a form of organization allowing greater range for analysis, and a more judicious emphasis are required to give the student a proper perspective of the American financial complex. There is still room for a really adequate survey text in the field of financial institutions.

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*Basic Business Finance—Text and Cases*. By PEARSON HUNT, CHARLES M. WILLIAMS, and GORDON DONALDSON. Homewood, Ill.: Richard D. Irwin, Inc., 1958. Pp. xi, 911. $7.00.

Modern medical schools no longer train students of medicine without anatomy laboratories, clinical facilities, and hospital internships. Likewise, most law schools of high rank utilize legal aid practice, editorial assignments in law journals, and court decisions to educate law students. In strange contrast, schools of business administration generally are not clinically oriented. After more than thirty-five years of development, the case method as a means for educating businessmen remains highly controversial. Only a single business school in the United States has the faculty and other resources to employ case material comprehensively. Few other business school faculties have been able even to agree on the effectiveness of the case method as an educational procedure.

That progress is being made is shown by the increasing number of case-

books in business administration being published. Early casebooks in finance were published by Masson and Stratton in 1935 and 1938. Not much followed until Hunt and Williams published *Case Problems in Finance* in 1949. This book has passed through two editions and five printings. Still in use, it serves different levels of collegiate sophistication from the first course in finance to graduate and executive development courses. Despite this flexibility, a number of professors have wanted cases of less difficulty. Then, too, a need has continued to exist for textual material more closely coordinated with the topical outline of the cases. Hunt, Williams, and Donaldson have responded with *Basic Business Finance—Text and Cases*.

Of the 893 pages of this big book, 581 comprising 27 chapters are devoted to text material and 312 to case material. There are 40 cases, of which the shortest is only two pages; yet it poses for students probably the most difficult challenge of all the cases, a capital planning or investment problem. The longest case, 13 pages, requires a dividend payment decision based on analysis of extensive financial data. Ten cases are reprinted from the preceding *Case Problems in Finance*. However, three are revisions and thus constitute different cases.

After an introductory chapter, the book embraces: (1) the nature of the need for funds (4 chapters, 4 cases), (2) techniques for analyzing past and current financing (1 chapter, 4 cases), (3) forecasting future needs (1 chapter, 4 cases), (4) how funds can best be obtained: short-, intermediate-, and long-term (8 chapters, 14 cases), (5) capital budgeting (2 chapters, 2 cases), (6) markets for corporate securities (4 chapters, 3 cases), (7) government regulation (1 chapter, 1 case), and (8) recurring and nonrecurring financial problems, e.g., dividend policy, financial difficulties, promotion (5 chapters, 8 cases). The point of view is that of the chief financial officer of an operating company. Primary emphasis is upon financial decision-making and administration in a going concern. Secondary consideration is given to the interests of the security analyst, private investor, and trade creditor.

By now enough water has gone over the ideological dam to permit dispensing with a discussion of the relative merits of the internal operating point of view vis-à-vis the social point of view dominant in the traditional book on corporation finance. Suffice it to say that social aspects are not completely ignored. The issues are in the cases even though subservient to operational procedures. They can be brought out to an extent depending upon the interest and understanding of individual professors.

Substantively, the text material advocates the so-called funds approach. Concepts, generalizations, and procedures are related to discovering sources and to planning the uses of funds. The substantive material also supports the analysis of cases. Useful background information includes aggregative data and descriptions of financial institutions. At a more sophisticated level the discussion presents basic ideas on important financial issues. Analytically, the cases are more than illustrations for assertions made in the text. Some require as a first analytical step identification of problem areas, itself a sticky task.

Others state the problem, but require a solution. Still others pose problems but demand the clarification of alternatives and a choice leading to a decision.

Primarily the audience for this book will be students coming to the first course in finance, but they should have a basic familiarity with accounting concepts and statements. *Basic Business Finance* has sufficient substance to make it adaptable for more advanced courses. However, a coming revision of *Case Problems in Finance* with cases to replace those pre-empted by this new book is expected to serve the needs of students at levels beyond a first course.

Instructors who are largely subject-matter oriented will discover that the text compares favorably from a topical standpoint with such yardsticks as Dewing and Guthman and Dougall. This is not to say that the discussion of individual topics is as exhaustive. But it is not the purpose of business finance to imitate corporation finance.

The text is outstanding for its treatment of short-term financing—discovering needs, locating sources, bargaining for workable terms and negotiating contracts. It is equally competent in discussing problems of risk, income, and control in relation to corporate securities. In addition the text remedies an early shortcoming of business finance in emphasizing short-term at the expense of longer-term problems. Firmly encompassed in this version of business finance are such traditional topics as the legal pattern of business organizations, basic security types, security markets, government regulation, mergers, dividend policy, refunding, recapitalization, and business failure. The "something new" is represented by chapters on appraising alternative investments, and cost of capital.

This book is not entirely free of difficulties. Discussion of dividend policy would become more meaningful if it were recognized that this policy is a by-product of asset management and its twin objectives of liquidity and investment. Limiting the chapter on mergers to financial implications closes out the whole history and importance of antimonopoly policy as an area in which the business citizen must live. The appraising of material dealing with investment opportunities and the cost of capital requires, especially for beginning students, a more carefully presented sequence of ideas and clarification of mathematical procedures. The attempt to wed earnings coverage to outlay costs of capital confuses most students and may not convince some professors.

Nonetheless, this comprehensive combination of text and cases makes possible the building of a highly respectable beginning course in finance. There is enough material to support a full year's work including, probably, cases for examination purposes. The cases are not so unreasonably difficult as to defeat students at the beginning. Nor are they simple little examples to demonstrate ideas developed in the text. Average students will make progress with the key issues. Mature students will go beyond to the secondary and more subtle problems in the cases. The strength of this book lies in its essential rightness for training students of business administration.

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## Business Organization; Managerial Economics; Marketing; Accounting

*Organized Business in France.* By HENRY W. EHRMANN. Princeton: Princeton University Press, 1957. Pp. xx, 514. $7.50.

Professor Ehrmann, known to most of us as the author of a distinguished book on the French labor movement, has now published a companion volume on *Organized Business in France.* The first of its kind, it will be looked to with great expectations both by the economist who wants to understand France's puzzling economic development, and by the political scientist interested in the nature and structure of political pressure groups on the other side of the Atlantic. Neither of them will be disappointed; although each may find in this book more information on his subject than he needs and wanted to have. But this shortcoming may well be inevitable in a book catering to such divergent interests.

The first half of the book is taken up with a 100-page account of the history of business organization from Popular Front days to the Liberation and with a 170-page discussion of its present-day structure. This latter part deals, among other things, with the relations between the different types of business organizations, between the different levels within the same hierarchy of business organizations, and between the bureaucracy of business organizations and their member firms. It also deals with the policies and methods of action of business organizations as political pressure groups; and it analyzes the social origins, educational background, political history, personality, philosophy, and position of all the important leaders of organized business. The detailed discussion of important and not-so-important events and their background, of important personalities and the gray eminences behind the scenes must make fascinating reading to a political scientist; they fill one with awe and admiration for the author's wide knowledge and thoroughness; but the economist is likely on occasion to lose the thread and his patience and to bog down in the welter of names and detail. One is tempted to blame the author for giving quite so much detail and for not organizing and summarizing it better; although it is difficult to offer constructive suggestions for better organization, and much of the detail may well be necessary not only for the political scientist but for the economist as well. For the French economy is so complex and so fraught with contradictions that its understanding requires much more knowledge of this sort than does that of, say, the British economy. The student of the French economic scene must accept the difficulty of his task even if he groans under the burden.

More easily digestible and more interesting for the economist is the second half of the book, whose over 200 pages deal with the attitude of organized business to various economic issues, such as taxation, productivity, nationalization, cartels, European integration, labor unions, worker participation in business management, to mention just the more important ones. At the same time, the reader also picks up much incidental information about the attitude

to these issues of governments, politicians, political parties, and businessmen, big and small; and one is again impressed by the author's detailed knowledge and ability to handle a vast quantity of material. For example, the 20 pages devoted to the issue of economic integration give a well-documented account of the views expressed by the many officials, committees and unofficial spokesmen of different business organizations at various levels and in various industries in connection with the parliamentary and public discussions of some of the early plans for economic integration, of the European Defense Community, of the abortive Franco-Italian Customs Union, of the Coal and Steel Community, and of the European Community in general. Readers should have no difficulty in drawing their own conclusions from all this material but some may object to having to do it themselves. Many economists, I suspect, will turn to this volume in the hope of finding authoritative statements on where French business stands on a variety of issues. What they will find instead is the many tidbits of factual information that form the raw material out of which authoritative opinions are made.

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## Industrial Organization; Government and Business; Industry Studies

*The Structure of British Industry: A Symposium.* Edited by DUNCAN BURN. New York: Cambridge University Press, 1958. Vol. I, pp. xvii, 403; $8.50. Vol. II, pp. xiii, 499; $9.50.

When the definitive history of the doings of economists during the twentieth century is written it will remark on the rise in Great Britain during the period 1935-60 of a considerable number of economists who concerned themselves with, and were extremely knowledgeable about, the structures and workings of British industries in their setting in the total British economy. It will be noted that both the surge in interest and the transformation in approach were dramatic. One of the impressive items in the documentation will be the two volumes here under review. A quarter-century ago British industries were beckoning to economists, but to British economists industries were out of bounds. In Manchester there were stirrings of systematic interest, but elsewhere, except sporadically, British economists were not greatly aroused. Today, this is changed and changing. No British industry needs to feel neglected; indeed, none is safe from analysis by British economic analysts who know how to analyze. Few of them as yet are confident of their prescriptions, but the analysis is vigorous and its quality is high. In this field, at this stage, acknowledged lack of confidence is one index of competence.

The present symposium of industry studies—nineteen industries under the micro- (and macro-) scopes of twenty-one economists—is one of the best. Sponsored by the National Institute of Economic and Social Research, and under the skilful editorship of Duncan Burn, the two solid volumes succeed notably in giving "an up-to-date picture of the structure of a group of

[representative] British industries—the number, size, scope, and interrelations of the firms or units within each industry, . . . and their relations with industries overseas, and with the government"; and in analyzing "the effect of the structure of each industry on its economic performance, its adaptability to markets and to technical change, and its contribution to such change." The industries covered are agriculture, building construction, inland road and rail transport, oil, chemicals, steel, building materials, machine tool, motor, aircraft, shipbuilding, electronics, cotton and rayon textile, woolen and worsted, man-made fibres, pottery, pharmaceuticals, and cutlery.

The studies are packed with pertinent information, but they are not statistical digests. Each is an independent unit, and the individuality of each writer is quite apparent in his chapter; yet each study makes its own complementary, even though quite distinctive, contribution to the whole work. Each chapter carries authority, yet each gains in authority from the others. The entire enterprise is characterized by functional, if not by formal, unity and by firm direction; and the reader is bound to suspect strong influence, effectively exercised, by the editor. The suspicion is further supported by the presence of a concluding chapter written by the editor (in addition to his two industry chapters on oil and steel) which "draws together and discusses what is said in the book on some problems of common interest: criteria of efficiency, . . . the forms and effectiveness of competition, the functioning of very large and of rather small firms, the impact of cartel-like activities, the differential effects of the structure of the market for new capital and of some forms of taxation, and finally the impact of the State as owner, supervisor, subsidizer, and buyer, with special reference to administrative problems and to the evolution of price and investment policies." This chapter entitled *Retrospect,* taken by itself must surely be rated as one of the most perceptive essays on industrial organization to appear in recent years. The total effect of the book is to give to the reader not only an understanding of the structure, operation and performance of present-day British industry, but to provide him as well with some deep insights into the whole fascinating, baffling problem of industrial organization in our modern economies.

It is not possible within the limits of this review to outline and comment on each of the nineteen studies and the concluding essay, although each invites and is well worth a complete review. It is equally impossible to refrain from a few of the observations suggested by the host of ideas which the symposium generates and forces the reader to entertain. One is constantly impressed by the rich and intriguing qualities of Great Britain as a living laboratory for the study of industrial organization. Britain has on display something of virtually everything, not only historically but contemporaneously. On what other shelves will we find in discrete packages such an array of structures—free enterprise, self-cartelized enterprise, government-influenced, government-regulated and government-owned enterprise, and all shades of each? And sad old enterprises, happy growing enterprises, and leaping new enterprises, all in manageable sizes, even the big ones? And in this setting, how easy it is to slip into attractive comparisons and spectacular contrasts between different in-

dustries, different times and different countries—and how dangerous!

It is certainly true that Britain offers us the best present contemporary laboratory for continuing studies of government ownership in a democratic society. Speaking of such studies, it is interesting to detect the typical change in approach as we move from analyses of "private" to analyses of "public" industries. Part of the difference is due to ingrained attitude, part to opportunity. Public industries appear as single cellophane-wrapped firms; private industries as tangled complexes of individual firms whose variegated wrappings tend to obscure as much as they disclose. Public industries belong to "us," not to "them"; every move is a matter of legitimate public concern; every decision is made not by the "system" but by an identifiable official responsible to "us." It is quite understandable that the standards of judgment, even those of economists, are more uncompromising when public performance is being appraised than when private performance is being traced through the wilderness of the market. We expect private industry to be "workable"; we expect public industry to measure up to our formulae and curves.

Reading Gilbert Walker on "Carriage by Road and Rail" reminds us again that of all the problem children of industrial organization transportation is by all odds the most intractable. To devise a sensible pattern of public regulation to superimpose upon the indeterminate mélange of competition, monopoly and legitimately vested interests that characterizes the hauling of goods and persons by air, road, rail and water simply defies doing and, worse, the attempt leaves us muttering to ourselves. We are still aware of no promising proposals either for relief of our frustrations or for processes by which relief is likely to be found. Next page, please!

There are other incorrigible problem children on the playground, too: how in the case of nationalized or regulated industries shall we establish measures of "enough" investment and "enough" output that are sufficiently precise to permit us to doze comfortably with our determinations. How soothing it is in the case of unregulated private industries to leave these troublesome matters to the decision of the competitive market—until the quiet is shattered by some such categorical proposition as that of Burns: "obviously there is no lack of competition among the motor car firms," and by the equally categorical assertion that the effectiveness of competition is to be tested by results rather than by industry structure. What do we really know about competition *as an economizing force,* and what do we really know about the motivating, measuring and guiding power of profit and loss as essential constituents of the competition concept? Can competition which, as a matter of policy is held "in check" by "competitors" or which is permitted by them to influence only quality and service, but not price, be equated with *economizing* competition?

If, as strongly suggested in this book, our theories of competition do not fit the facts, it will behoove us either to discover and name the economizing forces which have demonstrably taken the place of competition, or to devise forces to play the role which hitherto we have assigned to competition. It will not do for us to assume, on the basis alone of "performance" in an inflationary, expanding, war and cold-war-stimulated economy that everything will be all right

if we don't look too closely. Economizing is society's business, and society will do well to discharge its responsibility to itself in this regard if free men are to continue to set the conditions and mold the shape of their freedom.

On a reminiscent note, it is pleasant in this book to encounter some old acquaintances—the restrictive price and production schemes of the 'thirties. Some of them are still walking the streets, sturdy and bold as brass, some are hiding out from the Monopolies Commission, and some are just sitting out their ineffectual lives on park benches. It is interesting to speculate on just how justified some of us were to be so disturbed over their earlier antics. Of course, some of the more assertive of the clan, such as the arrangements in coal and steel, have in the meantime become canonized; and this leads one to wonder about the real significance of the tug-of-war over the political status of the British steel industry. Honestly, does it make much difference whether British steel is owned privately and regulated by the government with the advice and consent of the Steel Federation, or is owned by the government and run by its former managers with the advice and consent of the Steel Federation?

All of the above is to say to economists: read *The Structure of British Industry*. Read it and weep over the state of our science at its very best. Read it and rejoice over your choice of a profession. Nothing is more fun than a barrel of economists—if the country can stand it!

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*Pricing in Big Business: A Case Approach.* By A. D. H. KAPLAN, JOEL B. DIRLAM, and ROBERT F. LANZILLOTTI. Washington: The Brookings Institution, 1958. Pp. xiv, 344. $5.00; paper, $2.00.

This volume is a companion to the 1954 Brookings study *Big Enterprise in a Competitive System*. The earlier study focused on the quantitative aspects of industrial structure, with special reference to the position of big business; this one is a detailed examination of big-business behavior, especially with regard to pricing its output.

The big-business candidates selected for study are 20 well-known corporations. Eight have assets exceeding $1 billion; nearly all are among the top hundred. All have been highly profitable business ventures. Over the period 1947-55 the group averaged a 22 per cent rate of return (before federal income taxes) on invested capital, and in no year did one of them report a net loss. But while all 20 were large and profitable, their most striking similarity was the willingness of all to cooperate with the Brookings staff. The largest in the group (General Motors) is 40 times as large as the smallest (Carrier Corporation). One (again General Motors) earned an impressive 51 per cent before federal income taxes; another (Swift & Company) earned a modest 12 per cent. Some (United States Steel, du Pont, Union Carbide and American Can) sell almost exclusively to other business firms; others (A&P and Sears, Roebuck & Company) sell directly to consumers. Among the 20 corporations the lines of products range from heavy earth-moving equipment to canned

tuna; within a single corporation (General Electric), from large public-utility generators to lamp bulbs.

Through interviews, questionnaires, memoranda supplied by company officials, and information available in public sources, the authors attempted to classify big-business pricing policies according to over-all company objectives. The attempt encountered numerous difficulties: company objectives changed over time; they differed between new and established product lines and multiproduct and single-product firms; and objectives articulated by company officials were not always consistent with most of the facts of company policy. Nevertheless, the authors found sufficient consistency to classify 13 of the 20 according to three overriding policy objectives: to achieve (1) a target return on investment, (2) stable prices and margins, and (3) a desired market position. The remaining 7 either priced to meet competition or relied heavily on nonprice competition.

While the classification of individual firms was often arbitrary—a fact readily conceded by the authors—the study's principal deficiency appears to stem from the approach itself. The predictive quality of microeconomic theory derives from the traditional assumption of profits maximization. It is perfectly clear of course that Standard Oil, International Harvester, du Pont, and other members of big business were subject to fewer constraints (public and private) in the 1890's than they are at mid-twentieth century. But while it follows that big firms must now seek profits by means which differ from those they once employed, it does not follow that firms generally no longer seek maximum profits. There is then no strong presumption that profits-maximizing is "operationally a less useful" (p. 129) working hypothesis than the set of objectives the authors substitute for it. For example, take the 7 firms whose pricing policies were assigned to the "target return" classification:

| | *Target Return* | *Actual Rate of Return, 1947-55* |
|---|---|---|
| General Motors | 15%-20% | 24.0% |
| International Harvester | 10% | 8.7% |
| Aluminum Corporation | 20% | 9.7% |
| du Pont | "a reasonable return" | 17.6% |
| Standard Oil Company, New Jersey | "a fair return" | 12.1% |
| Johns-Manville | "at least past level of earnings" | 14.4% |
| Union Carbide | no definite rate | 11.7% |

Four of the targets are much too evasive to hit, or indeed for any one to know when they have been hit. Two are higher than their respective firms customarily attain, so the firms presumably are pricing to earn as much profit as they can. The remaining firm has consistently earned a rate above its announced target, which suggests that the target is not an effective constraint. Moreover, all 7 firms produce a wide range of products, which tend to earn rates of return that vary inversely with the intensity of the competition with which they are confronted. The evidence that profits-maximizing is not operational—or not operating—is not convincing. The authors would have made better use of

the wealth of materials available to them had they accepted the traditional assumption as a tentative hypothesis, and then proceeded to test its validity in the light of the constraints on big business they subsequently encountered.

This aside, the book is highly informative. The material, much of which is new and unavailable elsewhere, is organized to illuminate with considerable clarity the way big business prices, and this was the authors' principal objective. While the authors do not explicitly set forth the public-policy implications of their findings, they nevertheless reach a policy conclusion: The large corporation, far from residing in that safe and comfortable haven of stable prices, steady sales, and assured rates of return, must contend with the dynamism of an ever-changing economy by which it is forced periodically to re-examine and change its policies. The findings indicate in substance: if one can safely generalize that market forces typically do not control big business, then an equally safe generalization is that big business typically does not for long control the market.

The volume marks a decade of diligent search for the facts on big business by A. D. H. Kaplan, its senior author. It also marks his retirement from the Brookings Institution's staff. It is to be hoped that it does not ring down the curtain on Brookings', or Kaplan's, published inquiries in this field. The searchlight of economists, and others, has been turned unceasingly on big business since well before the founding of the American Economic Association, but it has not yet revealed all the useful facts.

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*Our Competitive System and Public Policy.* By THOMAS J. ANDERSON, JR. Cincinnati: South-Western Publishing Co., 1958. Pp. vi, 586. $6.75.

The continuing quest for elusive causes of inflation has now moved into the field of monopoly and competition: trust busting, as it were, is fashionable again. Accordingly, it is a pleasure to report that Dr. Anderson has written an excellent volume that should be on the "must" list of any crusading Congressman.

This is not to imply that the book is preoccupied with questions of administered prices, market-sharing and the like. On the contrary, it presents a well-balanced treatment of all the traditional areas generally covered in a course on the social control of business. But what does differentiate this product is the author's successful attempt to integrate the body of antitrust material into the general framework of economic theory.

Typically, the course on monopoly and competition is a problem child in curriculum planning. At one extreme the course is structured along the lines of a legal study—with undue emphasis on the dates and circumstances of Supreme Court decisions. Theory is all but abandoned as case after case is cited in tracing the evolution of one or more of the various statutes. Opposed to this is the more recent attempt to set up the course as a study of market models, with legal precedents relegated to a few, scanty footnotes.

Anderson has avoided both of these pitfalls. If anything his bias is toward the institutional: history and the evolution of policy are indeed given their due. But these elements are carefully combined with basic economic principles, so that the result can be readily integrated into the main body of a department's offerings. An innovation for this type of book is the author's inclusion of several chapters on the competitive and monopolistic aspects of factor markets. In particular, the discussion of organized and unorganized labor markets is a model of clarity; the same may be said for the sober appraisal of the special problems of small-scale business. In analyzing these and similar matters he has chosen to use a descriptive approach entirely: there is not a single instance of the use of curves or diagrams in the entire book.

If any one section should be singled out for specific mention it is Anderson's critical evaluation of policy considerations and proposals made by various investigatory bodies since 1938. Alluding to the work of the T.N.E.C., the Twentieth Century Fund (Stocking and Watkins studies), and the Attorney General's National Committee to Study the Antitrust Laws ("Committee of Sixty"), the author points out that similarity of views rather than conflict characterized these three separate reports. Although Anderson does not offer any extensive set of suggestions for amending the antitrust laws, he does set up a balance sheet of sorts, outlining the major problem areas that continue to confront us.

It is this latter section that would well serve the needs of a fact-seeking legislator. For professional economists, the volume would seem to provide an excellent text or a fine reference for brushing up on this vital area.

ROBERT A. KAVESH

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*Readings in Industrial Organization and Public Policy*. Edited by RICHARD B. HEFLEBOWER and GEORGE W. STOCKING. Homewood, Ill.: Richard D. Irwin, 1958. Pp. xi, 426. $6.00.

This volume, selected by a committee of the American Economic Association, has two avowed purposes: (1) to provide the nonspecialist with a compilation of the more important articles in industrial organization and public policy which have appeared since 1942, and (2) to offer a book of readings for graduate courses in the field.

The articles selected are grouped into five partly overlapping categories: the structure of industries and markets; case studies in industrial structure and behavior; business practices and market behavior; industrial organization and economic theory; and competition, monopoly, and public policy.

In selecting the articles, the editors decided not to include sections of books, proceedings of conferences, and governmental hearings, although they did reprint some articles which had appeared in other books of readings. They placed a high priority on articles "based on important research and on articles developing significant viewpoints on analytical or policy issues," but a low

priority on "articles representing a digest or survey of the literature." In some instances, the editors included papers presented at professional meetings, together with the "criticism by formal discussants"; in others, such criticism was omitted.

In comparing this volume with its predecessor, *Readings in the Social Control of Industry* (1942), the editors underscore the changing conception of industrial organization and public policy. The older volume, they say, "was concerned almost exclusively with policy issues. Several of the articles dealt with the governmental problem of regulating prices, a topic omitted almost entirely from the present volume," where the emphasis is on "the characteristics of markets of few sellers and the policy problems growing out of them. Policy problems [in the current volume] are conceived as problems of antitrust rather than problems of government regulation. But as antitrust problems they are concerned with an industry's organization and economic performance rather than with traditional legal concepts of antitrust administration."

A book of readings is not likely to please everybody, and this volume is no exception. Different specialists, depending on their orientation and point of view, will inevitably have different ideas on the articles to be included and the topics to be emphasized. In fairness to the editors, therefore, a reviewer should do little more than comment on the quality of the selections (which in this volume is generally high) and on the standards used in making the selections.

My major criticism is that the articles in this volume lack unity and coherence. The editors are aware of this shortcoming, but attribute it to the imprecise boundaries of the industrial organization field and to the great variation in the content of graduate courses. This explanation is not entirely persuasive. The editors might well have chosen a few major *Leitmotifs*—e.g. the measurement of industrial concentration, the relation between size and efficiency, the economic criteria for antitrust policy—and presented divergent viewpoints on each. This would not only have made for greater organizational unity, but would have also enhanced reader interest. It would have impressed on the student the essentially controversial nature of this field and the sharp differences of opinion expressed in the literature.

Some major policy issues which are the subject of current and recurrent debate simply do not come to life in this volume. Though Congress, the courts, and the antitrust agencies are in constant turmoil over price discrimination and Robinson-Patman economics; though the economic and legal literature abounds with controversy on the subject, there is hardly mention of it in this volume. Neither the landmark effort of M. A. Adelman nor that of his critics, J. B. Dirlam and A. E. Kahn, are given any space by the editors. Administered prices, countervailing power, the role of competition in the regulated industries are treated, if at all, only in passing. The issues are never brought out in bold relief.

Finally, I must disagree with the editors' decision to exclude (automatically) materials contained in governmental hearings. In recent years, such hearings have become a favorite forum for the industrial organization

specialist. They contain not only important policy pronouncements, but also significant research findings of leading economists. The roster of witnesses, especially before the Celler, Kefauver, and Joint Economic Committees, reads like a "Who's Who" of the American Economic Association: George Stocking, Abba Lerner, Richard Ruggles, Fritz Machlup, George Stigler, Ben Lewis, Edwin Nourse, Kenneth Galbraith, Gardiner Means, and many others. Some of their statements are contributions fully as significant as the articles reprinted in this volume. The source in which they appear should not have been determinative of their selection or rejection.

Heflebower and Stocking have undertaken a difficult and unrewarding task. They have turned out a useful volume. The fact that some specialists would have used a different organizational pattern or a broader selection base should in no way detract from this effort.

WALTER ADAMS

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## Land Economics; Agricultural Economics; Economic Geography; Housing

*The Dynamics of Supply: Estimation of Farmers' Response to Price.* By MARC NERLOVE. Baltimore: Johns Hopkins Press, 1958. Pp. ix, 267. $5.00; paper, $4.00.

Nerlove's book, *The Dynamics of Supply,* is on a subject most economists abstract from, or at best, talk about with the aid of textbook illustrative numbers. The neglect of dynamics by the profession can be largely explained by the difficulty of measuring "adjustment paths," rather than by the belief the subject is not significant. An approach, alternative to Nerlove's in economic dynamics research is the study of "special situations" in which nondynamic confounding disturbances can be disregarded as quantitatively insignificant.[1] Nerlove's approach emphasizes the use and development of analytical tools capable of sorting out dynamic and static components in normally available data.

This book is of value to persons concerned with economic dynamics defined, not as historical change, but with reference to the adjustment paths between equilibrium points following a disturbance when all other changes in wants, resources and technology are held constant. His analytical techniques need not be restricted to problems of supply—they can be applied to problems of income and demand as well. Specifically, this book deals with the (1) analytical problems of elasticity measurement, (2) supply elasticity measurements for corn, cotton and wheat, and (3) use of such data applied to forecasting, stability conditions and the evaluation of alternative agricultural price support programs.

[1] Examples of the "special situation" emphasis are: (1) G. K. Brinegar, "Income, Savings Balances and Net Savings," *Rev. Econ. Stat.*, Feb. 1953, *35*, 71-74; (2) C. H. Berry, et al., "Short Run Effects Following Controlled Price Changes: Skim Milk," *Jour. Farm Econ.*, Nov. 1958, *40*, 892-902. An example of the second emphasis is: Milton Friedman, *A Theory of the Consumption Function*, Nat. Bur. Econ. Research, Princeton 1957.

The author's major concern is with the problem of estimation, but as the author states ". . . the *raison d'être* of such estimation is application." His methodological contribution is centered on the use of distributed lags—relevant to much of economic research. His substantive findings suggest higher long-run supply elasticities than have usually been found. His application to policy illustrates, unintentionally I think, what not to do, as well as what to do. Chapters 1 and 2 concern theoretical considerations; Chapter 3, the application of this theory to agricultural commodities; Chapter 4 directs attention to the data and their limitations; Chapters 5, 6 and 7 present detailed background data on corn, cotton and wheat production and the accompanying government programs; Chapters 8 and 9 present the statistical analyses, with applications included as two-fifths of Chapter 8.

In the balance of this review some of Nerlove's other publications on this subject, along with the comments of others, are footnoted;[2] secondly, an observation on research methodology is noted; and lastly, one of Nerlove's applications to agricultural policy is criticized.

The distributed lags, both the technological-frictional ones and the expectational ones, can be usefully thought of as sophisticated, statistical grab bags in that they include a number of unknown elements. The content of these grab bags is similar to the content of statements that assert that something behaves *as if* such and such were the case, rather than similar to statements based on verified propositions. Thus the usefulness of the elasticity estimates will be determined by how well they stand up on the basis of continued testing—they can not be taken as useful because the underlying propositions have been verified. Lest this comment be interpreted to imply that an *as if* analytical procedure is unproductive, let me hasten to add this method has been used in the physical sciences with great success and with a significant measure of success in other areas. The pitfalls to be avoided are those of (1) shifting back and forth between the two methods inappropriately, thus neglecting to prove (more precisely, not disprove) anything, and (2) failure to know when these alternative techniques were employed.

The usefulness of the elasticity magnitudes obtained in this study must be evaluated in light of the above considerations. In a small section of Chapter 8, pages 222-35, Nerlove's study is extended to cover "Welfare Losses under Alternative Price Support Programs" by use of an illustration. Attention is directed to the part of his illustration that is a misapplication of his findings

[2] (1) Marc Nerlove, "Estimates of the Elasticities of Supply of Selected Agricultural Commodities," *Jour. Farm Econ.*, May 1956, *38*, 496-509; also comment by G. A. King, 509-11. (2) G. F. Brandow, "A Note on the Nerlove Estimate of Supply Elasticity," *Jour. Farm Econ.*, Aug. 1958, *40*, 719-22 and Nerlove, "Reply," 723-28. (3) Marc Nerlove, "Distributed Lags and Estimation of Long-Run Supply and Demand Elasticities: Theoretical Considerations," *Jour. Farm Econ.*, May 1958, *40*, 301-10 and "Comments," 311-14. (4) Marc Nerlove, "Distributed Lags and Demand Analysis for Agricultural and Other Commodities," Dept. Agric. *Agricultural Handbook No. 141*, Washington 1958; also review by A. S. Goldberger, *Am. Econ. Rev.*, Dec. 1958, *48*, 1,011-13. (5) Marc Nerlove and William Addison, "Statistical Estimation of Long-Run Elasticities of Supply and Demand," *Jour. Farm Econ.*, Nov. 1958, *40*, 861-80.

to agricultural policy. He assumes that the alternative uses of resources can be determined from a knowledge of supply elasticities—an assumption with which I have no quarrel when the elasticities are derived from and applied to situations in which "relatively free" markets exist. The author recognized this limitation when he calculated his elasticity estimates on the basis of pre-1933 data. He did not recognize this limitation in application when he presented some illustrative estimates of welfare losses when output is restricted through the use of quotas or direct controls. Supply elasticity estimates are used along with other data to compare welfare losses under direct control programs, direct payments programs and crop destruction programs. The trouble with this procedure, in making welfare comparisons, is that "free market" elasticities are not relevant to situations where direct controls are used. Resources denied use in the production of usual outputs, because of direct controls, may well have productivities near zero in their next best alternative uses. The productivity of these resources in their next best use will almost certainly be lower than that implied by accurate "free market" supply elasticity estimates. Useful data on the productivity of inputs released from their usual uses, as a result of quota impositions, can be obtained from other sources—one of the most direct being the prices of quotas—bootleg, as well as legal.

This volume, along with other literature on the subject, is helping to put new life in a much neglected subject that requires a lot of hard work. More publications of this sort are needed and should be encouraged.

GEORGE K. BRINEGAR

*University of Connecticut*

*Perspectives on Conservation: Essays on America's Natural Resources.* Edited by HENRY JARRETT. Baltimore: The Johns Hopkins Press, for Resources for the Future, 1958. Pp. xii, 260. $5.00.

These essays by 23 authors are the effectively edited and revised papers and discussions of a forum organized by Resources for the Future and held in Washington, D.C., during the first three months of 1958. The forum consisted of six programs, each with a principal paper and two or three shorter discussion papers.

The six major papers are concerned with a historical retrospect of the conservation movement (Ernest S. Griffith), a survey of the role of technology in natural resources (Thomas B. Nolan), resource demand and living standards (John Kenneth Galbraith), urban growth and natural resources (Luther Gulick), the political economy of resources use (Edward S. Mason), and the administrative organization of resource development (Gilbert F. White).

Review of each individual paper and discussion is excluded by the constraint of space. In any event, such detailed reviews do not appear necessary because most contributors to this forum have expressed similar views previously elsewhere.

In appraising the volume as a whole, the editor, Henry Jarrett, states in

the introduction that the selection of the authors and the grouping of the subject matter "made it possible to look at old problems from new angles and with fresh eyes and to relate resource conservation more closely with the country's total economy and social structure and with world developments." This and similar statements disclose an unawareness of the pertinent literature—not uncommon in the staff writings of Resources for the Future. The particular selection of contributions is interesting individually; but it does not fulfill the claims and is scarcely a representative sample of the original and systematic work that is concerned with resources in the social and natural sciences.

The book under review contains perspectives on conservation not because "new angles" and "fresh eyes" are applied in observation, but because a multiplicity of often contradictory views is presented through the six programs, and within each program, through several discussions of the major paper. Perspective in this sense is the strongest feature of the volume.

As a corollary, symposia of this kind frequently suffer from a lack of systematic thinking through a subject matter. This characteristic is also the main weakness of these essays. One may submit that such thinking through is the greatest need in the social science aspects of conservation policy.

S. V. CIRIACY-WANTRUP

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*Agricultural Adjustment Problems in a Growing Economy.* Edited by EARL O. HEADY, HOWARD G. DIESSLIN, HARALD R. JENSEN, GLENN L. JOHNSON. Ames: Iowa State College Press, 1958. Pp. xv, 315. $3.95.

This book is an assembly of papers presented at a conference on "Adjusting Commercial Agriculture to Economic Growth" and published under the sponsorship of the North Central Farm Management Research Committee. This conference was a follow-up to an earlier one on "Resource Productivity, Returns to Scale and Farm Size," the report of which was published by the Iowa State College Press in 1956.

The first paper by Earl O. Heady and Joseph Ackerman deals with the income and resources-allocation problem. Its most significant contribution is a clear statement of the position of the farmer in relation to national economic growth and the need for the transfer of resources from the farm to other segments of the economy. Farmers have made significant contributions to an expanding national economy without sharing equally in the fruits of growth. In fact the demands for some farm products have declined while the costs of goods and services employed by farmers in production have generally increased. This has resulted in a cost-price squeeze on the farmer. The conclusion is that some resources must be transferred out of agriculture.

Recent developments in agricultural production are reviewed by Lynn S. Robertson and Howard G. Diesslin. The trends in reorganization have been in the right direction but too slow. The effects of the transfer of resources have

been more than offset by the effects of technological research and education upon the productivity of the resources remaining in agriculture. The paper by Sherman Johnson and Glen Barton dealing with the effects of research and education presents an excellent analysis with suggestions for directing more research toward resolving or reconciling the conflict between national progress and income improvement for the farmer.

The prospective demand for farm products in relation to prospective supplies is examined by Norman Collins and George Mehren to determine the extent to which the growth of population and other sources of potential increase in demand might contribute to the solution of the problem. After careful analysis it is concluded that total consumption might increase by 40 to 45 per cent in the next 20 years; but production, now in excess of consumption requirements, is likely to increase even more. The conclusion is that there is no effective method of solving the problem through increasing demand. Therefore, it is necessary to seek a solution through adjustment of farm production.

The analysis of the supply function in farm production by Glenn Johnson is perhaps the most significant breaking of new ground presented at the conference. The new ground is in extending the theory and analysis of fixed assets in relation to production and the position of the farmer.

Willard Cochrane presents an interesting paper on some additional views on supply and demand including significant comments upon the papers by Glenn Johnson and others dealing with demand in relation to supply.

The internal structure of agriculture is analyzed to determine what reorganization of the use of resources might contribute to the solution of the problem. One suggested solution is to shift surplus cash crop resources to livestock production. James Bonnen indicates that only about one-quarter of the annual crop surplus could be absorbed in such a shift without serious consequences to that industry. He also concludes that taking land out of cultivation is not the answer because other resources are readily substituted for land to maintain production by increasing yields. Capital also is readily substituted for labor in maintaining or increasing production. The conclusion of this analysis is (p. 126): "Any effective effort to reduce production must involve the simultaneous transfer of some combination of labor, land, and capital resources to nonagricultural pursuits."

Reduction in the labor force as a means of solving the farm problem is discussed at length. Some agricultural economists consider that shifting labor to other cccupations is the best solution to the farm problem. In the first paper (by Earl Heady) dealing with this proposal it is noted that in the last 20 years a reduction of about one-third in labor input has been accompanied by an increase of 38 per cent in output. That is, technological improvements and the substitution of capital for labor have more than offset the migration of labor to industry. It seems clear, therefore, that the rate of migration must be increased substantially to be effective in reducing output.

Among the ways and means of increasing labor mobility discussed by D. Gale Johnson are: improvement in the educational facilities of low-income

areas, extension of information as to job opportunities, and loans or grants in aid of transfers. An analysis by Vernon Ruttan concludes that rural industrialization and local economic development may make significant contributions in some areas but longer distances or geographic mobility are necessary to relieve agriculture generally of surplus labor.

An analysis of current adjustment programs by G. E. Brandow indicates that they are not contributing much toward solving the surplus production problem. The soil bank has contributed to some extent to reducing the acreage of some crops, but price maintenance has encouraged the maintenance of production through increasing yields per acre. It is suggested that the programs be revised so as to be more effective in encouraging adjustments of supply in relation to requirements at prices that will improve the income position of farmers.

There were also papers presented which dealt with individual and social goals in relation to economic growth, the farmers' social and political psychology and the value problem in agriculture policy. One paper, by C. B. Baker, deals with the relation of the individual's interest in income to the social interest in the national product. A second, by W. R. Parks, emphasizes the interest of the farmer in security and deals with the problems of the farmer in obtaining political recognition of his interests. Equality of opportunity is emphasized by Kenneth Parsons; and he believes that the means of obtaining adjustments should be considered as important as the end results.

In summary the implied long-run objective of this conference was to suggest ways and means of eliminating surplus production so that farmers may obtain more adequate income in the free market. The objective for agriculture is to reduce production to levels in line with current requirements and reduce the unit costs of the products. Fewer farmers with larger farms and more extensive use of capital could supply the national needs of a growing economy from agriculture and receive more adequate net incomes in the free market.

Desirable adjustments are under way but they are too slow. They should be speeded up through research, education and extension services more specifically directed to adjustment problems. Government financial aid to the migration of surplus farm labor is also suggested. The present government programs should be revised to be more effective in encouraging and aiding adjustments.

The most significant contribution of this book is in bringing together analyses of various aspects of the agricultural adjustment problem. The several participants in the conference have written and spoken extensively in their special-interest fields. In this conference an attempt was made to concentrate the attention of each analyst on the relation of his special interest to the overall problem of adjustment. The result is a good reference book for economists and other students of agricultural economic policy.

O. C. STINE

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*Introduction to Agricultural Economic Analysis.* By C. E. BISHOP and W. D. TOUSSAINT. New York: John Wiley & Sons, 1958. Pp. xiv, 258. $5.25.

The authors' objective is "to provide a theoretical foundation for use in analysis of agricultural economics problems. . . . Economic theory is presented in an elementary but rigorous form, and illustrations are developed to show the use of theoretical concepts in solving empirical problems." Written primarily for sophomores and juniors, without prior exposure to formal economic analysis or mathematics, the book includes four sections: (1) economic organizations, the nature of decision making, and agriculture as a part of the general economy; (2) production and supply, utilization of productive services, cost functions, and a bit on uncertainty; (3) consumption and demand, international trade, and long-term and short-term price movements; (4) technological influences, population, and income with attention to the low-income areas in agriculture.

Sections 1, 3, and 4 are treated in broad terms and rather lightly. The heart of the book, and its main contribution as an instruction vehicle, is in section 2 which constitutes about 60 per cent of the entire volume. Following the long-established tradition in agricultural economics, it presents a more detailed discussion of what is commonly referred to as production economics than is found in most introductory texts on economic principles. Although oriented to concepts and analysis, the section on production and supply includes a very generous portion of examples selected from empirical studies by various researchers in the economics of agricultural production and resource use.

Another feature of the book is its more than usual attention to multiple products and uncertainty in production decisions, compared with most introductory texts on economic analysis. Although the beginning student is given reasonably detailed exposure to short-run production analysis, the treatment of long-run analysis will have to be supplemented heavily by the instructor's lectures.

In contrast with the rather detailed treatment of production and supply (at an elementary level, but certainly not rigorous) the explanatory exposition of consumption and demand is slim and sketchy. The student is made aware of price elasticity and income elasticity. Long-term changes in per-capita consumption are attributed primarily to changes in consumers' preferences and changes in income. The role of relative prices in influencing changing consumption patterns is neglected. Cross elasticities of demand with respect to price, for example, are not recognized in spite of the evidence that consumers do shift among food products in response to changes in relative prices as well as in response to changes in income. The discussion of demand will require shoring up and fortification by the instructor.

In terms of market structure, the analytical parts of the book deal almost exclusively with pure competition in buying and selling. The student is not exposed to the analysis (and problems therein) of alternative forms of market structures. Yet, a case can be made that American agriculture is breaking away from pure competition, with developments stemming from the growth of pro-

curement and marketing cooperatives and the extension of forward and backward vertical integration in addition to governmental participation in various forms. Some interesting and significant examples of and problems in nonpure competition may be drawn from the contemporary scene in American agriculture. But how economists struggle with the analysis of such problems is omitted from the book; the authors leave that task to the instructor.

For an elementary text heavily oriented towards production and supply for a firm in a purely competitive environment, the book has much in its favor, particularly the wealth of illustrations to show the use of theoretical concepts in analyzing empirical problems. To give the student a balanced introduction to agricultural economic analysis, however, the instructor will likely want to plug some significant gaps by his lectures and with the use of supplementary study materials.

SIDNEY HOOS

*University of California*

*Lesnaia promyshlennost' SSSR: Statisticheskii sbornik.* (The Timber Industry of the USSR: Statistical Handbook.) Moscow-Leningrad: Goslesbumizdat, 1957. Pp. 295.

Since 1956 the Soviet Union has released considerable economic information in the form of statistical handbooks. The timber handbook is one of the few dealing with a specific industry. For the most part, only favorable information is reported. Extensive tables describe the large quantity of forest wealth of the USSR, and the shift of logging activity from the "timber-deficit" areas of the South and West to the "timber-surplus" regions of the North and East. Impressive data indicate the manifold increase in total logging and wood-products output as well as the growth of logging mechanization within the timber ministry. (Since publication of the handbook the ministry has been abolished in line with Khrushchev's decentralization of administration.) Figures on the logging labor force indicate the virtual elimination of seasonal workers.

The favorable information presented is, in fact, riddled with statistical uncertainties. While outright falsification is apparently absent, the figures are frequently used in ways outside the bounds of Western statistical practice. For example, important qualitative characteristics of Soviet forests are obscured. Although total forest area and volume are divided into age classes, the important fact that much of the mature timber is small is not indicated. Also ignored are the large quantities of waste-wood in the vast stretches of overmature and swampy taiga. Figures on logging mechanization fail to indicate that only certain processes have been mechanized and that other processes of increasing importance remain completely performed by hand labor. In Soviet terminology, the "mechanization of production" has increased while the "mechanization of labor" remains low. While figures show the growth of a permanent labor force in logging, collateral information from Soviet timber

industry periodicals indicates a continuing high rate of labor turnover, much of which is interindustry turnover.

Considerable information of importance is not disclosed. Production costs are treated only sketchily. For example, a table is presented showing costs in the entire "factory-plant" branch of the timber ministry. Presumably, products included range from wood chemicals to yo-yos. Pricing of wood products and the policy on stumpage charges are not examined. Such information is absolutely necessary, however, if we are to assess the timber industry as a functioning branch of the Soviet economy because the industry is tied to the economy by just such centrally determined prices. No data are provided on regional or even total forest growth. Understandably, no mention is made of the role of "forced labor" in logging, although it must be considered in evaluating figures provided on labor productivity. A pervasive and serious limitation of the handbook is that only centralized wood enterprises are considered (except for figures on total production). In 1957 almost half the logging output was carried out by "self-suppliers," i.e., enterprises in steel, food, fishing, etc., outside the ministerial apparatus, which require wood in the normal course of production but have been unable to obtain it from central sources of supply. Nor is any information other than total production figures provided on agricultural timber supply, certainly a fact of great importance given the natural and institutional vicissitudes of Soviet agriculture.

Ten thousand copies of the handbook were printed, leaving few for circulation abroad after domestic requirements had been satisfied. Undoubtedly, the handbook is intended primarily to serve the needs of its domestic readers; thus to object to the lack of definitions of statistical categories utilized is to quibble. No one would accuse the Department of Commerce of obscurantism, e.g., for failing to define gross national product or disposable income each time those terms were used. Similarly, the frequent conversion of absolute figures into relatives may annoy Western readers. Perhaps we should not discount the possibility that many Soviet users of the handbook may lack mechanical means to perform just such simple computations. The absence of figures on self-suppliers and other items of interest possibly reflects weaknesses in the data-gathering process.

Finally, it must be remembered that the text was published to serve Soviet interests, but to do so in such a way as to protect the regime from criticism. The handbook must be used with great selectivity and restraint if meaningful conclusions are to be drawn from the data presented.

W. DONALD BOWLES

*The American University*

## Labor Economics

*As Unions Mature: An Analysis of the Evolution of American Unionism.* By RICHARD A. LESTER. Princeton: Princeton University Press, 1958. Pp. xi, 171. $3.75.

The time is ripe for a study of labor's "coming of age." In the 'thirties the prolonged infancy of unionism finally gave way to stormy adolescence. Only

in the last two decades can it be said that unions have achieved power, status and maturity, but with such maturation has come widespread public demand for "responsible" union behavior.

Lester's analysis of union maturity is not, however, designed to forge a standard against which union performance can be judged. Rather, his purpose is to evolve a theory of union growth. At the outset he notes the deficiencies of existing theories of unionism: Hoxie failed to reconcile competing forces operating on the union movement; Perlman's theory is said to be unnecessarily static with its focus on job consciousness; and Tannenbaum's analysis is largely historical. In his own "evolutionary" theory he indicates that the character and philosophy of unionism are influenced by the institutional setting in which the union movement finds itself. In his mind, a "theory" of unionism must be sufficiently pliable to explain labor's adaptation to continuing changes in the labor market.

Throughout his study Lester stresses the political aspect of unionism: Union success is measured not so much by economic power as in the capacity of the union to win certification elections, to be politically sensitive to employee needs, and to be politically effective in its relationships with the public and Congress. ". . . the theory of union behavior should ultimately extract more sustenance from political theory than it has been able to draw from economic models based on the theory of the firm" (p. 132).

The core of Lester's analysis is the development of a union growth curve. In his view, unions at the outset face a hostile and turbulent environment and overcome opposition through spirited militancy. A youthful energy and crusading spirit infuses the organization, giving it momentum and direction. Leadership displays a missionary zeal. Should the permanence of the union be assured, however, the aging process begins in two or three decades. The locus of union power shifts from the local to the national office; centralization discourages rank-and-file participation in union affairs and makes possible the building up of a political machine. The adjustments of the union member to his new union bureaucracy—usually made imperceptibly through time—has its counterpart in the adjustment of management to the union. Less and less resort is made to strike activity; the mutual interests of the union and management are made apparent (on occasion to the point where the union may not secure wage increases equivalent to those possible with the free play of market forces). The national office of the union insists on strict adherence to contract terms and in one sense becomes an element in management's control system. Ultimately the union disregards its class orientation in its accommodation to both management and society. The bargaining process shifts from table-pounding emotionalism to reasoned analyses of statistics, law and economic theory. The union staff specialist assumes additional importance: policy is made from the top and is sold, through skillful communications, to the membership. The union faces less of a challenge from management, less opportunity (and perhaps less need) for factionalism within, less rivalry from other unions. And the security of the union makes possible the security of the union officer.

The material success enjoyed by union officers is a further conservative force. Officers acquire the attributes of success and the prestige symbols of

management. And with labor's subsistence needs satisfied, union interests becomes diversified. As the union is no longer a microcosm of society but rather a substantial cross-section of it, it imbues and is imbued by the values of the community. It becomes concerned with the wider and more diffuse problems of all society, diluting its provincial "union-only" orientation.

The total effect of these forces is to reduce the pace of union growth. Barring radical change in our economy, Lester sees no increase in real union membership in the next ten years. As a test of his analytical framework, Lester provides a brief case study of unionism in Sweden and England and follows this with a thumb-nail sketch of the Amalgamated Clothing Workers, the United Automobile Workers, the Carpenters, the Teamsters and the Mine Workers. He finds that the organizational history of unions, both here and abroad, fits pretty well into his conceptual framework. Even the youthful Automobile Workers' union displays a few symptoms of this aging process.

Lester's analysis is not based on fresh research but is more, as he himself describes it, a "think piece," and is, in reality, a conversational review and reflection of his extended study of the labor movement. There is little in his analysis that is novel, for it largely represents a synthesis of existing research in union government. His task has been largely one of aerial reconnaissance or a mapping out of terrain. As such, *As Unions Mature* provides an appropriate point of departure for students planning further explorations of contemporary collective bargaining.

PAUL SULTAN

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*National Wages Policy in War and Peace.* By B. C. ROBERTS. London: Allen & Unwin Ltd.; New York: Macmillan, distrib. Pp. 180. $3.50.

This book is another contribution to the discussion of inflation, its causes and the alternative policies for its control, in which economists have frequently engaged since the end of the second world war. For the first time, however, a comparative study of what has been done and its degree of success in several different countries has been made. As the title of the book suggests, Roberts examines specifically the relationship between wages policy and the problem of maintaining economic stability during both wartime and peacetime.

The author studies the wage controls and wages policy in Britain during the war and under both Labour and Conservative governments, American wartime policy and the wage stabilization program of 1950-1952, experience in Sweden, Australia, Netherlands and West Germany. And with the exception of West Germany he finds that the change in the price level between 1939 and 1957 has been about the same in each country and irrespective of the wages policy. Australia has used a system of compulsory arbitration; Sweden a coordinated program of voluntary agreements between national trade union federations and national employer associations; Holland has adopted a comprehensive system of statutory wage regulations. Both British and American centrally administered wartime wage controls were unable to bottle up infla-

tionary pressures. Efforts in Britain by both the Labour and Conservative parties to effectuate a national wage policy of "wage restraint" by the unions or one of wage controls have been without success in checking the rise in prices. Accordingly the author concludes that, "Whatever in theory might be said in favour of a centralized system of wage controls, it cannot be said that those examined . . . have succeeded in preventing inflation" (p. 160).

If wages policy cannot check inflation, then what? Roberts argues that the root cause of price increases in the countries studied was not "wages rising faster than output, leading to an increase in labour costs that is passed on to the consumer in the form of higher prices" (p. 161). Rather, increase in the supply of money, brought about by the conscious political decision of the monetary authorities (p. 162) and "the transfer from idle to active balances and the increase in the turnover of deposits and cash in circulation has financed the continuance of inflation by building up the level of demand to meet each new [cost-induced] price level" (p. 163). In other words, "the problem [is] one of preventing the generation of excess demand . . ." (p. 164).

Roberts recognizes that an "attack on inflation by curtailing the supply of credit and capital expenditure will have the immediate effect of checking output." But he says that "this price may inevitably have to be paid to achieve stability" (p. 165). He sees no necessity, however, for average unemployment rates to rise much in the long run if swings about the average are allowed.

The choice which the author offers between inflation and a small rise in average unemployment is, however, oversimplified and incomplete. Roberts does not discuss what might happen to the distribution of income between labor and property, or between union and nonunion workers, and to the allocation of resources among various sectors of the economy. He implicitly assumes, for example, that unions will be unable or unwilling to alter the distribution of income. But this is not certain. The very expansion of the money supply since 1945 may well explain why unionism has been unable to influence the structure of wages substantially. Certainly a "get tough" monetary policy will not necessarily reduce or contain the unions' perpetual demand for higher real wages or a larger share of national product. Even though unions respond to unemployment, as the author indicates, this is a matter of degree. As union rates go up, new workers are denied jobs by employers; unemployed union members seek work elsewhere, and the pressure of "excess" supply in nonunion markets holds real wages below those pushed up by the unions. Only if the momentary authorities will accommodate the autonomous union wage increases by a corresponding expansion in the money supply will the growth in aggregate demand be sufficient to permit an increase in all factor prices in such a way as to avoid a significant change in the distribution of income.

In summary, there is a great deal to command in this book and very little to criticize. It is excellently written, well organized and easy to read, authoritative and well documented, and provides a comprehensive survey of the institutional arrangements, the wages policy and anti-inflation measures, as well as a wealth of factual data, in the six countries covered. Although

written primarily for a British audience and as an analysis and guide to policy in Britain, the book merits the close attention of American readers who are interested in the problem of inflation control and the role of unionism in achieving economic stability.

KENNETH M. MCCAFFREE

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*Les traitements des fonctionnaires et leur détermination (1930-1957).* By ANDRÉ TIANO. Paris: Ed. M.-Th. Génin, Librairie de Médicis, 1957. Pp. 554. 3,500 fr.

Since its hard core was established by the first Napoleon, the French civil service has weathered revolutions and restorations, the rise and fall of countless governments, and a number of efforts to reorganize the service itself. Professor Tiano, one of the rising generation of French labor economists, has devoted this book to an exhaustive study of wage trends, wage structure, and wage determination in this hardy perennial institution.

Tiano estimates that *fonctionnaires*, together with their retired list, dependents, and recipients of survivors' benefits, made up more than 11 per cent of the French population in 1947, with their total receipts from the government comprising about the same percentage of net national income. In addition to civil servants in the American sense, it should be noted that *fonctionnaires* include the armed forces, telephone and telegraph, radio and television, and public schools from kindergarten to university.

The first half of the book is descriptive. The main findings are that real wages in public service were about the same (in terms of basic salary scale) in 1956 as in 1930, though with big swings in-between; that salary scales had roughly kept pace with wage rates in private employment over time, but were generally a bit lower for similar work in the past decade than in private or nationalized establishments; that wage differentials had been compressed; that pay varied greatly from agency to agency. Certain estimates are based on personal interviews as well as official data, and "extracurricular" sources of income are treated with Gallic frankness.

Real incomes have increased more than statistics of basic salary would indicate, but the chances for extra money vary from agency to agency and job to job. Overtime pay in lower grades and flat-rate overtime allowances in higher ones (whether overtime is worked or not), efficiency premiums, free housing for certain officials and residence allowances for others, shoe-wear allowances for postmen and extra pay for professors who grade more than their quota of bluebooks, when taken together with family allowances, mean that either a doorman or a director-general can sometimes double his basic salary. Economists will be glad to learn that the chance of doing so is greatest in the Economic Council; teachers can take pleasure in the fact that their French colleagues have been up-graded steadily through the years and can supplement their salaries by tutoring or by doubling as town clerks.

The second half of the book is devoted to wage determination. While

the percentage of union members is higher in public service than elsewhere, the weaker Socialist and Catholic unions are more strongly represented than in the country as a whole. Though joint administration-staff committees were set up in 1947, the unions prefer to press their claims through other channels. Strikes are usually short and symbolic, since the right to strike is limited, and rivalry between unions and between branches and grades within the service tends to weaken solidarity. The *fonctionnaires* also exert political pressure, sometimes through deputies with ministerial ambitions who—at least during the Fourth Republic—tended to push the claims of the ministry of their choice, sometimes through priests or civil servants sitting as deputies.

Wage determination is seen as a process of jockeying between administrations (interested in keeping within their budgets and still getting the work done) and unions, whose claims are backed by parliamentary pressure, appeals to public opinion, and strikes. Tiano concludes that higher officials lean heavily on civil service unions and unions grouping higher-level employees in the public and private sectors, that lower officials have more solidarity with the working class as a whole, and that there is some evidence that the post-telephone-telegraph unions exert leadership as regards strikes in both private and nationalized sectors of the economy.

Tiano's conclusions about wage determination may be open to question, but as a work of documentation the volume deserves serious consideration. American economists who can read French and have the patience to wade through the alphabet soup of French government agencies and the numbers and letters which identify salary scales, the intuition to make up for incomplete table headings, and a reading-glass for some extremely complicated and crowded graphs, will find themselves rewarded with a wealth of statistical and documentary information which is available in no other single source.

A. Peter Ruderman

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## Population; Welfare Programs; Standards of Living

*The Population of Japan.* By Irene B. Taeuber. Princeton: Princeton University Press, 1958. Pp. xx, 461. $15.00.

The publication of Mrs. Taeuber's *magnum opus* has been eagerly awaited by economists, demographers, and various kinds of regional specialists. Preceded by a lengthy series of tantalizing and authoritative articles, *The Population of Japan* gives the author her first chance to present a full-dress, detailed and integrated picture of Japanese demography. She has made the most of this opportunity. This book lives up to the high standards which one has learned to expect from Princeton's Office of Population Research, and it is quite safe to say that Mrs. Taeuber has written the definitive work about Japanese population for at least a decade. (I shall be immensely surprised if a Japanese translation of this work does not appear in very short order.)

This study encompasses an impressive variety of topics. The author leads off with a fascinating discussion of population in the premodern period, followed by an analysis of the transition era from 1852 to 1918. Detailed work starts with the results of the first modern census of 1920, and here Mrs. Taeuber broadens her methods considerably. She discusses the changing composition of the population, underlining the economic aspects, which indeed form a sort of leitmotiv to the entire volume. There are long sections on internal and external migrations of the Japanese people, with the former providing a vivid picture of urbanization. Natural demographic movements—marriage, fertility, mortality, and natural increase—are all studied. A section also examines Japan's demographic ordeal during the war and in the postwar period, and discusses current policies and future problems. Note must also be made of an extremely valuable bibliography covering Japanese and foreign sources.

For obvious reasons the period since 1920 is heavily emphasized in this work. Mrs. Taeuber is quite pessimistic about whether precensus population data are accurate enough for meaningful analysis. Furthermore, her numerous, cautious, and well-documented conclusions rely primarily on census benchmarks; yearly series are infrequent. It would be foolish to take issue with her in these matters; but let us say that for quantitative economic historians certain large questions remain unanswered. For example, the process of urbanization before 1920 remains something of a questionmark.

Some of Mrs. Taeuber's most valuable insights come about through her regional classifications. She divides the prefectures of Japan into four principal categories—metropolitan, other industrial, intermediate, rural—and finds interesting demographic contrasts and similarities. In examining urbanization, the mechanical administrative *shi* and *gun* classifications are largely abandoned, and instead more rational typologies are employed in analysis. Here the economist must doff his cap in envious admiration, and conclude that population census-takers have dealt more kindly with demographers than economic statistics collectors with economists. In measuring economic development in terms of, say, national product or capital formation, the economist must usually rely on output figures which do not permit a breakdown in other than fairly large administrative units. This was certainly the case with the prewar census of manufactures in Japan. And it is of course also true that the prefectural statistics of Japan contain much more demographic than economic information. One must conclude that frequently it is not possible for quantitative economists to make direct use of demographic advances. However, there can be no question whatever about the over-all value of this type of analysis for the study of economic growth.

Mrs. Taeuber's findings and opinions are so rich and diverse that it is impossible to attempt an enumeration. But one further aspect of her work deserves comment. All through the book—implicitly and explicitly—she stresses duality, by which one means the intermingling of modern and traditional aspects in Japanese society. From the point of view of demography,

this may have meant that there are often no sharp divisions between urban and rural areas, or that among certain classes of workers in metropolitan areas rural characteristics of employment and reproduction have persisted for a long time. Similar characteristics are of course found in the economic analysis of Japanese development. The persistence of the demand for traditional commodities, ranging from houses to certain types of food, the apparent need and consequent indestructibility of the small (often midget) unit of enterprise, and perhaps the entire system of labor relations are different sides of the same coin. Certainly not all the aspects of this duality are adverse to industrialization; some may have made a vital contribution, and need to be considered in the general study of development policies.

I conclude with a minor complaint. Mrs. Taeuber's contribution could have been enhanced by the inclusion of a chapter comparing Japan's demographic experience with that of other industrialized countries. This reader's perspective would have benefited.

HENRY ROSOVSKY

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*The Older Population of the United States.* By HENRY D. SHELDON. Census Monograph Series for the Social Science Research Council in cooperation with the Bureau of the Census. New York: John Wiley; London: Chapman & Hall, 1958. Pp. vii, 223. $6.00.

Like the other monographs in this series, the objectives set for this volume are two-fold: the presentation of detailed, statistical information collected by the Census and other public and private sources on a particular subject of current interest; and the "broad exploration of the new questions suggested by the new information, as well as narrowing the elements of doubt and controversy on old questions" (p. vi). The present volume fulfills both objectives. The pertinent issues are raised and the data analyzed against a broad background of knowledge and insight. The authors never lose themselves, and consequently they do not lose the reader, in a maze of detail.

The material is skillfully organized to show the position of older pepole in those aspects of the society on which the census gathers information—age structure, geographical distribution, employment, occupation, marital status and the family life cycle, living arrangements, housing, and income. In each setting the position of older people is analyzed within the context of the processes which have contributed to their present status and may influence future trends.

There are a few surprises. For example, self-employment appears to be less favorable for occupational longevity than is commonly supposed. Those who worry about the growing proportion of older people in the population will be relieved to learn that this is by no means certain. For the most part, however, the findings do not refute familiar generalizations. They limit and refine them. Analysis of the varied effects of migration on age structure by states and type and size of communities is particularly noteworthy, as is also

the use of survival rates to measure the degree to which older people continue in former occupations and the extent of employment opportunities in particular occupations. Relationships between age and labor-force participation still defy generalization, except for the obvious statement that declining energy and health are major factors in retirement, and even here the subtler relationships between health and employment remain nebulous. Situations confronted by older people with changes in family status are sensitively treated. The growing proportion of older people without earnings indicates the need for a broader system of support. The chapter on housing shows up the inadequacy of data on this subject.

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"Some surprise may be occasioned by the small figure which John Stuart Mill makes in these pages, despite the thirty-five years he spent as an official at the India House. But, as I have pointed out, his official work was almost entirely confined to handling political relations with the Indian states, and neither by temperament nor belief was he fitted to take over the leadership of the doctrinaire programme laid down by his father and Bentham" (from the preface).

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## Population; Welfare Programs; Standards of Living

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## Related Disciplines

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# NOTES

A nominating committee consisting of John D. Black, chairman, Kenneth D. Roose, Alice E. Bourneuf, Raymond T. Bowman, James M. Buchanan and George Garvy has submitted the following slate of nominees for 1960 officers of the American Economic Association:

*President:*
Theodore W. Schultz

*First Vice President:*
Paul A. Samuelson

*Vice President:*
Lester V. Chandler
William J. Fellner
Raymond W. Goldsmith
Robert A. Gordon

*Executive Committee:*
Armen A. Alchian
John K. Galbraith
Frank C. Pierson
Ralph A. Young

The annual meeting of the Association will be held at the Sheraton-Park Hotel, Washington, D.C., December 28-30, 1959.

## Norman S. Buchanan Memorial Award

The services of the late Norman S. Buchanan to teaching and research in economics and to the strengthening of the social sciences are being commemorated by his friends through the establishment of a Norman S. Buchanan Memorial Award, at the University of California, Berkeley, in the form of a prize or scholarship for excellence in economics. Contributions to this fund, which are tax exempt, may be sent to: Philip E. Mosely, Chairman, Norman S. Buchanan Memorial Committee, 58 East 68th St., New York 21, N.Y. Checks should be made out to The Regents of the University of California.

## New Publications

A new journal, *Labor History,* sponsored by The Tamiment Institute in cooperation with the Society of Labor Historians, will be published in late 1959 or 1960. It will publish, in addition to original research in labor history, studies of specific unions and the impact of labor problems upon ethnic and minority groups; articles on the theory of labor history; and studies of foreign labor movements. Contributions may be addressed to the editorial director, Dr. Norman Jacobs, care of The Tamiment Institute, 7 East 15th St., New York 3, N.Y.

*The Social Sciences—A Journal of Student Research,* a periodical designed specifically to broaden the outlet for student research in the social sciences, will be published in the near future. Articles submitted should not exceed 2,000 words in length and should be typed in a form meeting publication standards. All communications and manuscripts should be sent to Professor Edward Rothstein, Department of Sociology, Dickinson College, Carlisle, Pa.

## *Announcements*

The Fund for Social Analysis is offering in 1959 a number of grants-in-aid for studies of problems posed by Marxist theory and its application. Projects for books and essays in all fields of social science will be welcomed. Grants will ordinarily range from $500 to

$3,000, and may be requested for an entire project, or for any part, or for assistance in research, editing or publication. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

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Section K—Social and Economic Sciences—of the American Association for the Advancement of Science will hold sessions for contributed papers at the annual meeting of the AAAS in Chicago, December 26-31, 1959. Association members interested in presenting a paper at these sessions should forward titles and abstracts, not later than September 20, to Donald P. Ray, Secretary, AAAS Section K, National Academy of Economics and Political Science, George Washington University, Washington 6. D.C.

## *Deaths*

W. C. Beatty, November 13, 1958.

Sidney D. Merlin, December 8, 1958.

Ragnar Nurkse, Columbia University, May 6, 1959.

A. C. Pigou, King's College, Cambridge, March 7, 1959.

John G. Rolph, November 2, 1958.

Clyde O. Ruggles, emeritus, Harvard Graduate School of Business, April 6, 1958.

James MacD. Terrell, November 1958.

Leonard S. Tyson, February 1958.

Julius V. Wyler, New School for Social Research, January 13, 1959.

## *Retirements*

Ruth A. Allen, University of Texas, June 1959.

Clyde O. Fisher, Wesleyan University, June 1959.

Reid S. Fulton, The City College of New York, June 1959.

H. L. McCracken, Louisiana State University, June 1959.

W. H. Steiner, Brooklyn College, June 1959.

Horace Taylor, Columbia University, June 1959.

Sam H. Thompson, Iowa State College, March 1959.

Kossuth M. Williamson, Wesleyan University, June 1959.

## *Promotions*

John E. Bishop: assistant professor of business administration, Harvard Graduate School of Business Administration.

Donald E. Cullen: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Francis W. Gathof: assistant professor of economics, The American University.

Charles J. Grayson, Jr.: assistant professor of business administration, Harvard Graduate School of Business Administration.

Neil E. Harlan: associate professor of business administration, Harvard Graduate School of Business Administration.

Werner Z. Hirsch: professor of economics, Washington University.

James E. Howell: associate professor of economics, Graduate School of Business, Stanford University.

Duncan M. MacIntyre: professor, New York State School of Industrial and Labor Relations, Cornell University.

Howard D. Marshall: associate professor of economics, Vassar College.

Lester B. McAllister, Jr.: professor of economics, Beloit College.

Simon Naidel: professor of economics and acting chairman, department of economics, The American University.

Alek A. Rozental: associate professor of economics, Saint Louis University.

Nathan Schmukler: associate professor of economics, Brooklyn College.

Irvin Sobel: professor of economics, Washington University.

Jack E. Thornton: Julian Price fellow in insurance, School of Business Administration, University of North Carolina.

Paul Wonnacott: assistant professor of economics, Columbia University.

### *Appointments*

Peter S. Albin, of Princeton University: instructor, New York University.

Curtis Aller, of Michigan State University: associate professor and chairman, department of economics, San Francisco State College, effective September 1959.

Claude A. Bitner, of Michigan State University: assistant professor, department of economics, Texas A & M College, effective September 1959.

Albert A. Blum: assistant professor, New York State School of Industrial and Labor Relations, Cornell University.

Emile Bouvier, of Georgetown University: president of the University of Sudbury, Sudbury, Ontario, January 1959.

Richard S. Bower, of Cornell University: assistant professor of economics and business administration, Vanderbilt University.

John A. Brittain, of Cornell University: assistant professor of economics, Vanderbilt University.

Philip W. Cartwright: associate dean of the College of Arts and Sciences, University of Washington.

J. K. Charles: lecturer in economics, McGill University, 1958-59 session.

Robert W. Clower: chairman of the department of economics, Northwestern University.

Robert P. Collier: acting dean, College of Business and Social Sciences, Utah State University.

Darwin W. Daicoff: instructor, University of Kansas.

Paul M. Dauten, Jr., University of Illinois: a director of Educational and Technical Consultants, Inc., Evanston, Ill.

Richard G. Davis: instructor in economics and research assistant, Princeton University.

Edgar O. Edwards: professor of economics, Rice Institute.

Jose Encarnacion, Jr.: instructor in economics, Princeton University.

Tibor Fabian: director, operations research, Lybrand, Ross Bros. and Montgomery, Philadelphia.

David Fand, of the University of North Carolina: associate professor of economics, Southern Methodist University.

James M. Folsom, of Vanderbilt University: instructor in economics, Duke University.

Paul Fox: research associate in business administration, Harvard Graduate School of Business Administration.

Richard B. Goode, of International Monetary Fund: senior staff member in charge of long-range program of research on economics of taxation, Brookings Institution.

Jack T. Guenther, of Harvard University: assistant professor of economics, Vanderbilt University, September 1959.

William Haber, of University of Michigan: elected president of Industrial Relations Research Association.

John Haldi: assistant professor of economics, Graduate School of Business, Stanford University.

George R. Hall: acting assistant professor of economics, University of Virginia, 1959-60.

Alvin H. Hansen, emeritus, Harvard University: visiting professor of economics, Haverford College, first semester 1959-60.

George W. Hardbeck, of Louisiana Polytechnic Institute; assistant professor of economics, Saint Louis University.

John D. Helmerger: lecturer in economics, University of Minnesota.

Benjamin H. Higgins, of Center for International Studies, Massachusetts Institute of Technology: professor of economics, University of Texas.

Leonid Hurwicz, of the University of Minnesota: visiting professor in economics, Stanford University, spring quarter 1959.

James James: lecturer in economics, Southern Methodist University.

Alexandre Kafka: acting associate professor of economics, University of Virginia.

Thomas F. Keller, of the University of Michigan: assistant professor, department of economics and business administration, Duke University.

Nathan Keyfitz, of Dominion Bureau of Statistics: professor in department of political economy, University of Toronto.

Charles Kretzschmar: economic statistician, Bureau of the Census, Washington, D.C.

Harold Kuhn, of Bryn Mawr: associate professor of mathematical economics, Princeton University.

Paul F. Lazarsfeld: Ford visiting professor, Harvard Graduate School of Business Administration.

Abba P. Lerner: professor, department of economics, Michigan State University.

Eugene M. Lerner: assistant professor, department of economics, The City College of New York.

Nelle P. Lewis: research associate, Bureau of Business Research, University of Kentucky.

Wallace Lovejoy, of Continental Oil Company: assistant professor of economics, Southern Methodist University.

David L. Lutin: member of staff of the Committee for Economic Development, Area Development Division.

George Macesich: assistant professor of economics, Florida State University, September 1959.

Allan B. Mandelstamm, of Northwestern University: assistant professor of economics, Vanderbilt University.

Theodore Mesmer: member of staff of the Research and Planning Division, U.N. Economic Commission for Asia and the Far East, Bangkok, Thailand.

C. Clyde Mitchell, of Harvard University Advisory Group to Government of Pakistan: chief of the FAO Mission in Colombia, March 1959.

Leon Moses: associate professor of economics, Northwestern University.

Richard F. Muth: associate professor of urban economics, School of Business, University of Chicago.

Herman L. Myers: program officer and economist, International Cooperation Mission in Mexico City.

I. D. Pal: lecturer in economics, McGill University, 1958-59.

James Pikl, Jr.: assistant professor of economics, Southern Methodist University.

C. Hock Quan, of Carthage College: research associate, Bureau of Business Research, University of Kentucky.

Robert F. Risley: acting dean of the New York State School of Industrial and Labor Relations, Cornell University.

Herbert R. Rouse: visiting consultant on statistics, Harvard Graduate School of Business Administration.

Warren Samuels, of Georgia State College: assistant professor of economics, University of Miami School of Business Administration.

Eric Schenker, of Michigan State University: appointment at University of Wisconsin, Milwaukee branch.

Charles L. Schultze, formerly senior staff Council of Economic Advisers; lecturer in economics at Indiana University.

Mark B. Schupack: instructor in economics, Brown University.

Richard T. Selden, of Vanderbilt University: associate professor of banking, Graduate School of Business, Columbia University, effective September 1959.

C. Joseph Sequin, of Michigan State University: assistant professor, department of business organization and management, Notre Dame University, effective September 1959.

Barbara A. Simpson: instructor in economics, College of William and Mary, effective September 1959.

Frank I. Stern, formerly Grey Advertising Agency, Inc.: now market analyst, New York, New Haven and Hartford Railroad.

George Sternlieb: research associate in business administration, Harvard Graduate School of Business Administration.

Walter F. Stettner, of International Cooperation Administration: senior economist, Council of Economic Advisers.

Albion G. Taylor, professor emeritus College of William and Mary: visiting professor, Hollins College 1959 spring semester.

Ronald L. Teigen, formerly St. Olaf College: instructor, University of Minnesota.

Carl E. Veazie: associated with Ebasco Services, Inc., in department of facilities, community, and industrial planning.

Barton Westerlund, of University of Arkansas: assistant director of research and senior industrial specialist at University's Industrial Research and Extension Center, Little Rock.

William V. Wilmot, Jr.: associate professor of management and head of the department of management and business law, University of Florida.

Tom Wise: lecturer in economics, McGill University, 1958-59.

### *Leaves for Special Appointments and Assignments*

Walter Adams, Michigan State University: visiting professor Salzburg, Austria, Seminar and visiting professor, Centre d'Etudes Industrielles, University of Geneva, spring 1959.

Richard C. Bernhard, University of California, Riverside: guest professor, University of Freiburg im Breisgau, May to July 1959.

Rudolph Blitz, Vanderbilt University: research appointment at Johns Hopkins University, spring semester 1960.

Henry Briefs, Georgetown University: second-year leave to continue in Council of Economic Advisers to the President.

M. P. Catherwood, New York State School of Industrial and Labor Relations: industrial commissioner, New York State Dept. of Labor, effective January 1959.

David C. Cole, Vanderbilt University: assigned to University of Philippines for year beginning February 1960 under Vanderbilt University's Overseas Professorship Program.

Harold W. Davey, Iowa State College: visiting professor in industrial relations, University of Minnesota, spring quarter 1959.

Corwin D. Edwards, University of Chicago: member staff Brookings Institution to undertake study on International Comparative Analysis of Trade Regulation, year 1959-60.

Frederick C. Joerg, Duke University: Fulbright lecturer Helsinki School of Economics, Finland, 1959-60.

John P. Lewis, University of Indiana: member staff Brookings Institution to undertake study on The United States and Indian Economic Development 1959-60.

Richard W. Lindholm, University of Oregon: in Korea as economic consultant to ICA with reference to establishment of an economic development council in Korea, early 1959.

Lawrence C. Lockley, University of Southern California: visiting professor of marketing, Graduate School of Business, Columbia University 1959-60; has resigned deanship of School of Commerce, University of Southern California.

Fritz Machlup, Johns Hopkins University: lecturer at the Universities of Cologne, Bonn, Mainz, Frankfurt, Munich, Freiburg and Saarbrücken, January-February 1959.

Harry McAllister, State College of Washington: research associate with National Bureau of Economic Research.

James W. McKie, Vanderbilt University: visiting associate professor of economics, Harvard University, 1959-60.

David N. Milstein, Rutgers—The State University: on research project in the Fiscal and Financial Branch, United Nations Secretariat, past year.

Carl L. Nelson, University of Minnesota: visiting professor, School of Economics in Turku, Finland, and Fulbright fellow, 1959-60.

Henry H. Schloss, Washington University: Fulbright lectureship at the University of Bombay.

John Sheahan, Williams College: Brookings Institution National Research Professorship for research in France, 1959-60.

Jack Stieber, Michigan State University: guest professor in department of industrial administration, Royal College of Science and Technology, Glasgow University, Scotland.

Charles J. Stokes, Atlantic Union College: Fulbright professor of economics at the University of Guayaquil and the Central University of Ecuador, 1958-59.

Philip Taft, Brown University: visiting professor, University of California at Berkeley, second semester 1958-59.

Anthony M. Tang, Vanderbilt University: assigned to the Institute of Social and Economic Research, Osaka University, Japan 1959-60 under Vanderbilt University's Overseas Professorship Program.

Gerald E. Thompson, University of Nebraska: appointed by Ford Foundation fellow of Institute of Basic Mathematics for Application to Business to be held at Harvard and Massachusetts Institute of Technology, 1959-60.

Gerhard Tintner, Iowa State College: taught econometries during spring quarter at Technical University, Lisbon, Portugal, at request of U.S. Department of State.

Walter H. Zukowski, Colby College: visiting professor of business administration, Al-Hirma University of Baghdad, Iraq, under Rockefeller Foundation grant for year 1958-59.

## *Resignations*

Frank T. Bachmura: Vanderbilt University.

John M. Baitsell: Harvard Graduate School of Business Administration.

Manuel Gottlieb: University of Kansas.

William A. Kamins: Harvard Graduate School of Business Administration.

Richard W. Kazmaier, Jr.: Harvard Graduate School of Business Administration.

Robert A. Solo: The City College of New York.

Maxine Woolston: Bryn Mawr College.

## VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the Review will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

### *Vacancies*

*Economics and business administration:* A Christian liberal arts college will have a vacancy September, 1959, for someone to handle courses in economics, business organization and finances, salesmanship, to be followed second semester by economics, marketing, and business management. Desire Ph.D. but master's degree with teaching experience will be considered. Salary and rank are open, depending upon training and experience. Write Chairman, Department of Economics and Business Administration, Geneva College, Beaver Falls, Pennsylvania.

*Marketing, finance, and management:* Unusual opportunities for competent teachers able to work with business groups. Two openings, from assistant to full professor. School of business administration in Washington, D.C. P214

*Business law and accounting:* School of business administration in the nation's capital has opening for instructor or assistant professor, LL.B. and accounting B.S. required minimum. For 1959-60 academic year. P218

*Business law:* Opportunity for assistant or associate professor of business law. Qualifications in business-government relationships highly desirable. School of Business Administration in nation's capital. P219

*City planning agency:* Associate research planner position open to economist with master's or bachelor's degree and a year of experience in capital programming and planning analysis (economic and population data). Starting salary $6,590 with annual increments of $200 plus cost-of-living bonus. Write to Arthur J. Reed, Commissioner of Planning, Department of City Planning, 211 East Water Street, Syracuse 2, New York.

*Accounting:* Senior-level position at midwest university. Desired qualifications are: doctoral degree, C.P.A. certificate, teaching experience at the university level, and publications. Salary and rank commensurate with qualifications. P220

### *Economists Available for Positions*

*International economics and international relations, economic policy and government in economics, corporation finance (American and international), public finance, economic history, economic geography, comparative systems, regional economics of Europe, Middle East, Latin America:* Man, in fifties; U.S. citizen; European professorial qualification; doctoral degrees in economics, finance, and law. Head of a department, full and visiting professor on four continents, including a leading university; 5 years of teaching at a U.S. university; research director; international bank-

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# DIFFUSION, ACCELERATION, AND BUSINESS CYCLES

*By* BERT G. HICKMAN*

The acceleration principle is a key element in many modern aggregative models of the business cycle. Coupled with the multiplier, it can be made to yield a series of self-generating cycles, provided that the phenomenon of excess capacity is ignored, as in Paul Samuelson's classic model [12]. Even when discarded as the usual cause of the upturn, as it was by J. R. Hicks [6], the accelerator remains the crucial factor on the downturn, because it is used to explain an endogenous lead of investment over national output. The principle derives its peculiar power and its attraction, in other words, from the proposition that a mere retardation in the rate of increase of real national income during a cyclical expansion may cause an absolute decline of investment and hence of production.

I propose to show that it is unnecessary to invoke the accelerator to explain how a downturn of aggregate investment may occur when aggregate output retards (decelerates), that it is substantially incorrect to do so, and that more than token weight must be given to other determinants of investment decisions which acceleration theorists themselves recognize are of some, though not "fundamental" importance. The negative aspect of my argument consists principally of the demonstration that fixed investment in the bulk of manufacturing industries is more nearly a function of the level of output than its rate of change, together with tentative evidence that accelerator-induced inventory investment may play a minor role in business downturns. The positive contribution is to emphasize the fact that a business expansion becomes less widely diffused over the economy as aggregate activity retards, and to show how the individual production *declines*

* The author is a member of the senior research staff of the Brookings Institution. His thanks are due to Moses Abramovitz and W. H. White for valuable comments and suggestions.

which regularly accompany aggregative *retardation* may induce a downturn of aggregate investment by affecting the capital outlays of the declining industries and by weakening investment incentives generally.

According to the acceleration principle, net investment for purposes of capacity expansion is a function of the rate of change of output. This follows from the assumption of a technologically determined, fixed ratio of capital to output in the individual firm. If output is constant at an economically efficient level of capacity utilization, no increase of capacity is called for. If output rises and the increase is thought to be permanent, it will pay to augment capacity in proportion to the *change* in output, so that net investment will rise by an amount determined by the output change. If output subsequently rises more slowly, net investment will decline, since the required increment to capacity will have diminished.

Now, no one contends that investment in new capacity invariably behaves in the foregoing manner. For one thing, the principle does not come into play until output has increased enough from a cyclical trough to eliminate the excess capacity which had developed during the preceding contraction—which means that it cannot be used to explain the upturn itself.[1] Acceleration theorists recognize, moreover, that a current rise of output may not always be regarded as permanent, that not all of the deficiency of capacity need (or can) be made up quickly, and that the cost and availability of capital funds may enter the investment decision. (Indeed, these factors are often represented explicitly in modified accelerator models.) Nonetheless, they are inclined to treat such considerations as secondary qualifications to the "fundamental" technological relationship expressed in the acceleration hypothesis. But are the qualifications sufficiently secondary so that the predictions of the original formulation remain valid?

Few economists have denied that expansions and contractions of output will induce corresponding fluctuations of investment—including replacement expenditure and "autonomous" investment as well as expenditure for capacity expansion—since current profits rise and fall with production and affect both expected returns from investment and the internal supply of capital funds. Moreover, if production presses on capacity and the condition is expected to continue, capacity expansion is indicated—unless near-term expectations suggest that a postponement would be profitable. In much the same way, when

[1] This statement would have to be modified in the extreme case where gross investment dropped to zero and output remained at a floor long enough to permit the gradual elimination of excess capacity and a subsequent rise of replacement demand. This is not regarded as a realistic situation even by acceleration theorists, however. Hicks, for example, assumes that the recovery is started by an uptrend of autonomous investment [6, pp. 101-5].

production and profits are falling, the appearance of surplus capacity is an additional, and powerful, incentive to defer investment. These capacity effects are only part of the story, however, and certainly need not be controlling, nor do they require investment to fluctuate in proportion to the rate of change of production.

These considerations suggest the testable hypothesis that for individual industries and periods as short as business cycles, fixed investment is more nearly a function of level of output than its rate of change.[2] But if this is correct, investment will usually lag production or synchronize with it, and this means that the acceleration hypothesis will be either inapplicable or invalid in most instances. That is, either output will not have retarded before falling, or if it did, the retardation will not have caused investment to lead output—and it matters little which, if either situation occurs commonly enough. Cases where investment led production would be consistent with the acceleration hypothesis, provided output retarded prior to the investment decline. Such cases could also result from other factors, however, and additional evidence would be necessary to select among the alternatives in such instances.

But all this has to do with the individual industry. Paradoxically enough, it is conceivable that a mere retardation of aggregate output could result in a decline of aggregate investment even though investment was a function of the level instead of the rate of change of output in every single industry.[3] This is because there is a strong correlation between the industrial coverage of a business expansion, as measured by a "diffusion index" which shows the ratio of expanding industries to all industries, and the rate of change of aggregate production.[4] When aggregative retardation occurs, it is not exclusively or even primarily because all individual outputs are increasing more slowly—the image which is probably at the back of the mind when the accelerator is applied to aggregate output—rather, it is principally because fewer industries are expanding at all and more have begun to decline.[5] But

[2] This is not to deny that the assumption of a comparatively fixed ratio between real capital and output is useful in the longer-run analysis of investment in individual industries. See below, fn. 23.

[3] This case is a logical extreme, involving unnecessarily strong assumptions. My general thesis that the relation between diffusion and aggregate activity is a key element in business cycles does not hinge on this one possibility.

[4] A. F. Burns [1] and G. H. Moore [10], working along lines pioneered by Wesley Mitchell, developed diffusion indexes and demonstrated their major cyclical properties in 1950.

[5] See Hickman [4], especially Chart 48, which shows that variations in the rate of change of aggregate manufacturing employment are due more to variations in the proportion of expanding industries than to changes in the average rate of expansion per industry, although both influences are involved.

individual production declines will induce individual investment declines, and under certain conditions to be examined later, the individual declines could cause aggregate investment to lead aggregate output. Persons with a taste for "as if" theorizing and high levels of aggregation, could describe this situation by a model in which aggregate investment was a function of the rate of change of aggregate output. It would be incorrect, however, to justify the assumed relationship by appeal to a technologically determined capital-output ratio, or to argue that it reflected accelerator-induced investment reductions in individual firms. To do so would be to mistake what was basically a diffusion phenomenon for another sort of behavior with a different rationale.

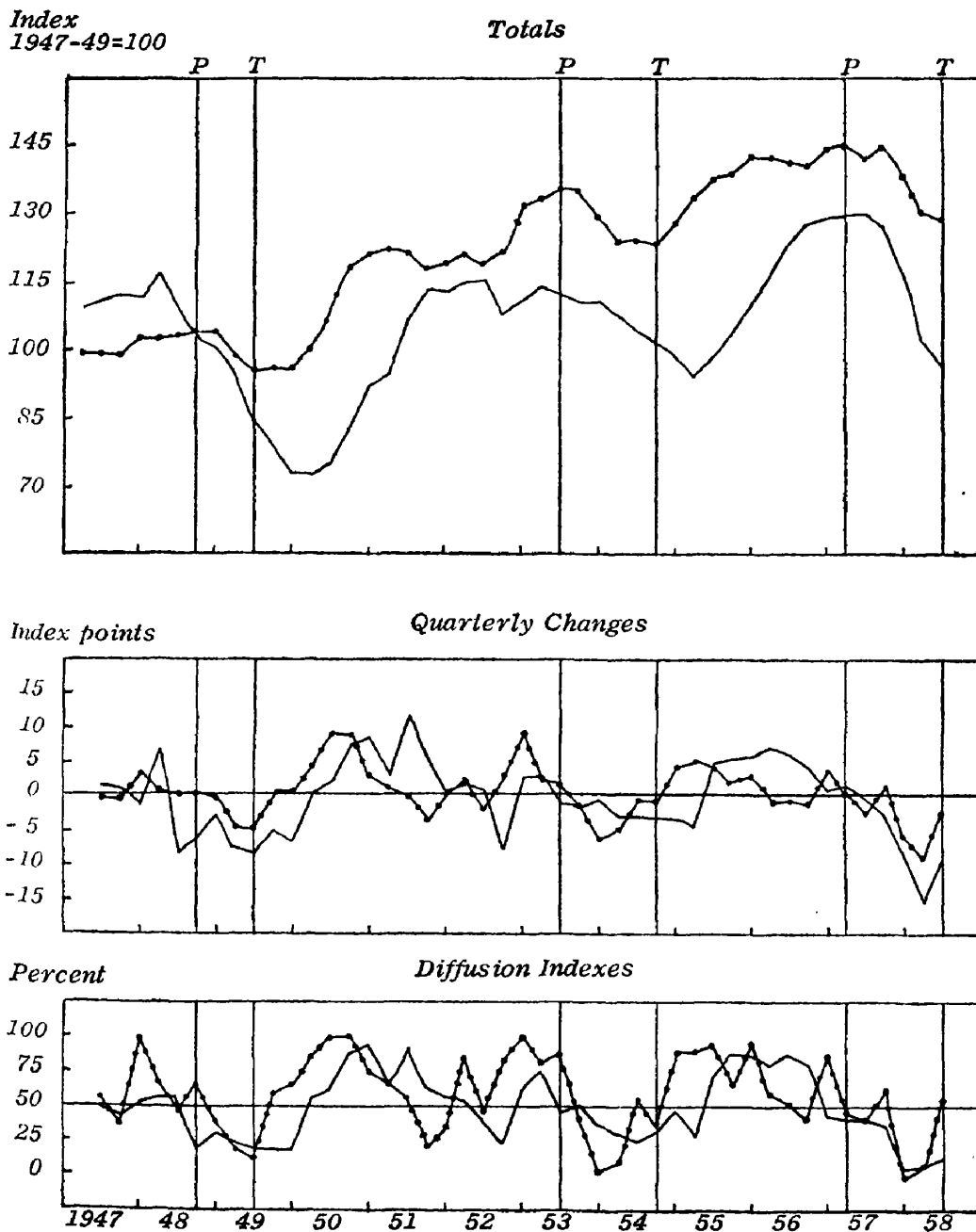
There are three main divisions to the paper. We begin with an empirical examination of industrial (manufacturing and mining) investment in plant and equipment since the second world war. The purpose is twofold: to test whether fixed investment does or does not lead production in the separate industries and the aggregate, and to investigate the conditions under which aggregate investment would lead aggregate output even if investment in each industry were a coincident or lagged function of its own output and of nothing else. The second section is concerned with the relationship of sales diffusion on the retail level to production diffusion in manufacturing, and more generally, with the relative importance of diffusion and acceleration as causes of turning points in inventory investment and business cycles. The paper closes with a qualitative analysis of the relation between diffusion and aggregate activity and of its implications for investment and cycle theory.

## I. *Industrial Production and Investment, 1947-1958*

Quarterly indexes of total industrial production and real gross expenditure for industrial plant and equipment are graphed for the period 1947-58 in the upper panel of Chart 1.[6] The fact that capital formation is measured gross is no handicap in the present connection. Although the acceleration principle deals with *one* variety only of *net* investment

[6] The investment figures were derived from the SEC-OBE estimates of capital expenditure by adjusting the raw data for price changes and seasonal variations. Details are given in the notes to Table 1. Deflation procedures are necessarily crude and may affect the timing comparisons between production and investment. The deflated indexes used here are less favorable to my hypothesis than the current dollar estimates, since rising prices will, if anything, make a deflated series turn down earlier than its undeflated counterpart. With regard to the downturn of 1957, for example, timing comparisons based on the undeflated investment figures would reduce the number of individual investment leads from 3 to 0, and would increase the length of the investment lags in 3 other industries. Deflation did not alter any turning points during the Korean period, however, nor did it affect the general consensus of investment leads during 1947-48.

CHART 1.—INDUSTRIAL PRODUCTION AND INDUSTRIAL FIXED INVESTMENT, TOTALS, QUARTERLY CHANGES, AND DIFFUSION INDEXES, SEASONALLY ADJUSTED, QUARTERLY, 1947-1958

*(Production•—•; Investment——)*

*Vertical lines indicate peaks and troughs of industrial production.*

*For sources and notes see Table 1.*

—that needed to increase the capacity to produce existing products—it would cease to be interesting if it consistently failed to explain observed downturns of gross investment merely because other categories continued to increase.

The vertical lines on the chart show the dates of the quarterly peaks and troughs of industrial production associated with the business cycles of the period. It will be seen that there is a one-to-one correspondence between the cycles in production and investment, but that the turns do not in general coincide. Investment regularly lags production on the upturns. With regard to the peaks, investment leads by two quarters in 1948 and one in 1953, but turns simultaneously with output in 1957.[7]

The investment lags on the upturn need cause no concern, for everyone agrees that output must usually rise before improved profits and increased rates of utilization become positive spurs to fixed investment over the broad range of commodity-producing industries. The downturns are another matter. The prima-facie case for the acceleration hypothesis is strongest in 1948, when the lead of investment was both long in duration and substantial in amplitude. Production, moreover, did retard one quarter before the peak of investment (see the absolute quarterly changes of production shown in the middle panel of the chart).

One must remember, however, that the early postwar years were abnormal and turbulent. The investment data for 1946 are unavailable, but they would show a swift rise as manufacturers reconverted to peacetime production and undertook programs of modernization and expansion to meet postwar levels of demand. Put a bit differently, the first postwar expansion began from a position of *deficient* capacity. From the engineering standpoint, a decline of investment was indicated once satisfactory postwar levels of capacity had been attained, even if production had continued to rise as rapidly as before. It appears likely that this stage of satisfactory capacity had been attained in most manufacturing lines by the end of 1947, after two years of intense investment activity and partly because, it is true, output rose more slowly in 1947 than in 1946. When to this supposition is added the continuing uncertainty as to whether the postwar inflation could be liquidated without a severe depression, it is easy to account for the decline of investment during 1948.

All this is quite consistent with modified versions of the acceleration

[7] These timing comparisons are based on small differences at two of the downturns. Production was nearly as high in the fourth quarter of 1948 as in the third, so that its peak might be considered to lag investment by 3 quarters instead of 2. Similarly, in 1957 the level of investment declined only fractionally from the first to the second quarter, so that it could be considered to lag production by one quarter.

principle, of course, for these are usually flexible enough to handle situations of initial disequilibrium and inelastic expectations. It does seem plausible, however, that these last determinants of investment were at least as important as the rate of change of production during this first postwar expansion.

The lead of investment was notably briefer and milder in 1953 than in 1948. It may be objected that the peak of investment should be dated in the second quarter of 1952 instead of the first quarter of 1953, but in that case we not only have investment leading production, but leading it during a period of accelerated production increase. Actually, neither aggregate production nor its rate of change was closely linked to total investment during this period, because the expansion was dominated by the Korean war and mobilization from mid-1950 to mid-1952, but was capped thereafter by a boom in civilian goods. The corresponding shifts of output composition and their consequences for investment will be discussed presently.

Finally, no lead of investment whatever appears in 1957, despite the fact that aggregate production was retarded after 1955 and actually decreased during the first three quarters of 1956, before rising in a last spurt to its ultimate peak.

None of the foregoing is conclusive. Because of its technological rationale, the acceleration principle must hold for individual industries, if it holds at all, so that final judgment on it must await examination of the industry detail. Before we take that step, however, it will be convenient to view the behavior of industrial production and investment in the light of the correlation between aggregative acceleration and industrial diffusion. Attention is therefore directed to the bottom panel of Chart 1, which contains diffusion indexes of investment and production corresponding to the aggregates already studied. Industrial production, for example, was broken down into 16 component industries, and the percentage of them experiencing increases of production in each quarter was then computed. The diffusion index of investment expenditures was similarly prepared for the same set of industries.

A comparison of the diffusion index of production with aggregate production will quickly demonstrate that the former leads the latter. Each decline of aggregate production is preceded by a period during which the proportion of expanding industries declines, until finally the aggregate turns down, usually when production is falling in more than half the industries. Not only that—and here is visual proof of the point stressed earlier—but variations in the rate of change of aggregate production are closely correlated with variations in the percentage of expanding industries, as may be seen from the middle and lower panels.

Exactly the same observations may be made with regard to the relationships among the three investment curves.

Of greater interest at the moment, however, is the relationship between production diffusion and investment diffusion. Investment diffusion usually lags the peaks and troughs of production diffusion by one or two quarters. If investment in each industry were a lagged function of output in that industry and of nothing else, we would observe just such a correlation between the diffusion indexes. As aggregate output retarded, and production declined in more industries, so too, with a lag, would investment in more industries.

Assume furthermore that each industry was the same size, as measured by value added, and that the individual production cycles were identical except for the time-phasing.[8] This would assure a perfect correlation between the percentage of expanding industries and the rate of change of aggregate production. Suppose also that investment per unit of output and the time-lag of investment were identical for each industry.[9] This would assure a perfect correlation between investment diffusion and the rate of change of aggregate investment. Taken altogether, these assumptions would mean that each of the investment curves in the three panels of the chart should be a lagged replica of the corresponding production curve.

Since the actual correlations approach but do not equal the hypothetical ones—and in particular, since total investment sometimes leads total production—we are led to ask what factors might account for the departures. First, investment in each industry may not be simply a lagged function of its output. This important question aside for the moment, there may be imbalances among the industries with regard to size or amplitude of production. Such imbalances do exist even on the average,[10] but they are comparatively minor, and in any event are unimportant for our problem. Production imbalances would merely make aggregate production turn down before (or after) the proportion of declining industries exceeded one-half.[11] Aggregate investment would then also

[8] Or, more realistically, that the average size and amplitude of the industries included in the expanding and contracting groups remained constant even as the size and composition of the groups varied over time.

[9] Or that the average investment-output relationships were the same for the expanding and contracting groups.

[10] The average size of the industries in the expanding and contracting groups remains fairly constant as industries shift from one group to the other, but there is a positive correlation between the number of rising (falling) industries and the average amplitude per rising (falling) industry [4]. This was demonstrated for employment in the paper cited, but it is also true of production.

[11] Suppose the early declines occurred among larger-than-average industries or were of more-than-average amplitude. Total production would then fall while the contracting industries were still a minority.

decline before (or after) investment was falling in half the industries, but it would still lag aggregate production.

Finally, it might be the case that one or more industries had a larger (or smaller) influence on investment than on output, either because the capital-intensities or the time-lags differed markedly from the industrial averages. In particular, if the early decliners invested heavily per unit of output, and with short or average lags, aggregate investment could lead aggregate output even though this was not true of any single industry. Since aggregate production seldom declines until half the industries are contracting, such an instance of disproportionate influence on investment would usually be reflected in a downturn of total investment before investment was falling in half the industries. This is not the explanation of the investment lead on the downturn of 1953, however, for the proportion of industries in which investment was declining fell below 50 per cent in the same quarter that aggregate investment dropped. (What actually happened was that investment led output in several industries.) And, as far as 1957 is concerned, investment inequalities *delayed* the downturn of aggregate investment, since the proportion of industries experiencing investment declines fell below 50 per cent two quarters before the rate of change of investment became negative. We will return to these general considerations after an examination of the cycles of production and investment in the component industries.

Chart 2 reveals substantial variation from industry to industry with regard to production, investment, and the relationship of the two: so much as to disabuse one of the notion that investment is a simple, stable function of either output or its change. Despite these differences, however, a common cyclical pattern may be detected among the production indexes. The variety of investment behavior is more pronounced and no attempt will be made to account for it fully. It will be possible, nevertheless, to reach some firm conclusions on the issue of whether as a rule investment does or does not lead production in the several industries.

The timing of the peaks and troughs of production and investment in each industry is compared with those of aggregate industrial production in Table 1. The average lead or lag at each turning point is recorded at the bottom of each column. With regard to the general peak of 1948, for example, it will be seen that the individual investment series led total production by an average of 3.5 quarters, whereas the several production indexes showed an average lead of .5 quarter. More relevant for us is the comparison between the peaks of production and investment in each industry, which shows that investment lagged in only 2 of 16 industries in 1947-48. The abnormal character of that expansion and

| Industry | Peak (III 48) | | Trough (II 49) | | Peak (II 53) | | Trough (III 54) | | Peak (I 57) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Prod. | Invest. | Prod. | Invest. | Prod. | Invest. | Prod. | Invest. | Prod. | Invest. |
| Primary iron and steel | +2 | 0 | +2 | +2 | 0 | −4 | 0 | +1 | −4 | +2 |
| Primary nonferrous metals | 0 | −5 | 0 | +5 | 0 | −3 | −2 | +3 | −5 | +2 |
| Electrical machinery | −3 | −5 | 0 | +3 | +1 | +2 | −1 | +3 | −1 | −2 |
| Nonelectrical machinery | −1 | −6[a] | +2 | +2 | 0 | 0 | +1 | +2 | −1 | +3 |
| Motor vehicles and equip. | none | −6[a] | none | +3 | 0[c] | +4 | 0 | +3 | −5 | −2 |
| Other transportation equip. | +2 | −1 | +3 | +3 | +2 | −5[d] | +1 | −3 | +1 | +1 |
| Stone, clay & glass prod. | 0 | −4 | 0 | +1 | +1 | −7[d] | −2 | 0 | −3 | −2 |
| Other durable goods | 0 | −5 | 0 | +2 | +1 | 0 | 0 | −1 | −5 | −2 |
| Textile mill products | −1 | 0 | 0 | +2 | 0[c] | −8 | −2 | +2 | −4 | −3 |
| Rubber products | −6[b] | −6[a] | +1 | +2 | 0 | 0[c] | 0 | 0 | −5 | −1 |
| Paper and allied prod. | 0 | −6[a] | 0 | +2 | +1[c] | +3 | −2 | +2 | −2 | −1 |
| Chemicals & allied prod. | +1 | −6[a] | 0 | +2 | 0 | 0 | −2 | +3 | +2 | +2 |
| Petroleum & coal prod. | +1 | −2 | +2 | +4 | +1 | +2[c] | 0 | +2 | 0 | 0 |
| Food and beverages | none | 0 | none | +3 | +1 | 0[c] | 0 | +4 | none | −2 |
| Other nondurable goods | −2 | −5 | +2 | +4 | 0 | +4[c] | 0 | +2 | +1 | −2 |
| Mining | 0 | +1 | +1 | +5 | +1 | +2[c] | 0 | +2 | 0 | 0 |
| Average | −0.5 | −3.5 | +0.9 | +2.8 | +0.6 | −0.7 | −0.6 | +1.6 | −2.0 | −0.5 |

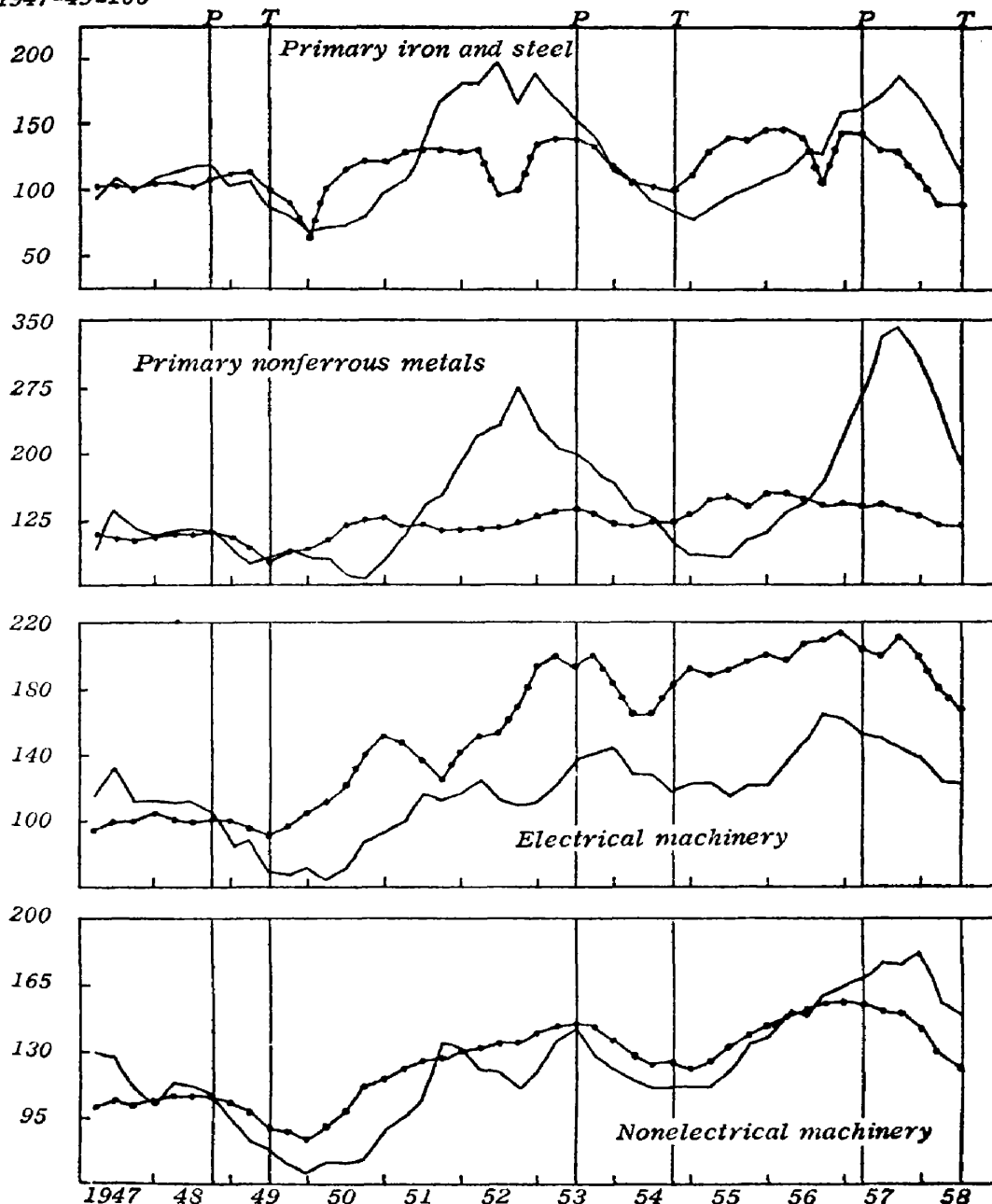
[a] Lead at least this long, although no investment data available for 1946 to check for earlier leads.

[b] Although data prior to 1947 are not included in Chart 2, inspection of the 1946 figures shows that the first quarter of 1947 was the peak in rubber production.

[c] During the Korean expansion, two peaks are often found for production and/or investment in a given industry. Unless the later of the two is markedly lower than the first, it is chosen for comparison with the general peak of II Q 1953.

[d] "Local" peak lags industrial production by one quarter—see Chart 2.

*Source:* Indexes of production from Federal Reserve Board. Expenditures on new plant and equipment in current dollars from U. S. Department of Commerce, Office of Business Economics, and Securities and Exchange Commission, adjusted for seasonal variation and corrected for price change by the author. The price deflator was a weighted average of an index of wholesale prices of producer goods for manufacturing industries (Bureau of Labor Statistics, weight 62 per cent) and an index of construction costs of commercial and factory buildings (E. H. Boeckh and Associates, weight 38 per cent). The weights reflect the relative importance of structures and equipment in manufacturing establishments in 1947, as given in *Survey of Current Business*, Nov. 1956. p. 9, Table 1. The index of industrial investment shown in Chart 1 is the sum of the individual series as separately adjusted and deflated.

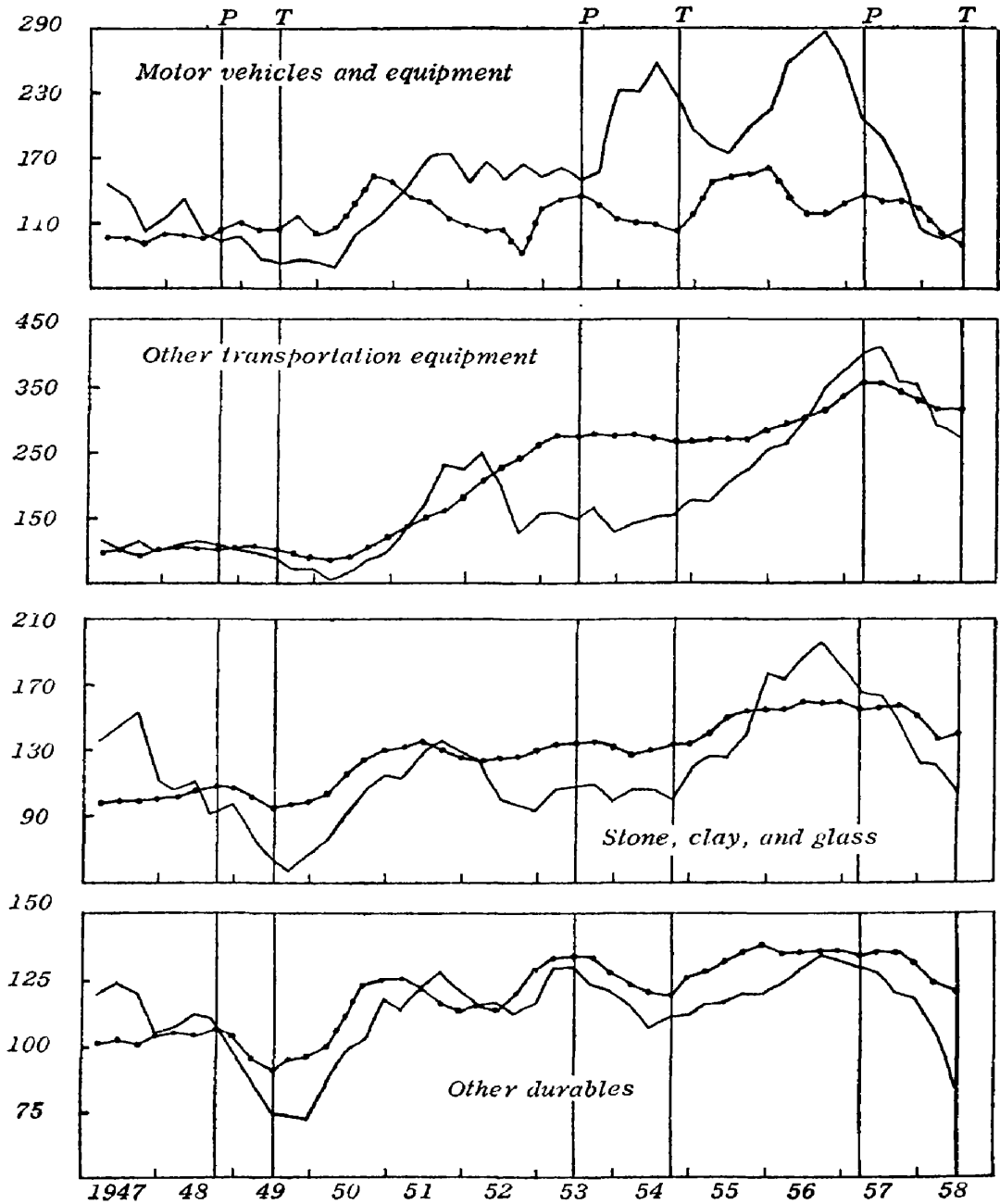
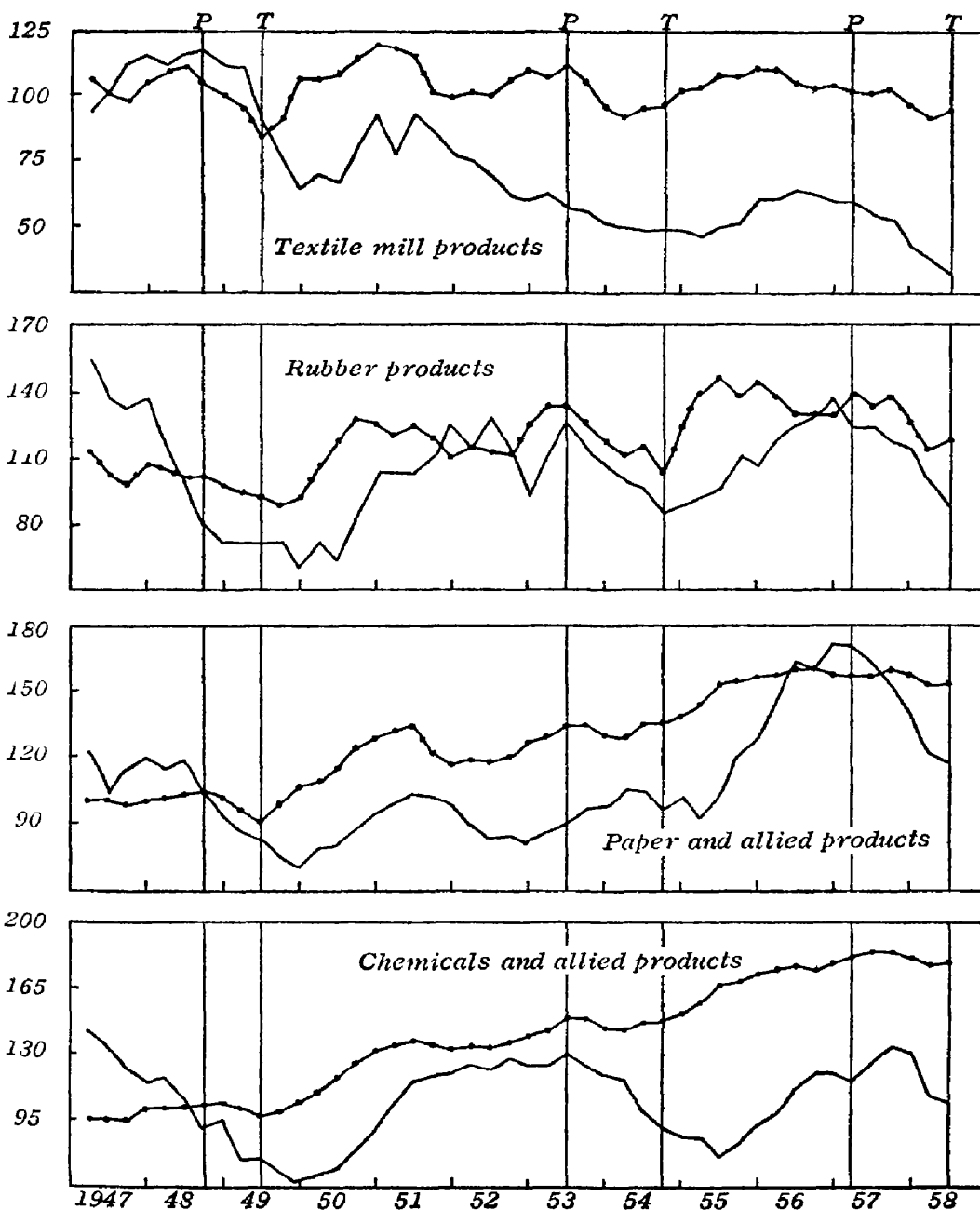
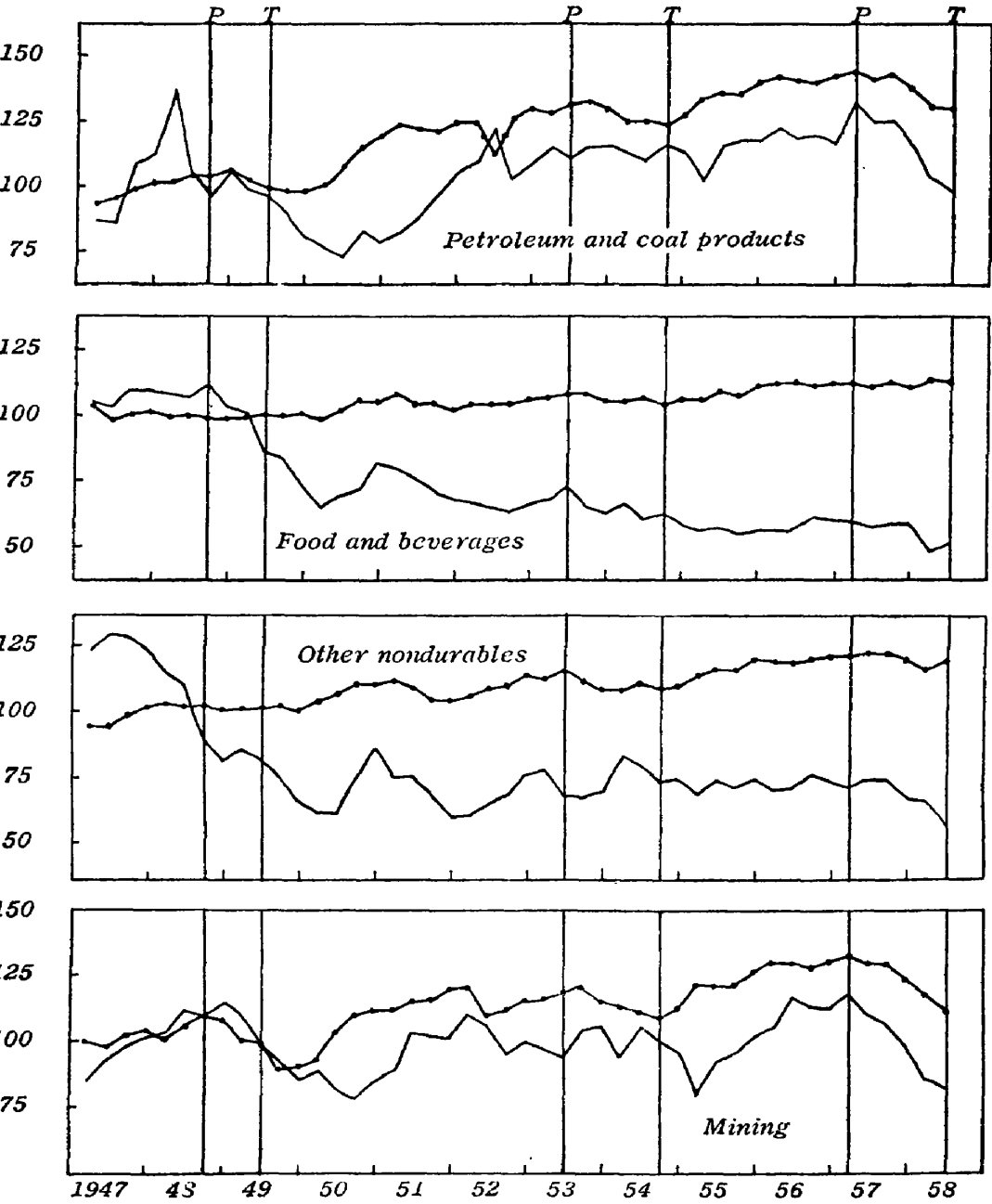
CHART 2.—PRODUCTION AND FIXED INVESTMENT IN SIXTEEN INDUSTRIES, SEASONALLY ADJUSTED, QUARTERLY, 1947-1958

*(Production—•—; Investment——)*

*Vertical lines indicate peaks and troughs of industrial production.*

*For sources and notes see Table 1.*

CHART 2 (*Continued*)

CHART 2 (*Continued*)

CHART 2 (*Continued*)

its implications for investment behavior were discussed earlier. For the same reason, there is no need to dwell on the timing relationships at the troughs.

Several peculiarities of the Korean expansion must be kept in mind for a correct reading of that period. The recovery which began in 1949 was accelerated by the outbreak of hostilities in June 1950, and from then until early 1951 most industries participated in a phase of intensive, heavily speculative, expansion. This phase gave way to a lull in civilian goods production which lasted until about mid-1952, whereas defense production rose all the while. During this interval also, fixed investment was subject to a variety of selective controls and incentives. Controls were relaxed thereafter and civilian production boomed during the last four or five months of 1952 and the first half of 1953.

Most of the individual industries had two humps in production corresponding to the first and last phases just described. These humps also appear in the total production index (Chart 1). I have not included the first of them in the timing comparisons of Table 1, but if that were done, investment would lag output in the overwhelming majority of industries which participated in the lull (Chart 2). With regard to the peak that actually terminated the expansion in 1953, we find investment lagging production in 6 industries and turning simultaneously in 3 more. It led, however, in the 7 remaining cases. As we know, aggregate investment also led aggregate production by one quarter. Why was this so, when slightly less than half the industries were investment leaders?

The wide fluctuations and early declines of investment in iron and steel, nonferrous metals and transportation equipment reflect the fact that these industries were strongly influenced by the Defense Facilities Expansion Program of the Korean war.[12] Investment was clearly not a function either of current output or its change in these industries; rather, its course was determined by the timetable of the mobilization. If these 3 are excluded from the total, the aggregate investment of the remaining 13 industries lags their aggregate production, and the proportion of investment leaders shrinks to a minority. It is worth noting also that the failure of textile investment to recover along with production in 1952-53 is probably attributable to the fact that the production revival did not carry to the 1950 peak and therefore did not move into the upper range of capacity utilization. Finally, the boomlet of late 1952 and early 1953 did induce an increase of investment in stone,

[12] Projects completed under Certificates of Necessity from 1951 through September 30, 1954 [13] accounted for half or more of the investment by these three industries during those years. The proportion was also substantial—about 45 per cent—in paper and chemicals, but was 25 per cent or less in the other groupings included in our breakdown.

clay and glass, but one I hesitated to date because its peak was far below 1951. If the 1953 high were chosen as a local peak, it would be simultaneous with production.

I conclude that the slight lead of industrial investment over industrial production in 1953 was due to autonomous factors rather than output retardation, and that for the bulk of industries in which autonomous influences were comparatively unimportant, investment was more nearly a function of output than its change.

There is little ambiguity with respect to the expansion culminating in 1957. On the average, production led investment by 1.5 quarters. Investment lagged in 9 industries and was simultaneous with production in 4 others. Production peaks began to appear as early as the fourth quarter of 1955, but these first declines were concentrated in industries with long investment lags—the primary metals, motor vehicles and rubber products—so that investment did not weaken on their account for several quarters. Nonelectrical machinery joined this group somewhat later and with similar results. In brief, the contraction of industrial fixed investment was delayed by these lags and might otherwise have caused an earlier downturn of business activity.

In this most recent of business expansions, then, which is the only one since the second world war largely free of war influences, investment did not lead output, except in a small minority of industries, despite the fact that production retarded prior to its peak in most instances (Chart 2); nor did aggregate industrial investment lead industrial production. The same may be said of the Korean expansion, if three defense-dominated industries are excluded from the manufacturing sector. These findings contradict the chief prediction of the acceleration hypothesis and lead us to reject it as an explanation of the investment downturns in most industries, and hence of the corresponding downturns of industrial fixed investment, during these expansions.

This conclusion could be questioned on the ground that it is the investment *decision* which is a function of the rate of change of production, and that the tendency for investment expenditure to lag output merely reflects the lag of expenditures behind decisions. It was possible to make a direct test of this hypothesis for the period 1955-58 with the assistance of the capital appropriations survey of the National Industrial Conference Board. Quarterly reports on appropriations outstanding, newly approved, committed or spent, and canceled, are available for 500 large manufacturing corporations and by major industries [3]. Since appropriations provide the budgetary authority to go ahead with investment projects, they are an ideal index of investment decisions for the present purpose. They measure decisions at a point where they are comparatively firm, just one stage before contracts are let or orders placed.

The turning points of new appropriations and of appropriation backlogs for these large corporations were compared with the corresponding Federal Reserve indexes of production. For all 500 companies, new approvals reached a peak in the second quarter of 1956, to be followed by appropriation backlogs (end of quarter) in the first quarter of 1957 and by capital expenditures in the third.[13] Thus new approvals led the index of manufacturing production, whereas appropriation backlogs synchronized with it. Evidently it is important to decide which concept of appropriations is the best measure of planned investment. Reflection indicates that the logical choice is the backlog of unexpired appropriations. This is because it is the relation of new approvals to expiring appropriations, rather than the trend of new approvals, which counts for future expenditures. In the early months of an investment boom, new approvals run far ahead of current commitments or expenditures, and the backlog mounts. Later, when new approvals begin to decline, it is not necessarily because firms wish to reduce capital outlays: they may merely be curbing the rate of increase of projected expenditures. As long as new approvals (net of cancellations) exceed current commitments or expenditures, the backlog of uncommitted or unspent appropriations will continue to rise, and this means that the amount of planned (currently budgeted) investment is still increasing. As a corollary, the pattern of new expenditures resembles that of backlogs instead of new approvals.

Comparable data on production and appropriations are available for 10 of the industries we have examined.[14] New appropriations led production by from one to 4 quarters in 4 industries, were coincident in 3, and lagged one or 2 quarters in the 3 remaining cases. Appropriations backlogs led in only two industries (by one and 2 quarters), were coincident in 4, and lagged in 4, by intervals of two to 4 quarters. Thus investment decisions as well as expenditures tend to lag or synchronize with output.

## II. *Role of Inventory Investment in Business Downturns*

The technological rationale of the acceleration principle is weaker for inventory than for plant investment, but perhaps the economic compulsion to maintain a fairly rigid ratio of capital to output is stronger in the case of stocks; and in any event, a constant marginal relationship between sales changes and inventory investment is suffi-

[13] When measured in current dollars, the peak in the SEC-OBE estimate of new plant and equipment expenditure by manufacturers also occurs in the third quarter of 1957.

[14] Primary iron and steel; primary nonferrous metals; electrical machinery; nonelectrical machinery; stone, clay and glass; petroleum and coal products; textile mill products; rubber products; paper and allied products; and chemicals and allied products. I adjusted the appropriations data for seasonal variation. However, comparison of unadjusted production and appropriations yields the same timing results.

cient to cause inventory cycles [9] [11]. Inventory investment (net change in inventories), moreover, is intrinsically short-run and quickly adaptable to changing conditions of production and sale. It is quite possible, therefore, that accelerator-induced swings of inventory investment may cause business downturns.

Thus far we have studied retardation and diffusion only on the level of industrial production. Now it is conceivable that production diffusion is due to acceleration effects stemming from retardations or accelerations of final demand. Suppose it were true, for example, that when retail sales retarded, this was not because sales of some goods were falling, but because sales of all types were rising more slowly than before. Assume also that retailers reduced orders for goods for stock when sales decelerated, producing first a retardation and eventually a downturn of total orders for stock and for sale even though sales were still rising. To cap it all, assume that the same thing occurred at each level of distribution and production—that because of accelerator-induced inventory effects, the orders by wholesalers from manufacturers first retarded and then led their own sales downward; and that orders placed by manufacturers of finished consumer goods for manufactured materials or components first retarded and then led their own production, etc., etc. We would then observe a sequence of production declines, and over time an increasing proportion of contracting industries, as a result of a sustained general retardation of retail sales unaccompanied by specific declines.[15]

Whereas the foregoing sequence is possible, the weight of empirical evidence goes against it. Ruth P. Mack's thorough-going case study of demand propagation showed that the cycles in production of hides, leather and shoes tended to be synchronous with retail shoe sales, and gave no sign of progressively earlier timing at the "earlier" stages of processing [7]. Other evidence, admittedly incomplete because of the paucity of matched data for the various stages of production and sale of specific final products, suggests that manufacturers' buying does not lead orders received by them for their own goods [8]. Since orders may be transmitted rapidly through the vertical market structure, the likely result will be approximately synchronous production cycles on the earlier levels, but cycles which might or might not lead retail sales,

[15] It is necessary to assume that there is a period of retardation not only of sales, but also of purchase orders at each stage. This is because the orders of one stage are the sales, often with a lag, of the next. Suppose retail sales retarded, and as a result, retailers' orders retarded for, say, 4 months and then fell. This would mean that wholesale trade would be retarded for 4 months, and the orders of wholesalers to manufacturers could then retard for, say, 3 months before falling, etc., etc. If retail retardation caused an immediate decline of purchase orders, in contrast, wholesalers' and manufacturers' orders would drop concomitantly and there would be no opportunity for prior turns at the earlier stages of production and sale.

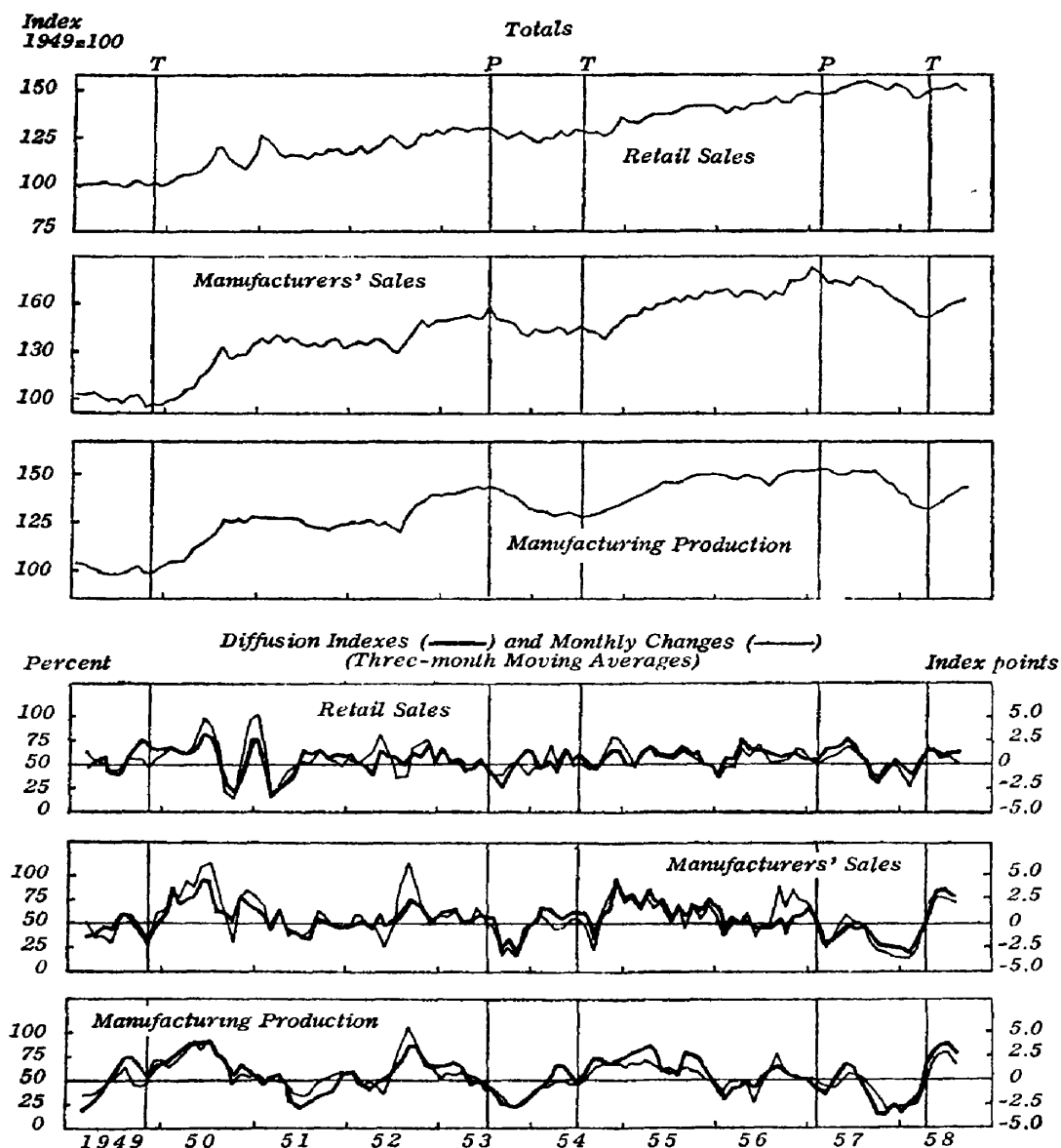
depending on whether and by how long retailers' orders lead retail sales.[16] However, because lags of manufacturing production behind incoming orders will tend to cancel leads of purchase orders by retailers over retail sales should the latter occur, factory sales and production may well synchronize rather closely with retail sales [8, pp. 47-48].

It appears improbable, therefore, that an increasing proportion of manufacturing industries would experience production declines unless there were also an increasing percentage of final products experiencing sales reductions. This judgment is supported broadly, though not conclusively, by the data contained in Chart 3. Indexes of total retail sales and of manufacturing sales and production are included in the top half of the chart. Below them appear corresponding diffusion indexes and rates of change, smoothed by moving averages to suppress random variations. The data are defective in two respects for our purposes. Most serious is the fact that the breakdown of industries underlying the diffusion indexes of manufacturing sales and production, differs considerably from the breakdown of retail sales by type of store.[17] Second, retail sales measure only one category of final demand which impinges on manufacturing industry.

Despite these deficiencies, the data tell a coherent story. First, comparison of retail sales with their diffusion index reveals the expected correlation between rate of change and extent of diffusion. This is enough to show that even when it comes to final demand, aggregative retardation manifests itself in individual declines as well as retardations. Particular products or lines will usually retard before reversing, but the leads are apt to be short, and it may be just as well to neglect them when it comes to generalizing about the relationships among sales retardation, inventory adjustments, and purchase orders by retailers in the aggregate—although this is more in the nature of hypothesis than conclusion. The close correlation beeween retardation and diffusion of retail sales shows that production diffusion is not a consequence merely of individual decelerations in retail markets, but it does not exclude the possibility that acceleration effects are present as well, causing brief leads of purchase orders over sales of individual items or lines.

[16] New orders by department stores sometimes lead their sales by several months, although they usually are synchronous [8, pp. 24-26].

[17] The components for the diffusion index of retail sales include motor vehicle dealers; tire, battery and accessory dealers; furniture and home-furnishings stores; household-appliance and radio stores; lumber and building-materials dealers; hardware stores; apparel stores; drug and proprietary stores; eating and drinking places; grocery stores; general merchandise stores; and liquor stores. The industries included in the diffusion indexes of manufacturing sales and production are primary metals; fabricated metals; machinery; transportation equipment; lumber and furniture; stone, clay and glass; food and beverages; tobacco; textiles; paper; chemicals; petroleum and coal products; and rubber. I have not included the heterogenous groupings of "other" durables and nondurables in the computations.

CHART 3.—RETAIL AND MANUFACTURERS' SALES AND MANUFACTURERS' PRODUCTION, TOTALS, MONTHLY CHANGES, AND DIFFUSION INDEXES, SEASONALLY ADJUSTED, MONTHLY, 1949-1958

*Vertical lines indicate peaks and troughs of manufacturing production.*

*Sources: Department of Commerce and Federal Reserve Board.*

Second, it is readily apparent that manufacturing sales and production move in tandem, although sales are more subject than output to random variations. Especially notable is the fact that the turning points are virtually synchronous, with sales either leading by one month or coincident with production on four of the five cyclical turns. Approximate synchronization is also the rule for individual industries, to judge from the diffusion indexes. These curves bear the expected relationships to the rates of change of their respective aggregates.[18] More to the point, they are also correlated with one another, just as one would predict if production were a positive, nearly synchronous function of sales in each industry.[19] Because of the nature of the data, each "industry" may embrace separate stages of production and sale, but for reasons already cited, the several stages probably are closely synchronized.

It is a matter of some importance that sales and production are approximately synchronous for the individual industries. Lloyd Metzler [9] and Ragnar Nurkse [11] have shown that efforts by businessmen to adjust inventories in proportion to recent changes in sales may cause aggregate production to decline simultaneously with aggregate sales and realized inventory investment. But the coincident downturn of sales and production in these models results from the (immediate acting) feedback of production income on effective demand. When production and sales decline simultaneously in a particular industry, however, it can scarcely be argued that its own production cutback induced its own sales decline. This would make no difference if all industries declined together, but as has been emphasized throughout this paper, the downturn of aggregate output is preceded by a fairly prolonged period of retardation during which there is a growing accumulation of individual production declines. Since production tends to turn with or behind sales in these instances of early decline and while aggregate income is still rising, it would appear that for individual manufacturing industries the simplest explanation is the correct one: namely, that causation runs from sales decline to production downturn.[20]

[18] The correlation of sales movements is even closer when the measures of monthly change and diffusion are based on deflated sales.

[19] Inspection of the data for the separate industries confirms that production turns with or shortly after deflated sales in the overwhelming majority of instances.

[20] In many manufacturing industries the advance warning provided by new and unfilled orders will minimize the tendency for production to lag sales. Metzler's assumption that current production decisions are based on past levels and rates of changes of sales is perhaps more applicable to retailers than manufacturers, since retailers do not usually possess advance information about their own sales. The explanation offered by Mack and Zarnowitz [8] of the lead of retailers' orders over retail sales on some downturns, incorporates a corrective-order accelerator similar to Metzler's, along with allowance for the influence of short-term market prospects on purchase decisions.

Manufacturers' inventory investment could, nevertheless, contribut to a downturn of business activity through its feedback effects o national income, without necessarily causing manufacturing output t decline prior to manufacturing sales in any industry or in the aggre gate. First, realized inventory investment may lead production eve if the latter is synchronous with sales. Provided the early decrease o inventory investment in a given industry were caused by sales retarda tion, this would be the accelerator at work. If the industry bulked larg in total inventory investment, or if investment leads occurred in sufficient number of industries, manufacturers' inventory investmen could decline while manufacturers' sales and production were still o the upgrade. Secondly, the same aggregative result could arise eve if realized inventory investment were synchronous with sales in eac industry, provided the early decliners were above-average investor (relative to sales). This is analogous to the case of unequal weightin discussed above in connection with fixed investment, and does no involve acceleration effects. Third, manufacturers' realized inventor investment might be coincident with sales in each industry and th aggregate (the case of equal weighting), but factory sales and produc tion might lead national product.

It would be interesting to compare the timing of real inventor investment with sales and production in each industry, but this woul be a research project in itself. As for inventory investment in manu facturing as a whole, quarterly changes in the estimated book valu of manufacturers' stocks as published by the Department of Com merce, show moderate leads or coincident turns with manufacturin production during the postwar cycles, and it is doubtful that correctio for price movements would alter this relationship appreciably. Thi in turn implies that manufacturers' inventory investment will some times lead the business cycle by half a year or more, especially when as in 1956-57, inventory investment leads manufacturing productio and the latter also declines considerably earlier than total business The important point, however, is that this route takes us right bac to demand diffusion, for if aggregate income is retarded by a down slide of inventory investment, the retardation will manifest itself partl in specific declines affecting particular products or industries.

All the same, if the income retardation is due to a reduction o manufacturers' inventory investment, is this not the accelerator i action, at least in situations where realized inventory investment lead sales and production in individual industries? Yes it is, but its rol is more limited than usually implied. This sort of inventory effect ma contribute indirectly to a general downturn by retarding aggregat income, but it will not in itself cause a decline of aggregate output. I

inventory investment falls while production is still rising in an industry, it obviously has not acted directly to decrease production, and its indirect effect is to slow the increase of personal production income and consumer demand, rather than to reduce them.[21] This differs considerably from a situation in which a decision to reduce inventory investment is in itself a decision to decrease production in advance of sales.

Finally, we compare sales of manufacturers with those of retailers. Here are both similarities and differences, as must be expected since manufacturers also sell producer goods and supply government demands. The two series do display similar contours through most of the period, however, amplitudes aside. The major discrepancies are during the Korean war and in 1957. At other times, interestingly enough, retail sales led or were virtually synchronous with those of manufacturers. These tendencies toward parallelism appear also in the indexes of sales diffusion, with retail diffusion synchronous or leading on the turns in 1949, 1953, 1954, and 1958. The reader is reminded that these last indexes differ in composition. Nevertheless, the correlation is consistent with the hypotheses and evidence already considered, and it is difficult to believe that it is an accidental one.

The principal discrepancy occurring during a "normal" period came in 1957, when manufacturing sales and production led retail sales by half a year. This experience should not be taken as verification of the acceleration principle as applied to retail inventory investment, however, if for no other reason than the fact that retail sales *accelerated* to their final peak. What happened was this. As we already know, sales of producer goods began falling early in 1957, primarily because of, and augmenting the spread of, production declines among manufacturing industries many of which dated back a year or more. When retail sales accelerated during the spring and early summer, and retail diffusion therefore increased, correlative increases appeared in manufacturing. These increases among consumer goods were too small to offset fully the declines in other demands, however—partly because retailers did *not* increase their orders enough to maintain stock-sales ratios—so that manufacturing production did move upward, but insufficiently to regain the peak. The closing spurt of retail sales, incidentally, indicates that the main channel by which the early decline of manufacturers' inventory investment could help induce a downturn —that is, by retardation of personal income and consumption—was

[21] This is the case when the reduction of inventory investment is offset by diminished business saving within the manufacturing sector. If the reduction should cause an early decline of demand for raw materials, however, it might decrease production and income in the extractive industries. As far as minerals production is concerned, however, it usually turns with or after manufacturing production.

blocked temporarily by other factors, including increases in government expenditure and net foreign investment during the first half of the year and a rise in the ratio of consumption to disposable income in the third quarter.

Despite the gaps in the evidence presented—the lack of data on diffusion of government demands, the imprecise information on the timing of manufacturers' inventory investment and retailers' orders, and the slender sample of observed cases of vertical synchronization of production and sales for specific products or lines—the tentative conclusion appears warranted that acceleration-induced inventory investment may be a comparatively unimportant initiating (as contrasted with amplifying) factor in business contractions. Its main effect may well be to retard income growth and thereby to augment the spread of specific declines, rather than to cause a direct reduction of aggregate demand. It is certainly consistent with the known facts to assume that the adverse effects of aggregative retardation are due principally to individual declines instead of individual retardations, that sales and production on all market levels tend to be synchronous with sales of the final products from which their demands are derived, and that such departures from synchronous timing as may result from inventory change are small enough to be neglected.

## III. *Diffusion, Investment, and Cycles*

Let us now inquire further into the phenomenon of diffusion and its relationship to the level and rate of change of aggregate activity. There are three intriguing questions: Why the positive correlation between diffusion and the rate of change of aggregate activity? What are the implications of that correlation for the theory of investment? For the theory of business fluctuations?

The answer to the first question becomes apparent when one asks whether all individual outputs should be expected to rise or fall in a fixed proportion to real national income. This is obviously absurd, since the structure of demand and supply, and with it the composition of prices and output, is constantly being altered by the differential impact of dynamic factors like technological change, population growth, and shifting tastes. When aggregate money income may rise freely and individual supplies are elastic, most demands and outputs will rise, although some more slowly than others. Once a restriction is placed on the advance of either real or money income, however, it becomes virtually certain that expansion will continue in some directions at the expense of contraction in others.

Suppose a full-employment ceiling is encountered, for example, so that the advance of real national income is limited to the rate made

possible by growth of the labor force and man-hour productivity. Firms and industries favored by bright profit prospects because of process or product innovations are unlikely to remain content with such a slow rate of progress, and will doubtless bid vigorously for resources. And, of course, on the side of final demand, households, businesses and governments are apt to diminish some demands in order to augment others within the budget constraint imposed by real income. The result will be that some industries will be squeezed between upward shifting cost curves and more slowly rising or falling demand schedules, and will be forced to curtail production.

Should a monetary ceiling be approached instead of or in addition to one on real income, differential impacts are again to be expected. General credit restraints do not retard all demands evenly, and some will decline as others increase, with corresponding effects on supplying industries owing to specialization in production and the resulting resource immobilities.

Finally, the scope and rate of expansion will be positively correlated even when the latter is unchecked by external constraints. There are two main possibilities: First, individual demands may diminish for reasons unrelated to the level of national income, such as prior errors of business judgment about investment or production, autonomous changes of taste, market saturation after a period of vigorous growth, or special circumstances due perhaps to replacement waves or peculiarities of market structure, as in cobweb situations or the building cycle. If resources are imperfectly mobile, these specific reductions may not be offset immediately by increases elsewhere, with the result that aggregate income will increase more slowly or decline.

Secondly, aggregative retardation may occur independently of external ceilings or individual maladjustments, but with adverse consequences for particular demands all the same. The marginal propensity to consume may decline, for example, retarding aggregate demand relative to income and inducing declines in specific industries. A similar result may follow with a fixed marginal propensity to consume if personal income retards because of a drop of inventory investment. Another possibility which may generate endogenous "free cycle" downturns analogous to those of accelerator-multiplier models, will be discussed presently.

The empirical correlation between industrial diffusion and the rate of change of aggregate activity, then, has a solid rationale in the theory of resource allocation under conditions of disturbance and disequilibrium, of shifting functions and ceaseless change. What are the consequences for investment theory? The most obvious consequence is that the composition of investment matters, in the sense that it affects the

behavior of aggregate capital formation. I would like to sketch a conceptual scheme which provides, I believe, a useful framework for the analysis of the interaction between components and aggregate.[22]

The first element of the framework is that "law of industrial growth" which says that individual industries pass through a life cycle of several stages: introduction of the product and a period of vigorous expansion as it wins general acceptance, then slower growth as it nears market saturation, and eventual decline. These phases would occur, I assume, even if national income were stationary, and would have obvious consequences for the composition of demand and production. Since national income itself has a rising trend, however, each industry growth curve will be tilted upward to reflect the increase which would occur even with constant output composition. Now, since the life span of an industry is usually long—at least if it is not defined too narrowly in terms of product—a given segment of its growth curve during a period of say, 5 or 10 years, may be approximated by a straight line of constant percentage increase. The line may tilt up or down depending on the growth stage, and represents the prevailing tendency of output due to the combination of specific growth factors affecting the industry and the secular uptrend of national income.

Next, make gross investment a positive function of level of output. This means that industries with strongly rising production tendencies during our 5 to 10 year period, will have correspondingly rapid investment uptrends. It may be objected that rapid capacity growth could be achieved with a constant, though large, annual flow of net investment. If we measure our production growth tendencies at constant percentage rates, however, this implies that steady growth would require an increasing amount of net investment per year: increasing, in fact, at the same percentage rate as production if capacity were always adjusted optimally to output.[23] Additionally, replacement demand will increase over time in growing industries, although not in constant proportion to output if growth occurred at different rates in previous decades. Remember, these are not rigid relationships we are assuming, but merely positive correlations between average growth rates of production and investment.

[22] In doing this, I will merely be restating propositions made familiar by a long line of students of the cycle, including Schumpeter, Mitchell, Robertson, Burns, Hansen, Haberler, Fellner, and Gordon.

[23] The postulated relation between production and investment "trends" amounts to a loose application of the acceleration principle to the case of long-term adjustment of capacity to output, but without the implication that investment fluctuates with the rate of change of output in the short run. Notice that under the assumed trend conditions, net investment would always be in fixed proportion to both the level of output and its absolute change. The factor of proportionality in the latter case would be the accelerator coefficient, and in the former, the product of the accelerator coefficient and the growth rate of output.

Actual growth will not, of course, occur steadily. National income will fluctuate, and with it production in each industry, according to its price and income elasticities. And, as noted earlier, there will be factors which foster independent fluctuations specific to certain industries. Nor will investment proceed steadily, for it varies with output during the short run as well as over the decade. Notice that our formulation simultaneously embraces those autonomous investment determinants which underlie industry growth curves, and the tendency for movements of national income to induce fluctuations of investment by influencing individual outputs. What is left out, because imperfectly reflected in output if at all, is autonomous investment of the sporadic variety—the defense investment of the Korean war is a pertinent example—and the influence of cost and availability of external funds.

Now we are in a better position to understand the implications for investment of the correlation between retardation and diffusion. When national output rises more slowly, it is primarily because more industries are contracting than before, and these decreases spell trouble for the affected firms—they are members of "sick" industries or of industries in late stages of growth, or they are experiencing temporary difficulties due to special factors. In any event, this is more serious than if all industries were retarded evenly and none declined. In that case, profits would continue to rise everywhere, unless margins narrowed sufficiently to offset rising outputs, whereas when individual declines occur profits are virtually certain to fall in the distressed sectors. But a situation where production and profits are falling, is more detrimental to investment than one in which output is merely retarded. On the financial side of the investment decision, retained profits are an important source of investment funds. Expected returns from investment are doubtless sensitive to current profits, moreover, especially when the latter are declining during a period of general business expansion and are therefore symptomatic of weakness within the industry.

As far as the declining industries are concerned, then, investment is likely to fall, whereas it would probably continue to rise if output were merely retarded. But will not investment be enough higher in the expanding industries by virtue of the fact that their outputs and perhaps their profits can now increase more than otherwise, to compensate the decreased investment of the distressed sectors? This sort of offsetting certainly does occur, particularly when the specific declines are not an independent cause of the retardation of aggregate income but are themselves induced by expansions in other industries within a given over-all constraint. As we shall see, however, there are several reasons why the offsetting is unlikely to persist indefinitely. This brings us to the last question: How does our conception of the typical investment-

output relationship in individual industries, fit into the theory of business fluctuations? First, as regards the upper turning point, how can general contraction set in if investment usually synchronizes with production or lags it in each industry? There are several possibilities.

For one, investment may lead output in some industries and on some occasions even if it does not do so typically. Financial constraints may prompt investment leads, for instance, as may autonomous political or military events. And, of course, some categories of final demand, not ordinarily classified as investment, are partly independent of real income and may decline autonomously. Government expenditures and consumer demands for durables are the principal examples. Now, in all these cases, the specific "investment" declines may readily be absorbed if other demands are rising sufficiently, but if they occur at a time when the offsetting movements of investment in the expanding and contracting sectors of the economy are approximately balanced, it may require only one individual lead to cause a general downturn.

It must be emphasized in connection with the role of income-autonomous demands in the downturn, that my empirical analysis of fixed investment was confined to manufacturing and mining industries: that is, it dealt with industries producing goods instead of services. This is important because services cannot be stockpiled by buyers and must be produced as consumed, with the result that production of marketed services often continues to increase (at a retarded rate) during mild contractions of national income. Yet fixed investment in these industries—public utilities, housing, trade and the like—often fluctuates with the business cycle. Evidently in such cases, investment is linked only loosely to sales or production in the short run, and it is certainly conceivable that investment declines in these industries are caused by production retardations via the accelerator mechanism. Notice, however, that even if such accelerator-induced declines do occur, they will not initiate a general downturn unless they come during the expansion phase of the business cycle. Residential construction did lead on the downturns in 1948 and 1956-57, but these declines were clearly caused by financial constraints rather than an excess supply of housing. Investment by public utilities, moreover, has either failed to fall or has lagged on the three postwar downturns. Of course, three instances do not make a timeless generalization, and it is necessary to remember that when excess capacity does develop in the housing market, the condition may persist for many years, and that business contractions are apt to be especially severe during the long downswings in building. But this is an example of a maladjustment which may develop in a particular industry independently of the level or rate of change of national

income, and has little bearing on the processes by which business expansions are regularly reversed, or on the accelerator as a general theory of short-term investment demand.

Closely related to the foregoing point is our second observation, that a downturn may be delayed as well as precipitated by specific income-autonomous demands. This occurred in 1957, for example, when industrial investment lagged industrial production and yet led the business cycle,[24] because offset temporarily by autonomous increases in other sectors, which then ceased to rise or fell before they could feed back to the industrial sector.

Third, investment inequalities among industries may cause a downturn of aggregate investment before aggregate production even when there are no individual investment leads. Actually, it is more likely that inequalities will delay the investment decline, as in manufacturing industry during 1956-57. This is because unequal lags can act only in the direction of greater or lesser delay, whereas there is no systematic reason why capital-intensive industries should be the first to decline.

Fourth, the composition of the aggregate may matter in the sense that individual investment demands are partly a function of the extent of diffusion. This hypothesis was advanced by Burns. "The rise in construction and financing costs during an expansion impinges broadly on the investing class and would check the investment boom sooner or later even if prosperity diffused itself uniformly over the economic community. But this does not happen, and the uneven spread of expansion is our second vital fact" [2, p. 22]. As the proportion of firms experiencing rising profits begins to shrink, firms become more hesitant about investing *now*: "The firms whose fortunes are waning are likely to be among the first to reduce investment expenditure, and their curtailments spread doubt among others whose profits are still rising, but many of which have come to feel that investment costs will be reduced before long from the abnormal level to which they have been pushed by prosperity" [2, p. 22].

Here, diffusion becomes an expectational variable affecting the timing of investment expenditure and capable of causing an endogenous downturn. It could do this even in the absence of resource immobilities and external constraints on aggregate activity. If investment in each industry is a function of general diffusion as well as its own output; if the individual production tendencies imply that some will be forced into declines because others expand more rapidly than aggregate in-

[24] The peak of industrial production came in the first quarter, that of industrial investment in the first or second (see fn. 7), and the business peak in the third.

come; and if the average propensity to consume is constant at less than one, aggregate income will turn down endogenously, provided the individual investments sum to less than the average propensity to save when diffusion falls below some critical value. Burns refers to profits diffusion, but the correlation with output diffusion is so close that one may stand for the other. The major difference that could arise would be a price-cost squeeze which narrowed margins sufficiently to offset stable or rising outputs and caused profits to lead production.

Abramovitz has directed my attention to a fifth way in which the spread of individual declines may act to depress aggregate investment. When the proportion of firms enjoying increasing profits diminishes, this reacts adversely on the prices of the stocks of unfortunate firms on the market and sympathetically on all share prices. The uncertainty and pessimism caused by the accumulating declines, may depress the market while total profits are still rising. Hence it becomes more difficult to market new issues on favorable terms, and investment plans may be postponed even by firms which are still prospering.

I have concentrated my attention on the downturn, since this is where the relationship between diffusion and the rate of change of aggregate activity is most critical for cycle theory. The apparatus I have sketched is also useful for the study of contraction and recovery, however, and of the relation between growth and stability. Just as the accelerator has pre-empted attention in cycle theory in recent years, so also has it, or its cousin the capacity principle, in growth theory. Earlier writers, such as Schumpeter, Robertson and Hansen not only got closer to those mainsprings of growth—innovation and population—which are usually autonomous in the modern models, but they also merged more intimately the determinants of growth and fluctuation. For them, business cycles were the necessary consequence of industrial progress, and the dispersion which occurred among industries during a cycle was critical to its development. Whether contractions are long or short, deep or shallow, depends partly on the influence of population, technology and other income-autonomous factors as they are expressed in the prevailing production and investment tendencies of the industries within the economy, and partly on the response of spending units to changes of income, capacity utilization, financial conditions, and prices. The autonomous factors deserve more attention than they have received of late, and industrial disaggregation is one of the most promising lines of attack.[25]

[25] On this subject, see [5] and the works cited there. The topic is treated more extensively in my forthcoming Brookings study of the growth and stability of the postwar economy.

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# UTILITY THEORY, DECISION THEORY, AND PROFIT MAXIMIZATION

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Two seemingly contradictory hypotheses as to the shape of a consumer unit's utility function for choices involving risk are current in economic literature. The older, often termed the Weber-Fechner law, postulates diminishing marginal utility of income as income increases at all income levels [16]. The application of a utility function possessing this property to the analysis of choices involving risk dates back at least to Bernoulli's analysis of the St. Petersburg paradox [3, p. 60]; a contemporary exposition of risky choices in terms of diminishing marginal utility can be found in Vickrey's 1945 treatment [20].

More recently, Friedman and Savage have advanced a rationalization of an important class of reactions to risk in terms of a utility function which is concave upward over a certain range of incomes; i.e., a function such that the marginal utility of money income increases over that range [3]. The primary contribution of this analysis was to rationalize gambling and insuring by the same individual.

The purpose of this paper is to point out some inadequacies and oversimplifications of each of these two theories and to suggest an approach which seems to make possible a more satisfactory analysis of economically significant risky choices.

## I. *The Validity of Utility Analysis*

The Weber-Fechner law, postulating continuously diminishing marginal utility, has been recognized even by its proponents as inadequate in at least two major respects. A popular criticism is that the Weber-Fechner law implies no gambling by an individual reacting rationally to choices involving risk; proponents of this law have usually characterized gambling as irrational behavior [9, p. 135n]. A more telling criticism is that the Weber-Fechner law makes no allowances for differences in individuals with respect to their taste for risky situations

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and fails to consider the peculiar characteristics of various kinds of choices involving risk.

The utility hypothesis advanced by Friedman and Savage explains gambling and insurance by the same individual acting rationally. This is done by asserting that an individual's utility function is shaped like that of Figure 1. This function is nevertheless unsatisfactory as a general theory of reactions to risk since some of its main predictions do not accord with the facts [8]. For example, Friedman and Savage state
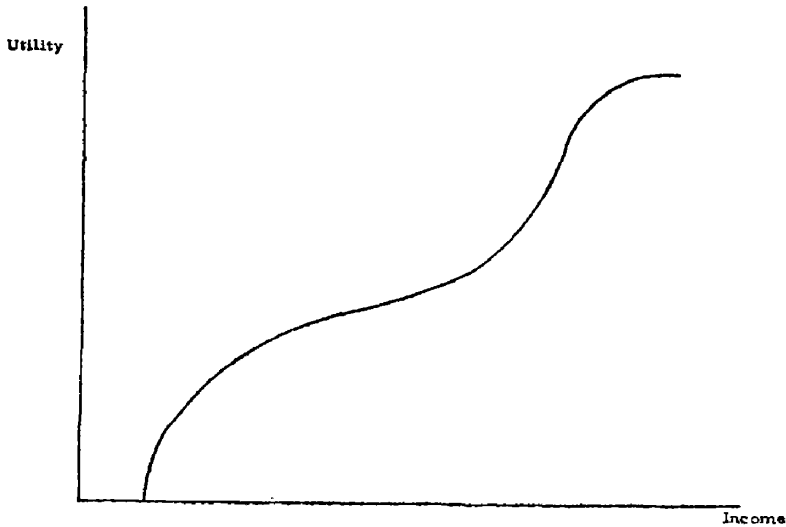
FIGURE 1

[3, p. 93] that the concave (upwards) segment of the utility function represents the situation of a "disproportionate fraction of entrepreneurs . . ." and [3, p. 92] that individuals in the concave segment will be ". . . continually subjecting themselves to risk . . ." and hence ". . . are likely to move out of the segment; upwards, if they are lucky; downwards, if they are not." These statements taken together imply a rapid turnover in the ranks of people who act as entrepreneurs because entrepreneurs are individuals who continually subject themselves to risk. Further, these statements imply that individuals do not continue to act as entrepreneurs over wide ranges of income because once they leave the concave segment their preferences regarding risky choices are markedly altered.

We maintain that these implications of the Friedman-Savage hypothesis are inconsistent with observable facts since entrepreneurs are the same individuals over relatively long time spans (on the average at

least 6 years, in many cases much longer) and over relatively wide ranges of income.[1]

More fundamentally, we object to the implicit equating of "subjecting one's self to risk" and "gambling." Friedman and Savage were careful to define gambling as a venture wherein the mathematical expectation, using objective probability calculations, was a loss of income. "If it [the consumer unit] gambles, the actuarial value of its income is $\bar{I}$, which in general is less than $I_0$. $I_0 - \bar{I}$ is the premium it is paying for the chance to gamble (the "take" of the house, or the "banker's cut") [3, p. 76]. The value of their analysis lay in its ability to account for just this kind of gambling. But by then extending their analysis to entrepreneurs and predicting that many of them came from the concave (upwards) segment of the utility function, they attribute to the entrepreneur a peculiar psychology indeed. The entrepreneur now becomes an individual who maximizes his expected utility by expecting to lose money.

How many entrepreneurs undertake a business venture when they really *expect* to lose money? We suspect very few. We doubt seriously that the Ford Motor Co. expected to lose money by introducing the Edsel. The person who invests his life savings to open a corner delicatessen or yet another service station hardly does so in the expectation of losing money. These ventures are risky, to be sure; they can hardly be classified as "gambling" in the definition given by Friedman and Savage. Dun and Bradstreet have outlined the major reasons for small business failures [2]. All these reasons are types of mismanagement or mistakes—not one is related to an intentional gamble, as defined above. In other words, we submit that "gambling" and "subjecting one's self to risk" are not synonymous. Gambling decisions are a particular sub-

[1] The following statistical data for the United States support our contention [18, p. 8]:

| *Date* | *Firms in Operation* (thousands) | *Discontinued Business* (thousands) |
|---|---|---|
| | (during year ended at date given) | |
| Dec. 31, '52 | 4, 178.8 | |
| Dec. 31, '53 | 4, 185.3 | 690.2 |
| Dec. 31, '54 | 4, 232.3 | 649.3 |
| Dec. 31, '55 | 4, 252.0 | 642.2 |

If we assume, conservatively, that no business employed more than one person who acted in an entrepreneurial capacity for the firm (i.e., that one man made all the production and investment decisions) and that all the discontinued businesses resulted in the permanent conversion of an entrepreneur into an ordinary employee, then it would take the following number of years for the entrepreneurial population to turn over once: using 1953 data: 6.05 years; using 1954 data: 6.44 years; using 1955 data: 6.59 years.

We have found no satisfactory statistical evidence to support the contention that entrepreneurs continue to act as entrepreneurs over wide ranges of income; we must lean on the weak reed of "consistency with general observations" here.

set of a much more inclusive set of risky decisions. One can avoid gambling; one can hardly avoid risk in one form or another. The decisions involving risk in which economists are primarily interested are not the gambling ones, but rather the much more pervasive class of nongambling yet risky decisions—the kinds of risky choices individuals must of necessity make in the day-to-day conduct of their affairs.

A major weakness of each of the above explanations is a failure to distinguish between three major classes of decisions involving risk which are in fact marked by fundamental differences. The most important class of decisions, for economic analysis, we will term operational. For operational decisions, utility is a function primarily of income alone; nonmonetary considerations are of minimal importance and may be safely relegated to Haberler's "*ceteris paribus* dump." This class includes most important economic decisions involving risk and is the object of central concern for economic theory. Examples are business production decisions (the Edsel), plant expansion decisions, business inventory purchases, security investments; in fact, most day-to-day risky business decisions fall in this class.

The other two classes of decisions involving risk are those in which factors other than income exert a significant influence. One class we shall call marginal decisions; the other structural. Marginal decisions are those associated with the possibility of only relatively small losses and which at the same time are influenced appreciably by factors other than income. Some of these other factors may be a desire for entertainment, a desire to be sociable, Albert Hart's "purchase of the right to hope," Adam Smith's "overweening conceit," and similar considerations. The purchase of lottery tickets, participation in games of chance for small stakes, flipping for the dinner check, and the like are examples of marginal decisions. In each case *the possible loss is small,* while at the same time nonmonetary factors weigh heavily in determining the utility used as a basis for the decision. Friedman and Savage developed their model in terms of this class of decision.

The remaining classes of decisions, which we shall call structural decisions, are characterized by choices the outcomes of which may "change the whole course of relevant future events for the individual" [14, p. 6], and which might be expected to substantially ". . . alter many of the surrounding economic conditions" [10, pp. 6-7]. Decisions falling in this class include: the choice of a career, a business venture involving all or most of an individual's capital, a general's choice at the crucial stage of a battle, and the choosing of a wife (a risky, structural, and at least partly economic decision certainly not amenable to actuarial calculation). Insurance purchases are not included in the

above examples, for they are not homogeneous. Purchases of fire, theft, and disaster insurance are apparently considered only in conjunction with some other type of decision.

The decision process might take this form: Alternative A is a business venture without insurance which calls for an investment of $100,000 (in an oil drilling rig, say); offers the prospect of a maximum return of $1,000,000 or the loss of the total investment of $100,000 by disaster before oil is found. Alternative B is a combination of A plus insurance. Now the prospects become, if the insurance premium is $10,000, a gain of $990,000 or a loss of only $60,000 ($50,000 depreciation of equipment, $10,000 insurance premium).

Alternative A's bleakest prospect may be one of complete bankruptcy, while B's worst possible outcome, though markedly unfavorable, is still far short of total ruin. It may well be that an individual would reject A but still undertake B; yet venture B is really a *combination* of venture A plus insurance. Thus, the possibility of insuring against fire, flood, hurricane, and other disasters makes it possible for a decision-maker to alter the terms of the venture under consideration, reduce the spread of possible outcomes, and put some sort of "reasonable" floor under the losses involved. We suspect that most insurance purchase decisions are in fact of this type, rather than a decision taken after one has already decided to undertake the venture in question. Behavior which would tend to disprove this hypothesis would be widespread evidence of persons undertaking a venture for some considerable period of time, then deciding to insure against disaster losses. Likewise, if the same firm had several such projects under way and the distribution of insured vs. noninsured ones were random, our hypothesis would be questioned.

Failure to insure at all would not, it should be noted, contradict this formulation of insurance decisions. This would simply mean that project A was of itself acceptable, or that the decision-maker was big enough to act as his own insurer, or both.

The purchase of life insurance, on the other hand, will usually be a structural decision in that the possible losses without insurance are too large to be considered in operational terms. In addition, nonincome considerations may exert a significant influence on the decision to purchase life insurance.

As can be seen from the above discussion of the various decision types, size of possible loss relative to total assets is always a necessary and sometimes a sufficient criterion for classifying decisions. In case size of possible loss alone does not furnish enough information for classification, it then becomes necessary to consider the presence or absence of appreciable nonmonetary influences. Using these criteria,

marginal decisions are always associated with small possible losses and often with nonmonetary influences as well. Structural decisions differ from marginal decisions in that the possible losses are very large relative to total assets. Operational decisions are those involving neither extremely small nor extremely large losses and for which nonmonetary considerations are insignificant.

Many writers, including Vickrey, have mixed structural and operational decisions when analyzing choices involving risk in terms of utility. Friedman and Savage, taking a different tack, seem to put a good deal of emphasis on the marginal type of decision, but they appear to be unwilling to distinguish the three classes of choices. They state that ". . . individuals choosing among occupations, securities, or lines of business activity are making choices analogous to those that they make when they decide whether to buy insurance or to gamble" [3, p. 58]. We contend that while gambling decisions are for the most part marginal decisions, the purchase of insurance (other than life insurance) seems to be usually only one part of an operational decision, and the choice of a career or line of business activity seems to be clearly a structural decision.

We propose to develop a model capable of analyzing operational decisions; it may not yield useful results when applied to either marginal or structural decisions. Operational decisions, after all, are the very subject matter of economics.

## II. *The Decision Model*

We can frame our model in terms of the subjective probabilities approach made famous by L. J. Savage [13]. In the case of operational decisions, the individual of necessity is unsure of the outcome. In the most favorable circumstances, the individual expects the outcome of a particular venture to assume any of a set of possible values, and acts as if he can attach a particular probability to each of these results [13, Ch. 5].

For the sake of clarity, we will develop the model in terms of an investment decision, then show how the model can be applied to other operational decisions. Essentially, we view the decision process as a sequential narrowing of the focus of attention to the point where a discrete value is selected, which selection makes comparison and thereby decision possible. This step-by-step concentration of attention on an ever-narrowing range of possible outcomes can be thought of as embodying five successive phases: (1) deciding how much decision effort, measured in man-hours, dollars, or other suitable units, to expend on the particular choice in question; (2) selecting a "manageable" set of possible outcomes on which to spend this effort; (3) eliminating im-

mediately from further consideration by a "scanning process" those choices for which the *relevant* extreme outcomes are either too bad, not good enough, or both; (4) selecting for each project which passes the scanning process a single central value which allows the decision-maker to abstract from uncertainty; and (5) selecting from a number of possible ventures the one with the most attractive central value.

We will develop the model by explaining each of these steps. We will then apply the model to two examples to demonstrate applications of its principles and draw some of the economically interesting implications from the analysis.

A. *Decision Effort*

We assume that the total amount of decision effort available to an individual is limited to some finite and definable quantity $\Phi$. By reducing executive time, consultants' fees, investors' service subscriptions, and expense account lunches all to dollars, one could very probably measure decision effort in dollars. Be that as it may, it seems reasonable to suppose that as a result of the physical time limit placed on many decisions (bids for new treasury issues, say) and the press of other affairs, the effort one can afford to spend on any operational decision is necessarily limited.

Moreover, it seems not unlikely that the amount of effort devoted to any particular decision will be some monotonically increasing function of the size (relative to the total assets available to the decision-maker) of the particular investment under consideration. If this is the case, it follows that $\Psi_i$, decision effort spent on any single choice, becomes $\Psi_i = \chi_i \cdot \Phi$, where $\chi_i$ is the relative size of the venture, subject to two conditions:[2]

$$(1) \qquad \chi_i \geqq 0,$$

$$(2) \qquad \sum_1^n \chi_i = 1.$$

(In this case,

$$\sum_1^n \Psi_i \leqq \Phi,$$

the inequality allowing for the circumstances in which total available decision effort is not fully utilized.)

Thus, the computation of $\Psi_i$, amount of decision effort to be spent

[2] It should be noted that $\sum_1^n \chi_i$ is equated to the total assets at the disposal of the decision-maker, not necessarily the total owned by him as an individual.

on choice $i$, appears as the first step in our decision-making model. Keeping in mind the kinds of decisions the model is designed to handle, it will be clear that more often than not this first step will itself consume little if any measurable time or effort, but will of necessity be made—consciously, as in the case of "Let me know by noon tomorrow," or unconsciously as one pursues the necessary daily routines of decision-making. We find evidence of this sort of process in the widespread tendency to "not bother the boss with this problem" and the progressive increase in the dollar magnitudes of the "this" in question as one progresses upward through most hierarchies.

B. *The Attention Span*

The decision-maker is immediately confronted with another problem: on what to spend that effort available for a given choice. For any given investment project the range of possible outcomes is in principle infinite. Spreading a finite amount of decision effort ($\Psi_i$) evenly over an infinite number of outcomes would mean that each possible outcome received an infinitesimal amount of attention—hardly a feasible procedure. Further, it is clear that not all possible outcomes do, or should, exert equal influence on the decision-maker. Hence, he is faced with the problem of optimally allocating his available decision effort $\Psi_i$ among certain of the possible outcomes. A necessary precondition to such an allocation of effort must be the ordering of the possible outcomes in some manner so that certain of them may be selected for further attention. The most obvious, and at the same time the most logical, criterion for this ordering is the dollar magnitudes associated with each outcome. The dollar magnitude of the outcomes is, after all, the primary focus of the decision-making process. Consequently, we suggest that the investor begins his analysis by arranging the possible outcomes according to their dollar magnitudes.

Here, it appears that Koopman's solution to the problem of optimum distribution of target search effort can be adapted to posit a solution to this allocation-of-decision-effort problem.[3] Assume that the investor

[3] We believe this to be an acceptable adaptation in view of the rigorous proof offered by Koopman [6, pp. 616-21] and the generality of the method as expressed by him when he states [6, p. 623], "Moreover, the basic quantitative ideas and methods of this paper need not be confined to the case of *search:* Any other form of *effort*, provided merely that its pay-off is additive and that each unit is expressible by the exponential saturation formula, as above, is an appropriate field of application for the present material." For our treatment an additive pay-off function means that the greater the amount of information available the more accurate is the decision-making process. If the assumption of an exponential saturation formula is not acceptable, Koopman goes on to state [6, p. 626], "The same is true [the analysis can be extended], with even greater difficulties, if the detection law (or pay-off function) is not of the simple exponential type assumed" (parentheses from original)

assigns to each of the possible outcomes a subjective probability $p(x)$ subject to two conditions:

$$p(x) \geqq 0, \tag{1}$$

$$\int_{-\infty}^{+\infty} p(x)dx = 1. \tag{2}$$

Further assume that the investor wishes to maximize another probability, namely the probability that he will consider that particular outcome $Z$ which will (*ex post*) prove to be the actual outcome of the venture in question. In the nature of the case this outcome is not now recognizable; still, it would be a strange investor indeed who did not want to maximize his chances of letting this actual outcome figure in his decision. Yet, the investor at this stage must of necessity reduce the decision to manageable dimensions by eliminating some outcomes from further consideration; hence the problem of maximizing the probability of considering $Z$ now confronts the investor. The graphical solution to this problem (following Koopman) is illustrated in Figure 2. Plot the possible outcomes of the venture (measured in dollars, say) along the $x$-axis with the origin representing a complete loss of the entire investment. On the vertical axis plot the natural logarithm of $p(x)$. Let $\psi(x)\ dx$ be the amount of decision effort expended on outcomes in the range $(x, dx)$ subject to the conditions:

$$\psi.(x)dx \geqq 0, \tag{1}$$

$$\int_{-\infty}^{+\infty} \psi(x)dx = \Psi_i. \tag{2}$$

Over the interval $(x, dx)$ the probability that the investor will consider $Z$ is given by:

$$p(x)[1 - e^{-\psi(x)}]dx.$$

The over-all probability that he will consider $Z$ (the actual outcome) then becomes:[4]

$$P = \int_{-\infty}^{+\infty} p(x)[1 - e^{-\psi(x)}]dx = P[\Psi_i].$$

The graphical solution is to construct parallel to the $x$-axis a line $AA$ which contains between it and the curve $y = ln\ p(x)$ an area (shaded on Figure 2) equal to $\Psi_i$, decision effort allocated to this particular choice. Here the horizontal dimension is dollars, the vertical one the natural logarithm of a probability. Since logarithms are nondimen-

[4] For a rigorous proof of this, see [6].
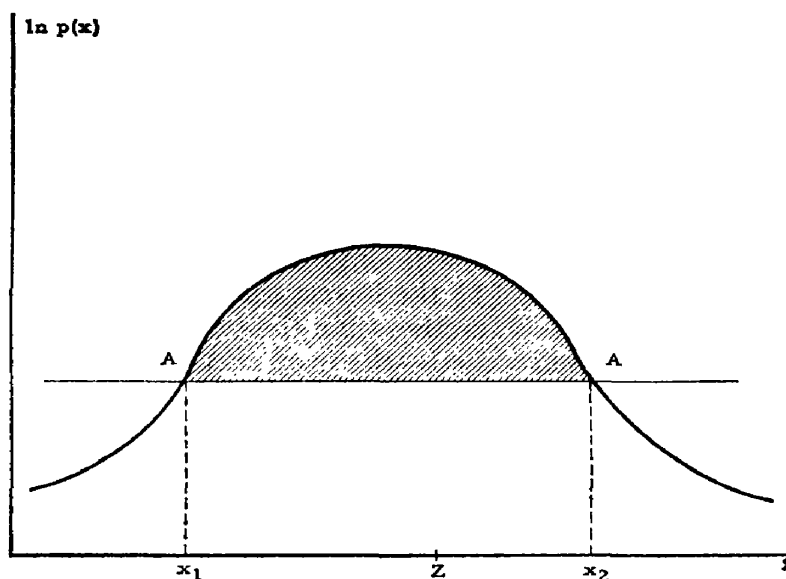
FIGURE 2

sional, the shaded area of Figure 2 represents a sum of dollars. Equating this area to $\Psi_i$ amounts to saying that we can express $\Psi_i$ in dollars, as indicated above.

This step in the decision-making process accomplishes three things. First, it enables one to optimally distribute the available decision effort, in that if one expends $\Psi_i$ on the outcomes between $x_1$ and $x_2$ one maximizes the probability that the actual outcome $Z$ will be considered. Second, this step furnishes the investor with some "logical" method of defining the range of possible outcomes he will consider in detail—the range between $x_1$ and $x_2$. Third, this step provides the investor the first two specified values yet available in the decision process: the most favorable ($x_2$) and the least favorable ($x_1$) outcomes he is willing to consider under the stated limitations of effort to be expended. This most favorable outcome to be further considered ($x_2$) can be called the highest relevant outcome, or HRO; $x_1$ can similarly be termed the lowest relevant outcome, or LRO. Outcomes outside this range are in a sense "not worth worrying about." Within this range ($x_1$, $x_2$) lies a manageable set of possible outcomes which the investor must evaluate in some manner to arrive at a decision on the venture.

But it appears that a great many operational choices (investment projects) are rejected before any discernible amount of effort is spent on them. This phenomenon we would explain by asserting that the individual at this stage subjects the venture to a "scanning process" to

determine whether or not it is worth while to continue with a more detailed analysis.

### C. *The Scanning Process*

The explanation of the scanning process requires a further assumption: investors behave as if they had stable range-of-interest limits that correspond to what we might call *boundary loss* and *boundary gain* criteria. That is, an investor will only consider investment projects which meet two conditions: (1) the lowest relevant outcome (LRO) as determined above is more attractive than the boundary loss (maximum acceptable loss) of the individual—the project does not "scare him off" by affording the relevant prospect of losses too great to be even risked by him; and (2) the highest relevant outcome exceeds his boundary gain (minimum acceptable gain)—the project offers at least prospects of interesting gains. These personal limits of boundary loss and boundary gain are assumed to be stable—they shift only slowly, as the individual's "outlook on life" changes. Persons with a wide span between their loss and gain boundaries (risk-lovers) could be thought of as willing to undergo a great deal more risk than those with relatively narrow spans (risk-averters).

The scanning process consists simply of matching the HRO and LRO of the previous section against the individual's range-of-interest limits. If the LRO is lower than the individual's boundary loss, he then rejects the project out of hand—it is too dangerous for him.[5] If the HRO is less than the boundary gain, the individual rejects the project as not sufficiently enticing. But for those ventures which do "pass" the scanning process, the investor must go on to devise a central value, here called the certainty-equivalent approximation, or CEA, which abstracts from uncertainty and can be used for comparison with CEA's of other ventures.

These first three steps can be thought of as consuming almost no time. In fact, they are probably quickly taken for many projects which never receive further consideration. They may be taken in an almost automatic fashion. We may accordingly consider that the decision effort is expended in the formulation of the certainty-equivalent approximation (CEA) for those decisions which have successfully passed the scanning test.

### D. *Certainty-Equivalent Approximation*

It will be remembered that the investor now has a finite span $(x_1, x_2)$ of outcomes to consider. He has maximized the probability $P\ [\Psi_i]$ that

[5] See the treatment of the possibility of insuring against disasters above. This possibility may enter here; more likely, it has been considered *before* assigning probabilities to the possible outcomes in the preceding step.

this span contains $Z$, the actual outcome. But in the nature of the case he cannot identify which of the outcomes is in fact $Z$. The problem is: what is the best estimate of $Z$?

The best estimate of $Z$ is the mean of the entire subjective probability distribution, but use of this entire distribution would require more effort than the investor has allocated to this decision. Under these constraints, the best estimate available to him is the mean of that portion of his subjecive probability distribution contained between $x_1$ and $x_2$ of Figure 2. This mean we will take as the certainty-equivalent approximation.

The expenditure of $\Psi_i$ can be considered a process of trying to define more and more clearly that portion of the distribution between $x_1$ and $x_2$. This could be done by securing more information, seeking advice and counsel, recalling in detail one's prior experiences along similar lines, and other like measures. All have as their purpose the "sharpening" and further clarification of that part of the distribution covering the relevant range of possible outcomes. Quite probably the shape and position of this refined distribution segment may change during this process; still, all outcomes within this range are examined and affect the CEA.

E. *Selection of Most Attractive Venture*

The fifth and last step in the decision process is quite straight-forward. It consists of comparing the CEA's of those investment projects available and still considered attractive, and choosing that venture with the highest (dollar value) CEA. It may well be that the entire decision process has effectively eliminated all but one or a very few choices, or that this last stage is relatively easy since one course of action stands out as clearly preferable to alternative courses.

For example, it may be that the final comparison is one between constructing a new plant and buying government bonds, with the new plant offering expectations of considerably better returns. This formulation would explain the oft-noted tendency of businessmen to justify past decisions on the basis of "we couldn't really do anything else" or the investor's tendency to give yes or no answers to an investment question. In this latter case, "yes" really means "I prefer this investment to some other one—at worst holding cash."

## III. *The Model Applied to Some Simple Examples*

The *New York Times* (December 9, 1958, p. 63) carried an interesting note on risky decisions which might be examined in terms of this model. The passage describes the plight of a runner who was almost late submitting a government security dealer's bid on a Treasury bill

offering, then goes on to outline the procedure used by banks and security dealers to formulate their bids.

> Although the window is open each Monday from 1:00 to 1:30 P.M., on [sic] most bids for part of the Treasury's weekly offering of almost $2,000,000,000 of its shortest-dated securities aren't received until minutes before the deadline.
>
> Banks and dealers, who bid competitively for the bulk of the auction, narrow the range of the likely bidding in telephone conversations with customers that start early in the morning. About 1 P.M. they begin translating yields into prices. Then from 1:15 on, they sharpen their prices until, at the last possible moment, they put them on paper and send their messengers scurrying.

This example illustrates first the necessity of deciding how much time to spend making the decision. In this case it appears that dealers have decided to spend from 9 to 1:30, or some four and one-half hours, on this decision.

Our model would indicate that the dealer's knowledge of market conditions enables him to form almost immediately a subjective probability distribution of the possible outcomes of bidding on an issue, once the Treasury announces the terms of the issue. Using this subjective distribution and the amount of time available to him, he is able to secure a highest relevant outcome (HRO) and lowest relevant outcome (LRO) as explained by step two of the model. In step three he compares the LRO to his boundary loss and the HRO to his boundary gain, either rejecting the project at this point or accepting it for further consideration. None of these steps has consumed a significant amount of time. It is step four, formation of the CEA, on which the decision effort is expended.

The decision to bid on the particular issue having passed the scanning process, the dealer commences early in the morning to secure all the information he can on the current and expected future state of the market. This may be taken as an attempt to more clearly define the dealer's subjective probability distribution between the limits of the venture's HRO and LRO. He continues to expend decision effort to firm up his evaluation of the subjective distribution. The clearer (to him) the relevant segment of the distribution becomes, the more nearly able he is to select with confidence the mean of that segment. This mean (the CEA) is his best estimate of the most profitable bid he can make which will secure the amounts he desires.

A second example which might be explained in terms of this model is the business inventory purchase decision. Inventory decisions are usually phrased by businessmen in terms of how much they desire to have

on hand, which amount is itself likely to be some function of expected future sales. An uncertainty model which takes account of expectations as to future sales may yield useful results. In our treatment the businessman would order his expectations of future sales along the $x$-axis of Figure 2, assigning a subjective probability to each value. Then, after deciding how much decision effort to spend on this particular choice, he would follow the sequence of steps specified by the model to arrive at a CEA of expected future sales. The amount of inventory purchased then becomes equal to desired stocks plus future sales less current stocks, where the CEA is used as the deciding value for future sales.

Our model can, we believe, be used to explain many if not most operational risky decisions. Space limitations prevent further multiplication of examples, but the two we have cited seem to be representative of a wide range of day-to-day business decsions.

## IV. *Implications of the Model*

Since this model evolved from a dissatisfaction with two schools of utility-income theory, perhaps we should examine our own model's utility-income implications. The first of these is that, of the three kinds of decisions here described, marginal and structural decisions may not be amenable to useful analysis by *any* theory which limits itself to considerations of income and utility alone. For these types of risky choices many nonincome factors may be expected to intrude with a significance sufficient to submerge income-utility considerations. As Majumdar points out, Friedman and Savage have offered a model which has its greatest applicability in the area of marginal decisions, but which is not likely to be useful in other areas [7].

Further, our model would imply that many operational decisions—those rejected by the scanning process—are not susceptible to utility-income analysis at all since the decision-maker does not even calculate in these terms for these rejected choices. These projects are eliminated on risk considerations alone. For those operational decisions which are amenable to utility-income analysis, our model requires that utility be an increasing linear function of income. We posited that the individual would, for these choices, always prefer a venture with a higher expected return to a venture with a lower CEA. This result would not necessarily follow with either diminishing or increasing marginal utility of money income. For example, in Figure 3 utility is plotted against income, and diminishing marginal utility of money income prevails to the left of $B'$ while increasing marginal utility of money income prevails to the right of $C$. Here the income, or CEA, $I_1$ associated with a venture having the relevant range of outcomes $AA'$ would yield an expected utility of $U_1$,
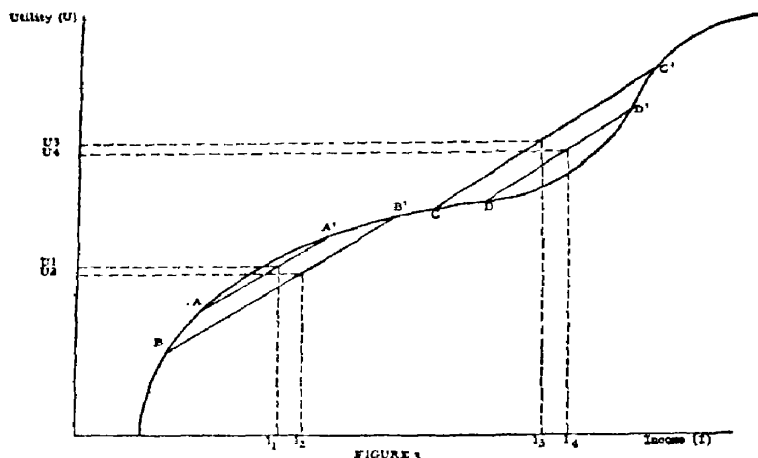
FIGURE 3

while CEA, $I_2$ of the venture $BB'$, would be associated with an expected utility of $U_2$. Assuming that the individual maximizes expected utility, he would prefer $I_1$ to $I_2$. Similarly, $I_3$ of venture $CC'$ would be preferred to the higher CEA, $I_4$ of venture $DD'$. Neither of these results is admissible to our model since we specify that a higher CEA is always preferred to a lower one.

The only utility-income function consistent with this stipulation is one for which the marginal utility of money income is constant. Here it is clear that the higher CEA is always preferred to the lower, and that the individual is indifferent between acceptable projects with identical CEA's. To keep from claiming too much, we repeat that this linear utility-income relationship is predicted only over the range of incomes derived from operational decisions as heretofore defined. Our hypothesis is not necessarily inconsistent with the very persuasive arguments for diminishing marginal utility of income *over other ranges of income.*

A tax implication of these utility conclusions would be that proportional taxation of capital gains seems to be proper since these gains are usually the outcomes of operational decisions. This model gives us one consideration to bear in mind when framing tax policy: we are likely to skew the inventor's distributions of expectations by imposing progressive taxes on investment returns. We may want to do this for various reasons; our model simply states that we may do so with a progressive tax. Alternatively, the probability distribution of expected returns can be deliberately skewed by certain types of tax policies—a regressive tax on investment returns might increase the willingness of individuals to accept risky ventures. The proportional corporate income tax seems

to have this effect on the total tax bill of American taxpayers in high income classes [5, Ch. 2, and pp. 192 ff.] [19, p. 153] [15, pp. 187-96] [1], and tax exemptions on new industrial facilities may have the same result.

Another implication of our hypothesis, one that directly contradicts that drawn by Friedman and Savage from their analysis, relates to the expected stability of that part of the population which makes operational decisions. We would expect that an old hand at any line would be one who could form relatively accurate subjective probability distributions, could define these clearly, and choose a CEA which often comes close to anticipating actual developments. We would expect the chances for survival of more experienced entrepreneurs to be considerably better than those of inexperienced entrepreneurs. In fact, experience acts through this model to improve both the scanning process and the formation of the CEA. It does so by enabling the individual to form his initial probability distribution with more speed and accuracy, thereby improving the scanning process. Experience also helps make more accurate his determination of the CEA by giving him additional information and teaching him what information he needs for the decision.

The widespread notion of regret enters the decision-making process in at least two places: the scanning process and the CEA determination.[6] If the investor rejects a project at the scanning-process stage, only to find that it later proves to be quite profitable for someone else, it is natural to suppose that this later knowledge will give rise to a feeling of regret at having acted so hastily. Regret may also arise as the result of large inaccuracies in the determination of the CEA. The concept of regret may have a feedback effect on the learning process as the individual experiences the fabled "hard knocks" of economic life. The obverse feedback effect is associated with "elation" at making decisions which later prove to have been based on acccurate predictions. Regret and its obverse, elation, thus seem to play a central role in the process of learning [12], helping the individual sharpen his scanning process and his CEA selection.

## V. *Concluding Remarks*

Like all subjective probability theory, our theory is highly abstract. The crucial problems of the time horizon of risky choices have been discussed elsewhere, as have those of risk-spreading [4], so we can consider our abstraction from these problems to be merely a con-

[6] Samuelson and others have noticed the importance to any expectation model of being able to explain regret [12, pp. 205-6].

venience. Still, we ought to be able to specify the kinds of empirical data which would tend to contradict our model. It is clear that a businessman who ignores all possible outcomes except one single "best guess" when evaluating an investment project acts in a manner that contradicts our theory. Taking the other extreme, evidence of widespread concern for estimates of very unlikely outcomes of a small investment project would also tend to contradict our model. As we have noted above, evidence that businessmen expected to lose money when they made new investments would contradict our theory.

If utility is an increasing straight-line function of income associated with operational decisions, then our theories based on profit-maximizing entrepreneurs are justified. To deny profit maximization, of course, is to deny our ability to make determinate statements about anything, as Stigler and others have noticed.[7]

The chief virtue of our model is its provision of systematic insight into the role of uncertainty as it affects investment and other types of operational decisions.

[7] Largely because the theory of the firm is developed in terms of profit maximization, there are no other really successful theories—utility theory included [17, pp. 144 ff.] [11, pp. 17-19, 21-23].

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# PAYMENTS FOR LABOR AND FOREIGN TRADE

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Since the end of the second world war, the governments of the world's major trading nations have been re-examining their foreign trade policies and engaging in efforts—for the most part, joint efforts—to reduce tariff and other barriers to trade. In most countries, this reconsideration has involved more or less extensive investigation of the probable effects of increased exports and imports on the supply of goods, prices, returns to invested capital, employment and wages as well as on government revenues. The international comparisons which these studies require are difficult to make because of the lack of comparable statistics.

The most difficult of all are international comparisons of wages and supplementary benefits (which for convenience we shall call payments for labor). The purpose of the following analysis is to show that available statistics make possible a better understanding of differences in payments for labor between countries than is obtained by the usual crude comparisons of outlays for hourly wages. From the point of view of the United States, it demonstrates a method by which currently published statistics for the major trading countries may be used to indicate whether imports represent "unfair" competition to American industry. It also shows that more intensive research is needed particularly on the cost of supplementary benefits paid to workers in certain industries important in world trade.

Studies of the probable effect of proposed changes in international trade barriers almost always include, at least in the United States, whatever data can be assembled on comparative costs in the importing country and in the foreign countries which are major sources of supply. Whatever material throws light on differences in labor costs and wage rates is brought together in order to estimate whether labor costs in the industry in the exporting countries are "substandard." Frequently, but not always, it is possible to find, for the commodities and countries concerned, all the required material on money payments to labor

* Miss Williams was chief of the Office of Labor Economics of the Bureau of Labor Statistics prior to her death in 1958 and Mr. Eaton was chief of the Branch of Foreign Trade Employment within that office. The interpretations and conclusions are those of the authors and do not necessarily reflect the views of the U.S. Department of Labor.

(direct wage payments plus social security taxes plus other supplementary payments established under collective bargaining or required by local custom). However, for very few commodities is it possible to locate data on comparative output per man-hour worked.

Comparisons of the money paid out for labor without data on output are usually not adequate for evaluating international competition in the production of a given commodity. If payments for labor in a given country are (at current exchange rates) one-half those in another country, but output per man-hour is only one-third as great, both factors must be taken into account in estimating comparative labor costs.

Payments for labor as a factor in international trade are considered from two different points of view. First, as one element in the cost of production, they may well be translated from one currency to another by means of the foreign exchange rate. Second, they may be considered from the social point of view as determining the real levels of living of workers in different countries—in this instance foreign exchange rates do not provide a satisfactory conversion factor for measuring the comparative domestic purchasing power of different currencies.[1] The bill of goods and services which enter into foreign trade and determine the exchange rate is quite different from the bill of goods and services which make up workers' family budgets.

An understanding of differences between payments for labor in export industries in a given country and those in the relevant import-competitive industries in another country is facilitated by relating payments for labor in those industries affected by foreign trade to other measures of the relative economic position of the nations concerned: comparative national income per member of the labor force, comparative wage and salary payments in the total economy, and comparative payments for labor in all manufacturing industries combined. This is because the level of wages and supplementary payments for labor in a given industry in a given country is determined by a combination of these interacting factors. The data brought together in the following

[1] Differences between the values of currencies in foreign exchange and in domestic purchasing power have been calculated by M. Gilbert and I. B. Kravis for 5 countries as of 1950 and 1952 [2]. Data are given on comparative purchasing power in consumers' goods purchased on the average in these countries; the ratios would not be directly applicable to the measurement of wage-earners' buying power, but the price comparisons on which the ratios are based could be used to calculate such relationships. This study has recently been extended to Belgium, the Netherlands, Norway and Denmark (M. Gilbert and Associates) [1]. In an endeavor to remedy this lack as regards the workers for which it is responsible, the Coal and Steel Community made a study in 1956 of retail prices of goods workers buy in important coal and steel centers in six countries.

An extremely useful analysis of the economic implications of international wage comparisons is the ILO Expert Committee Report (chaired by B. G. Ohlin) [3].

analysis indicate that these factors operate with a high degree of consistency in the great trading nations. The price advantage of competitive imported goods appears in most cases to be based upon cost advantages in elements other than wage levels in the exporting country.

The data cover 17 of the 25 countries each of which was a source of at least 1 per cent of the value of imports into the United States in 1955.[2] In the case of 8 of these 25 countries, U.S. imports were highly concentrated in one or two raw or semiprocessed materials—that is, one commodity accounted for 75 per cent of U.S. imports from the country or two commodities accounted for 80 per cent of U.S. imports from the country in 1955.[3] Since the economic and statistical problems involved in making comparisons of labor payments in these countries with those of other countries were particularly difficult they were omitted from the present study. Partial information indicates that in most cases wages in the exporting industries in each of these countries are above the general level of wages for comparable work in other industries in each country.

The remaining 17 nations accounted for 55 per cent of the value of total U.S. imports in 1955. There is some tendency for the commodities imported from these nations to be concentrated in the more highly fabricated goods, although in the case of the heavy U.S. imports from Canada this concentration in processed commodities is less because of our large purchases of raw or semifinished materials like iron ore, nickel, newsprint, lumber, and similar products.

It is these 17 nations which furnish most of the commodities which compete with rather than supplement U.S. production. This does not imply that some imports from other nations are not directly competitive with some U.S. production nor that all imports from the 17 nations are competitive with U.S. production. But most U.S. import-competition

[2] In 1955 the United States imported over $11 billion worth of goods for consumption from some 131 separate countries. Twenty-five of these 131 countries, each of which supplied at least one per cent of total U.S. imports supplied more than 80 per cent of total U.S. imports for consumption; these 25 countries plus the United States and the Soviet bloc constitute the major trading countries of the world. In 1955 the total foreign trade of all free-world countries included merchandise exports of $84 billion and merchandise imports of $89 billion. The total merchandise trade of the 25 important U.S. suppliers, plus the United States, was $67 billion for both imports and exports. The 26 countries accounted for over 79 per cent of the exports and 75 per cent of the imports of all free-world nations.

[3] These 8 countries, together with the commodities respectively dominating their exports to the United States and the percentages of total U.S. imports from these countries consisting of the specified commodities, are as follows: (1) Brazil, coffee and cocoa, 85 per cent; (2) Venezuela, petroleum and products, 84 per cent; (3) Columbia, coffee, 92 per cent; (4 Cuba, sugar and products and tobacco, 83 per cent; (5) Curacao, petroleum and products, 98 per cent; (6) Chile, copper, 80 per cent; (7) Malaya, rubber and tin, 97 per cent; (8) Indonesia, rubber and petroleum, 82 per cent.

problems which are alleged to originate in differentials in payments to labor will automatically be included if the analysis deals only with these 17 nations.[4] In fact, the only major problem of import competition with U.S. production of this type which is not specifically included is that of competition from Hong Kong in very inexpensive textile and some electrical products.

## I. *Relation between Wages and National Product*

The general level of wages within any country is dependent upon the total national product of that country and the division of that product among the factors with claims upon it. Total national product reflects available resources, the degree to which those resources are utilized, and the productivity of capital and labor. The division of total national product among the various factors making a claim on it depends upon the relative supply and bargaining position of the various factors, the social organization and mores of the country, and the range of alternatives open to each factor. The general level of wages, given some level of national product, will reflect the position of the workers vis-à-vis capital and government and the extent to which their requirements and desires are filled from sources other than take-home pay, such as dismissal allowances and medical care programs.

The dispersion of wage levels among industries within any single country reflects the same general factors as those determining the general level of wages in that country—the natural resources, capital goods and techniques available to that industry, the extent to which those resources are utilized, the degree of skill required by the workers in the industry, the relative bargaining powers of the factors involved in the industry, and the demand for and growth in production of the industry. The range of alternatives open to the factors of production may be somewhat more important in the determination of industry wage levels than in the determination of national wage levels, but this is purely relative.

International comparisons of general wage levels and international comparisons of wage levels for individual industries must take these factors into account. The following analysis of the basic problem proceeds in two separate steps. The first is to compare the general level of payments for labor with the ability to pay (as measured by gross domestic product) for each of the 17 countries and the United States. The second is to compute the ratios of the levels of payments for labor in individual manufacturing industries to the average for all

[4] Competition which arises from differences in natural resources, etc., is not included here.

**Table 1—Gross Domestic Product per Member of Labor Force and Compensation per Wage and Salary Worker**
(U.S. dollars)

| Country | Gross Domestic Product per Member of Labor Force (U.S. dollars) | | | Compensation per Wage and Salary Worker (U.S. dollars) | | | Labor Force (thousands) | | Wage and Salary Workers (thousands) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1950 | 1955 | Per Cent Change | 1950 | 1955 | Per Cent Change | 1950 | 1955 | 1950 | 1955 |
| Argentina | 647 | 504 | −22.1 | 499 | 375 | −24.8 | 6,446[a] | 7,500 | 4,520[a] | 5,250[b] |
| Australia[c] | 2,488 | 3,143 | +26.3 | 1,348 | 1,946 | +44.4 | 3,238 | 3,702 | 2,489 | 2,949[d] |
| Belgium[e] | 2,006 | 2,471[d] | +23.2 | 1,264 | 1,574[d] | +24.5 | 3,511 | 3,568 | 2,400[b] | 2,414 |
| Canada | 2,871 | 4,259 | +48.3 | 1,951 | 3,029 | +55.3 | 5,300[e] | 5,583[f] | 4,085[e] | 4,364[f] |
| France[c,g] | n.a. | 2,438 | — | n.a. | 1,755 | — | — | 19,188 | — | 12,000 |
| Germany | 840 | 1,358 | +61.7 | 663 | 1,102 | +66.2 | 22,074 | 24,522 | 15,631 | 17,331 |
| India[h] | 197 | 136 | −31.0 | n.a. | n.a. | — | 101,775[e] | 153,000[f] | — | — |
| Italy | 584 | 930 | +59.2 | n.a. | n.a. | — | 20,672 | 20,117 | 11,566 | 11,225 |
| Japan[h] | 257 | 444 | +72.8 | 283 | 551 | +94.7 | 36,310 | 42,640 | 13,970 | 16,320 |
| Mexico[c,g] | 808[i] | 700 | −13.4 | n.a. | n.a. | — | 8,345 | 9,603 | 3,831 | 4,409 |
| Netherlands | 1,159 | 1,628 | +40.5 | 791 | 1,127 | +42.5 | 3,896 | 4,166 | 2,750[b] | 2,961 |
| Philippines | 415 | 411 | − 1.0 | 442 | 796 | +80.1 | 7,416[i,j] | 9,497[f] | 2,992[j] | 2,078[f] |
| Scandinavia | | | | | | | | | | |
| Denmark | 1,522 | 1,946 | +27.9 | 937 | n.a. | — | 2,063 | 2,118[k] | 1,498 | 1,526[k] |
| Norway | 1,428 | 2,099 | +47.0 | 985 | 1,529 | +55.2 | 1,388 | 1,473 | 987 | 1,004 |
| Sweden[c] | 1,793 | n.a. | — | 1,267 | n.a. | — | 3,105 | — | 2,384 | — |
| Switzerland[l] | 1,944 | n.a. | — | 1,560 | n.a. | — | 2,156 | — | 1,607 | — |
| United Kingdom | 1,401 | 1,948 | +39.0 | 1,038 | 1,466 | +41.2 | 22,579 | 23,912 | 20,500 | 21,299 |
| United States | 4,040 | 5,107 | +26.4 | 3,138 | 4,217 | +34.4 | 64,599 | 69,692 | 49,295 | 53,160 |

*Note*—Unless otherwise indicated gross domestic product is at current factor cost. Conversion to U.S. currency based on official exchange rates.
[a] 1947 data; used in computing 1950 per capita figures.
[b] Estimated on basis of change in labor force.
[c] National product data at current market prices.
[d] 1954 data.
[e] 1951 data; used in computing 1950 per capita figures.
[f] 1956 data; used in computing 1955 per capita figures.
[g] Gross national product data.
[h] Net domestic product.
[i] 1952 data.
[j] 1948 data.
[k] 1953 data; used in computing 1955 per capita figures.
[l] National income.
*Sources: U. N. Statistics of National Income and Expenditure*, Ser. H, No. 10, New York 1957; *I.L.O. Year Book of Labour Statistics*, Geneva 1957; *OEEC Statistical Bulletin*, Paris, Sept. 1957.

TABLE 2—RATIO OF PER CAPITA WAGES TO PER CAPITA GROSS DOMESTIC PRODUCT,[a] 18 NATIONS, 1950 AND 1955

| Country | Year | |
|---|---|---|
| | 1950 | 1955 |
| Argentina | 77.1 | 74.4 |
| Australia | 54.2 | 61.9 |
| Belgium | 63.0 | 63.7 |
| Canada | 68.0 | 71.1 |
| France | — | 72.0 |
| Germany | 78.9 | 81.1 |
| India | — | — |
| Italy | — | — |
| Japan | 110.1 | 124.1 |
| Mexico | — | — |
| Netherlands | 68.2 | 69.2 |
| Philippines | 106.5 | 193.7 |
| Scandinavia | | |
| Denmark | 61.6 | — |
| Norway | 69.0 | 72.8 |
| Sweden | 70.7 | — |
| Switzerland | 80.2 | — |
| United Kingdom | 74.0 | 75.3 |
| United States | 77.7 | 82.6 |

[a] Compensation per wage and salary worker and gross domestic product per member of the labor force.
*Source:* Based on Table 1.

manufacturing in each of the 17 countries and the United States. These ratios are then compared with each other. In order to eliminate the element of differences in the size and rate of participation of the population in the labor force, all data are expressed on a per-member-of-labor-force or per-wage-and-salary-worker basis.

Tables 1 and 2 summarize the data on gross domestic product per member of the labor force and total wage and salary payments per wage and salary worker in each of the 17 nations and the United States for 1950 and 1955. For ease of comparison all data have been converted into dollars on the basis of official exchange rates. These tables also show the total labor force and the total number of wage and salary workers in each of the countries.

In 1955 gross domestic product per member of the labor force ranged from $136 in India to over $5,100 in the United States while compensation per wage and salary worker, not available for India, ranged from $375 in Argentina to over $4,200 in the United States. Restricting comparisons only to countries for which both gross domestic product and wage and salary compensation data are available,

the ranges are about the same in the two series—the lowest consumption country at about 10 per cent of U.S. levels and the highest at about 75 per cent of U.S. levels. There is almost perfect rank correlation between per capita gross domestic product and per capita wages and salaries for the 17 countries for which data are available in either 1950 or 1955—the higher the per capita product, the higher is the general wage and salary level and vice versa.

Table 1 also indicates the relative importance of wage and salary workers in the economies of the countries. In the United States wage and salary workers make up about three-fourths of the labor force and this ratio is approximately the same (70-80 per cent) for most of the countries of Western Europe, Canada, and Australia. Wage and salary workers make up a larger share of the labor force in the United Kingdom (90 per cent), largely because of its relatively small agricultural employment; while the comparatively greater prevalence of small independent farms and shopowners causes the figures in France and Italy to fall in the 55-65 per cent range. The three countries, however, which show extremely low participation of wage and salary workers in the labor force are Mexico (45 per cent), Japan (40 per cent), and the Philippines (20 per cent). (India, with over 70 per cent of its labor force in agriculture, would also probably be in this low range if comparable data were available.) These latter four countries all have proportionately large shares of their labor force engaged in agriculture, and a large number in subsistence production.

If the United States ratio of per capita wages and salaries to per capita gross domestic product is used as a basis for comparison (Table 2), it appears that Australia, Belgium, and Denmark devote a relatively small share of their product to wages and salaries, or that the returns to self-employed workers in these three countries are higher than wages and salaries. The Japanese and Philippine ratios are greater than unity, primarily because an extremely small portion of their respective labor forces work for wages and salaries. This may also reflect shortages of personnel in relation to the demand for trained workers.

A large share of the differences in the ratios of per capita wages and salaries to per capita gross domestic product for the different countries is probably due to variations in the distribution of the labor force between wage and salary workers and self-employed (or unemployed) persons. In Table 3, the basic data on per capita gross domestic product and per capita wages and salaries have been converted into ratios to the United States data for both 1950 and 1955; these ratios have been plotted in Chart 1.

If each country followed exactly the U.S. pattern in the ratio of

TABLE 3—RATIOS OF GROSS DOMESTIC PRODUCT PER MEMBER OF THE LABOR FORCE AND OF AVERAGE COMPENSATION PER WAGE AND SALARY WORKER FOR SELECTED EXPORTING COUNTRIES TO THOSE OF THE UNITED STATES

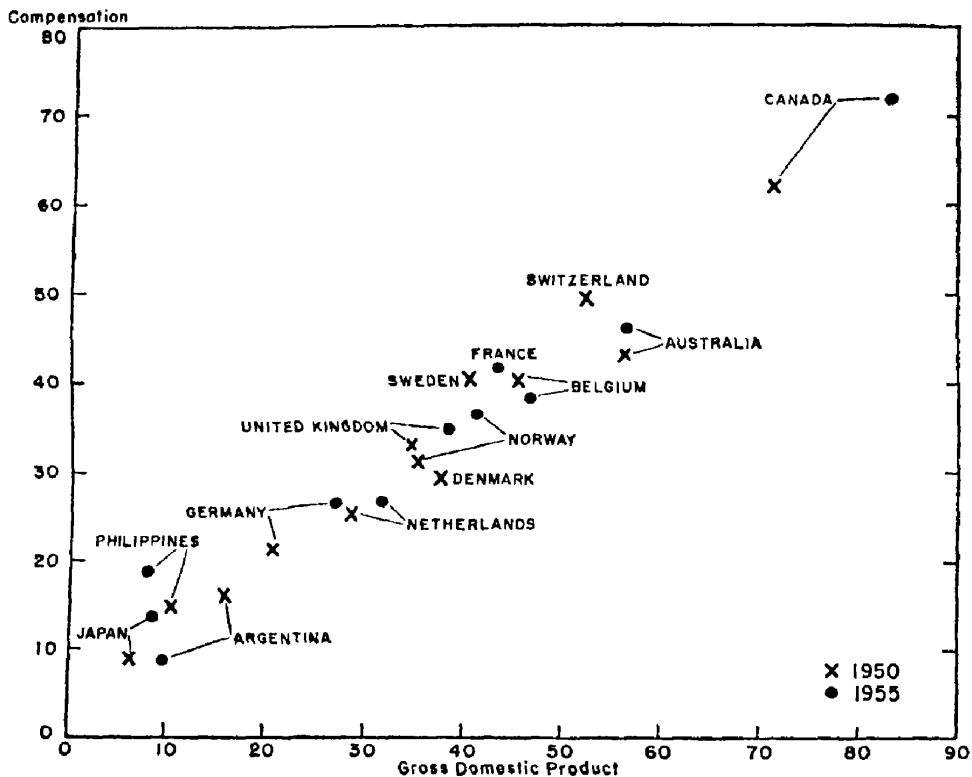
| Country | Ratio of Gross Domestic Product per Member of Labor Force to that of United States (U.S.=100) | | Ratio of Compensation per Wage and Salary Worker to that of United States (U.S.=100) | |
|---|---|---|---|---|
| | 1950 | 1955 | 1950 | 1955 |
| Argentina | 16.0 | 9.9 | 15.9 | 8.9 |
| Australia | 56.3 | 56.1 | 43.0 | 46.1 |
| Belgium | 45.4 | 46.4 | 40.3 | 38.0 |
| Canada | 71.1 | 83.4 | 62.2 | 71.8 |
| France | — | 43.5 | — | 41.6 |
| West Germany | 20.8 | 26.6 | 21.1 | 26.1 |
| India | 4.9 | 2.8 | — | — |
| Italy | 14.5 | 18.2 | — | — |
| Japan | 6.4 | 8.7 | 9.0 | 13.1 |
| Mexico | 15.5 | 12.5 | — | — |
| Netherlands | 28.7 | 31.9 | 25.2 | 26.7 |
| Philippines | 10.3 | 8.0 | 14.1 | 18.9 |
| Scandinavia | | | | |
| Denmark | 37.7 | 38.1 | 29.9 | — |
| Norway | 35.3 | 41.1 | 31.4 | 36.3 |
| Sweden | 40.6 | — | 40.4 | — |
| Switzerland | 52.5 | — | 49.7 | — |
| United Kingdom | 34.7 | 38.1 | 33.1 | 34.8 |
| Average for all countries[a] | 24.1 | 27.9 | 26.7 | 30.1 |
| United States | 100.0 | 100.0 | 100.0 | 100.0 |

[a] Only includes countries for which gross domestic product and compensation data are available—13 countries in 1950, 11 countries in 1955.

*Source:* Derived from Table 1; all country notes in Table 1 also applicable here. All comparisons with United States are based on identical time periods and national accounts data

wages and salaries to national product, all the points in Chart 1 would lie on the diagonal passing through the zero point at a 45-degree angle. If all nations followed an identical pattern but this pattern was different from that of the United States, the points would still fall along some line, but not the diagonal.

As shown in Chart 1 the relationships between per capita wage and salary payments and gross national product per member of the labor force in the United States and in these 17 countries are very close in both 1950 and 1955. The coefficient of correlation is + .98 for both 1950 and 1955; this relationship is confirmed by inspection of material for intermediate years. The gross national product per member of the labor force in the United States divided by the average for the 13 countries for which both figures are available for 1950 gives a quotient of 4.14. Wage and salary payments per wage and salary earner

CHART 1. COMPENSATION PER WAGE AND SALARY WORKER AND GROSS DOMESTIC PRODUCT PER MEMBER OF THE LABOR FORCE—RATIOS TO UNITED STATES, 1950 AND 1955

United States Department of Labor, Bureau of Labor Statistics.

for the United States divided by the average for the 13 countries gives a quotient of 3.74. The ratio between these two quotients is .9. Following the same procedure for 1955 gives a ratio of .92. These figures imply that wage and salary payments in the United States are somewhat higher in relation to the gross national product than they are in the other countries.

When the ratios of payments to labor in manufacturing in each of 10 countries to payments in the United States, given in Table 4 and in Chart 2, are put side by side with the ratios for value of output[5] per worker in manufacturing the correspondence is much closer. The difference in the degree of correspondence between (a) manufacturing payments to labor and output per worker and (b) total labor payments and gross national product might indicate that salary levels and the level of payments to labor outside the manufacturing industry appear to be relatively higher in the United States than in the other

[5] Contribution of manufacturing to gross domestic product.

TABLE 4—VALUE OF OUTPUT AND INCOME PER WORKER IN MANUFACTURING IN SELECTED COUNTRIES, 1955

| Country | Gross Domestic Product from Manufacturing per Worker in Manufacturing (U.S. dollars) | Labor Income in Manufacturing per Worker in Manufacturing (U.S. dollars) | Ratios to United States[a] | |
|---|---|---|---|---|
| | | | Product | Income |
| Argentina | 877 | 578 | 14.3 | 13.3 |
| Belgium[b] | 2,060 | 1,194 | 36.8 | 31.2 |
| Canada | 4,760 | 3,194 | 77.4 | 73.4 |
| Denmark | 2,206 | 1,497 | 35.9 | 34.4 |
| Germany[c] | 1.594 | 1,068 | 25.2 | 24.5 |
| India[bd] | 335 | 247 | 5.8 | 5.7 |
| Japan | 633 | 475 | 10.3 | 10.9 |
| Netherlands[e] | 1,731 | 776 | 27.5 | 21.6 |
| Norway | 2,428 | 1,444 | 39.5 | 33.2 |
| United Kingdom | 1,954 | 1,315 | 31.8 | 34.3 |
| United States | 6,146 | 4,351 | 100.0 | 100.0 |

[a] Comparable U. S. figures including mining and/or construction and/or utilities used in computing ratios.
[b] 1954 data.
[c] Includes mining and utilities.
[d] Includes utilities and construction.
[e] Includes mining, construction, and utilities.
*Sources: U. N. Statistics on National Income*, Ser. H. No. 10, New York 1957. *I.L.O. Year Book of Labour Statistics*, Geneva 1957.

major trading countries. It might be worth while to investigate whether this difference in relative salary levels results from differences in the status of executives in the United States and other countries. It may be that management in other countries is more often provided by owners of enterprises who receive their remuneration primarily in the form of interest and dividends, while in the United States it is more often provided by salaried officials.

Tables 3 and 4 and Charts 1 and 2 appear to indicate that none of the countries studied is maintaining an unduly low general level of payments for labor in terms of its own economy. The fact that these countries all appear to be paying labor at a rate which is justified by their general economic condition does not, of course, rule out one aspect of the question of wages in international competition. International trade in specific commodities may, in some cases, exist because the advantage of low wages in an export country is not offset by higher productivity in countries with generally higher levels of payment for labor. What these data do show is that none of the 17 countries has a general comparative advantage based on payments for labor at a level lower than that which its economy could support.

TABLE 5—TOTAL LABOR FORCE AND DISTRIBUTION BY MAJOR INDUSTRY, SELECTED COUNTRIES

| Country | Year | Total Labor Force (000) | Per Cent Distribution by Major Industry | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | Agriculture | Mining | Manufacturing | Construction | Commerce and Communication[a] | Services[b] | Other[c] |
| Argentina | 1947 | 6,446 | 25.2 | 0.5 | 22.1 | 5.2 | 19.3 | 21.8 | 5.9 |
| Australia | 1954 | 3,702 | 13.5 | 1.7 | 28.0 | 8.9 | 27.6 | 20.3 | 0 |
| Belgium | 1947 | 3,449 | 12.3 | 5.6 | 38.0 | 5.7 | 20.5 | 15.0 | 2.9 |
| Canada | 1957 | 5,716 | 14.8 | 2.1 | 26.4 | 7.7 | 26.9 | 21.8 | 0.3 |
| Denmark | 1950 | 2,063 | 25.1 | 0.2 | 26.1 | 6.4 | 20.3 | 20.8 | 1.1 |
| France | 1954 | 18,824 | 27.7 | 2.2 | 26.2 | 7.2 | 19.4 | 16.0 | 1.3 |
| Germany | 1955 | 24,178 | 27.8 | 4.0 | 29.8 | 8.1 | 13.2 | 13.3 | 3.8 |
| India | 1951 | 101,775 | 70.5 | 0.6 | 9.0 | 1.1 | 7.7 | 11.1 | 0 |
| Italy | 1956 | 20,092 | 32.8 | 25.4 | | 7.8 | 30.5 | | 3.5 |
| Japan | 1955 | 39,554 | 41.7 | 1.4 | 17.6 | 4.6 | 20.3 | 14.4 | 0 |
| Mexico | 1955 | 9,603 | 57.8 | 1.2 | 11.7 | 2.7 | 10.7 | 10.8 | 5.1 |
| Netherlands | 1947 | 3,708 | 20.1 | 1.4 | 24.9 | 7.3 | 21.2 | 22.4 | 2.7 |
| Norway | 1950 | 1,388 | 25.9 | 0.7 | 25.8 | 9.3 | 20.9 | 16.9 | 0.5 |
| Philippines | 1956 | 9,497 | 54.1 | 0.2 | 11.5 | 2.4 | 11.3 | 10.5 | 10.0 |
| Sweden | 1950 | 3,105 | 20.4 | 0.5 | 31.5 | 7.9 | 21.1 | 17.9 | 0.7 |
| Switzerland | 1950 | 2,156 | 16.5 | 0.3 | 38.2 | 8.1 | 16.3 | 19.8 | 0.8 |
| United Kingdom | 1955 | 23,281 | 4.4 | 3.7 | 40.3 | 6.3 | 19.5 | 24.8 | 1.0 |
| United States | 1955 | 65,847 | 10.2 | 1.3 | 28.4 | 4.7 | 28.4 | 23.0 | 4.0 |

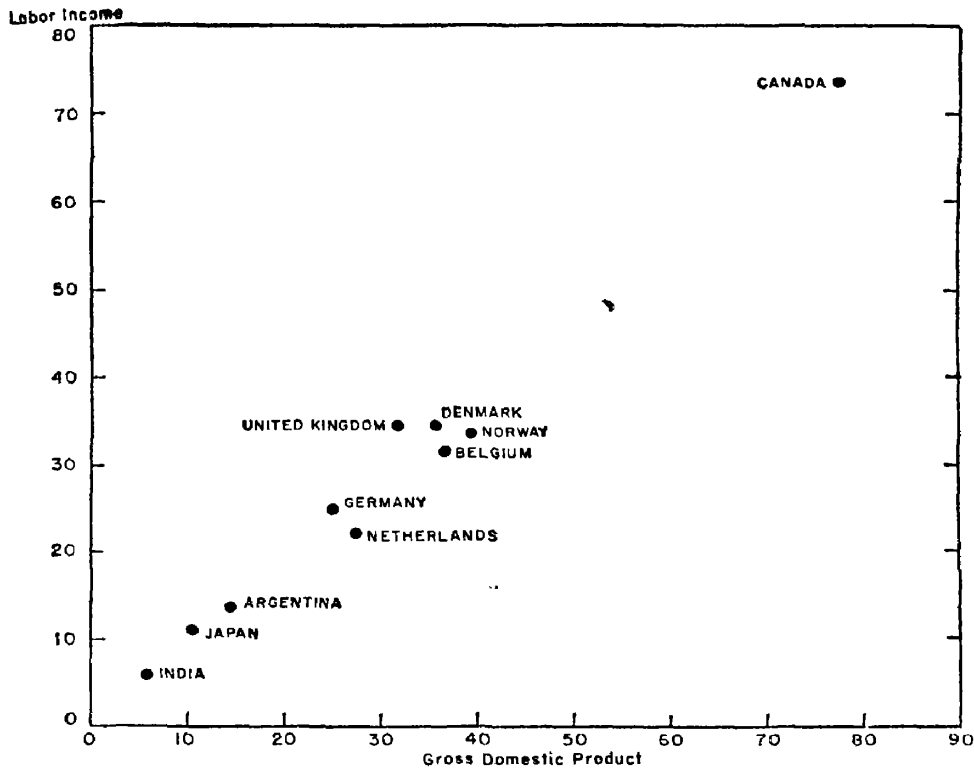
Labor force excludes members of armed forces.

[a] Includes transportation and storage.

[b] Includes gas, electricity, water and sanitary services.

[c] Not described, unemployed or persons seeking work for first time.

*Source:* *I.L.O. Year Book of Labour Statistics*, Geneva 1957, and current statistical reports from individual countries.

CHART 2. MANUFACTURING LABOR INCOME AND OUTPUT PER WORKER RATIOS TO UNITED STATES, 1955

United States Department of Labor, Bureau of Labor Statistics.

## II. *Labor Force Analysis*

Tables 5 and 6 summarize some of the labor force data which help to explain differences in wage and salary levels which are not directly explained by differences in national product among various countries. For example, as was pointed out in the discussion of Table 1, the high ratios of average wages and salaries to per capita national product in countries like Japan and the Philippines are probably in large part due to the very high proportion of the population who are entrepreneurs or family workers in lower-income agricultural pursuits.

It should be recognized that the labor force data may not be strictly comparable from country to country due to differences in details of definition, methods of data collection, and tabulation.[6] The data shown

[6] The total labor force or "economically active" population in each country, for example, may differ because of age limitations—that is, in some countries only those persons who have obtained a certain minimum age are included in the statistics of economically active population even though they may be gainfully occupied at lower ages, while in other countries there is no such limitation. In some cases it is also not clear to what extent unpaid family workers who assist in family enterprises are included in the economically active

TABLE 6—TOTAL POPULATION AND LABOR FORCE BY SEX

| Country | Year | Per Cent of Total Population in the Labor Force | | | Females as Per Cent of Labor Force |
|---|---|---|---|---|---|
| | | Males | Females | Both | |
| Argentina | 1947 | 63 | 17 | 41 | 20 |
| Australia | 1954 | 63 | 19 | 41 | 23 |
| Belgium | 1947 | 63 | 19 | 41 | 24 |
| Canada | 1956 | 54 | 17 | 36 | 22 |
| Denmark | 1953 | 65 | 34 | 50 | 34 |
| France | 1954 | 61 | 30 | 45 | 35 |
| Germany | 1956 | 67 | 35 | 50 | 36 |
| India | 1951 | 54 | 24 | 39 | 16 |
| Italy | 1956 | 63 | 20 | 41 | 26 |
| Japan | 1955 | 56 | 34 | 45 | 42 |
| Mexico | 1955 | 57 | 9 | 32 | 14 |
| Netherlands | 1947 | 61 | 20 | 40 | 24 |
| Norway | 1950 | 65 | 20 | 42 | 24 |
| Philippines | 1956 | 54 | 34 | 44 | 40 |
| Sweden | 1950 | 65 | 23 | 44 | 26 |
| Switzerland | 1950 | 67 | 26 | 46 | 30 |
| United Kingdom | 1951 | 67 | 27 | 46 | 31 |
| United States | 1950 | 58 | 22 | 40 | 28 |

*Source: I.L.O. Year Book of Labour Statistics.* Geneva 1957.

in Tables 5 and 6, therefore, can only be considered as indicators of some of the reasons for differences in wage structures and levels among nations.

It does not appear as though the rate of participation in the labor force (Table 6) has any great bearing on the relationship between per capita product and wage levels. Countries with comparatively high percentages of the population in the labor force like Denmark, Germany, the United Kingdom, and Switzerland, show different ratios of wages to national product, both with respect to one another and in relationship to the United States (Tables 1 and 3); similarly, countries with low rates of participation like Canada, Mexico, and India show different ratios. There also does not appear to be any relationship between the absolute or per capita levels of national product and the rate of participation in the labor force, either of females or of both sexes.

population. In some countries the census data on the labor force refer to the actual position of each individual on the day of the census or during a brief specific period such as the week immediately prior to the census data, while in other countries the data recorded refer to the usual position of each person. The percentages relating to females, in particular, are frequently not comparable internationally, since in many countries relatively large numbers of women who assist on family farms or other family enterprises without direct pay may or may not be counted as members of the labor force.

There does, however, appear to be some relationship between per capita gross domestic product and the degree to which the labor force is concentrated in manufacturing and mining. Generally, the countries which have the lowest concentration of labor force in manufacturing and mining have the lowest per capita product. This may in large part merely reflect the fact that agriculture in these countries is frequently substandard in terms of soil or human productivity or that small subsistence-type family farms are an important part of the economy in these nations. The countries which have a relatively high concentration of the labor force in agriculture and low total domestic output are India, Philippines, Japan, and, at a somewhat higher level, Italy. The countries with high concentrations of the labor force in manufacturing and mining, and with relatively high output for the total economy are the United Kingdom, Belgium, Switzerland, Germany and Sweden.

## III. *Payments for Labor in Individual Industries*

Table 7 presents data on payments for labor in selected export industries in the United States and in 16 leading U.S. supplying nations.[7] In order to eliminate the problems of exchange rates, domestic purchasing power, differences in consumption levels, different time periods (hour, day, week, and month) and some of the differences in supplementary payments among countries, the earnings data for each country have been expressed as a ratio to average earnings in manufacturing in that country. Although this procedure eliminates many of the problems inherent in intercountry comparisons, it must be recognized that in the absence of complete industry data, it was necessary to assume equal supplementary and nonwage payments among industries within each country. Such fragmentary data as are available indicate that such an assumption is contrary to fact, but no satisfactory alternate method was available.

As the earlier portion of this analysis indicated, no country studied showed up with a general wage level markedly out of line when compared with the total national product of that country. If payments for labor in a specific industry in one of these countries were to be higher than average payments in manufacturing in that country, it would seem unlikely that the payments in that industry could normally be called "substandard" as far as international trade is concerned. The case of an industry where payments are lower than the average in all manufacturing would indicate that further investigation to determine whether such payments were "substandard" might be warranted. The determination of the level to be regarded as substandard must

[7] Data are not available for the Philippines.

TABLE 7—RATIOS OF AVERAGE EARNINGS OF LABOR IN MANUFACTURING IN SELECTED INDUSTRIES TO AVERAGE EARNINGS OF LABOR IN ALL MANUFACTURING IN EACH COUNTRY—17 COUNTRIES 1955

| Industry | Argentina | Australia | Belgium | Canada | Denmark | France | West Germany | India | Italy | Japan | Mexico | Netherlands | Norway | Sweden | Switzerland | United Kingdom | United States |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| All Manufacturing | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 |
| Lumber & wood products | — | 101 | — | 89 | — | — | — | — | — | 66 | — | — | — | 90 | — | — | 89 |
| Furniture & fixtures | — | — | — | — | — | — | — | — | — | 71 | — | — | — | — | — | — | 86 |
| Stone, clay, & glass products | — | — | 97 | 102 | — | — | 93 | 67 | 89 | 104 | — | — | 102 | — | — | 95 | 99 |
| Metals & machinery | 107 | 99 | 109 | — | 102 | 131 | 105 | — | 111 | 117 | — | 103 | 103 | 105 | 100 | 104 | 112 |
| Primary metals | — | — | 118 | 116 | — | 126 | 118 | 143 | 135 | — | 148 | — | — | 105 | — | — | 119 |
| Fabricated metal products | — | — | 97 | — | — | — | — | — | — | 98 | — | — | — | 101 | — | — | 105 |
| Primary & fabricated | 97 | — | — | — | — | — | — | — | — | — | — | — | — | — | — | — | 113 |
| Machinery | — | — | — | — | — | 131 | — | — | — | — | — | — | — | — | — | — | 106 |
| Electrical | 142 | — | — | — | — | — | — | — | 106 | 108 | — | — | — | 104 | — | — | 100 |
| Nonelectrical | 108 | — | — | — | — | — | — | — | 103 | 98 | — | — | — | 104 | — | — | 111 |
| Transportation equipment | — | — | — | — | — | — | — | — | 123 | — | — | — | — | — | — | — | 119 |
| Machinery & transportation equipment | — | — | 106 | — | — | — | 102 | — | — | — | — | — | — | — | — | — | 111 |
| Instruments & products | — | — | 97 | — | — | — | — | — | — | 103 | — | — | — | — | — | — | 102 |
| Watches | — | — | — | — | — | — | — | — | — | — | — | — | — | — | 118 | — | 92 |
| Food & kindred products | 98 | 99 | 87 | 84 | 92 | 110 | 87 | — | 78 | 94 | 59 | 92 | 89 | 91 | 97 | 86 | 93 |
| Textile mill products | 93 | 98 | 88 | 76 | 93 | 102 | 85 | 102 | 83 | 63 | 89 | 103 | 90 | 85 | 89 | 91 | 74 |
| Apparel & finished textiles | — | — | — | 68 | — | 90 | 90 | — | — | 53 | — | — | — | — | — | 92 | 72 |
| Paper & allied products | — | — | — | 117 | — | — | — | — | — | 132 | — | — | — | 100 | — | — | 97 |
| Chemicals & allied products | 91 | — | 105 | 105 | 90 | 133 | 104 | 82 | 108 | 122 | — | 101 | 107 | 99 | 108 | 98 | 106 |
| Leather & products | 110 | — | — | — | — | — | — | — | — | — | — | — | — | — | — | 92 | 75 |
| Footwear | — | — | — | — | — | — | — | — | — | — | 75 | — | — | — | — | — | 71 |

*Source:* *I.L.O.;Year Book of Labour Statistics*, Geneva 1957, Tables 17 and 18

vary from country to country and industry to industry in relation to its over-all income level. The most that the material contained in Table 7 can do is point out cases which may be worth further investigation.[8]

In such further investigation it should be recognized that there are differences in wage levels among industries within any country—these differences reflecting such factors as skill differentials, training or experience, custom, prestige values or danger, organization of both labor and management, capital investment, net productivity per worker, and the rate of growth of the industry. The purpose of this analysis is to demonstrate a method by which, with the currently available data, one may throw light on the question whether imports in some instances represent "unfair" competition to American industry. A first step involves a comparison of payments for labor per worker and gross domestic product per member of the labor force in the country involved and in the United States, and then a distribution of labor payment levels by industry in the exporting country as compared to the United States. Under this procedure, the basis for comparison is not only whether payments for labor in a foreign industry are low in comparison with the general level of payments in manufacturing in that country, but whether they are significantly higher or lower in comparison with the wage ratio for the same industry in the United States. This comparison is presented in Table 8 which is based on ratios of ratios.

In Table 8, for each of 16 countries, the ratio of earnings in each industry to the appropriate country average for all manufacturing has been divided by the similar ratio for the United States. These data are arrayed by industry in order of magnitude of the U.S. earnings ratios, starting with U.S. industries with highest hourly earnings. The general limitation concerning nonuniformity of supplementary payments mentioned earlier applies even more directly in this portion of the analysis. That is, if supplementary payments in a specific industry in Japan, for example, are proportionately higher than the average for other Japanese industries, and are also a more important share of net payments for labor in Japan than they are in the U.S. industry, the data shown in Tables 7 and 8 will understate the comparative wage level in the Japanese industry as compared to the U.S. industry.

Tables 7 and 8 indicate that in the vast majority of the foreign industries, wage levels are not low in comparison with the country's

[8] This type of analysis is necessarily limited to fairly broadly defined industries; it can not cover homework, cottage, or other indigenous craft operations which may be substandard, although vital to the economy of the specific country. In general, products of these workers would not bulk large in international trade.

TABLE 8—RATIOS OF EARNINGS IN SELECTED INDUSTRIES TO EARNINGS IN ALL MANUFACTURING IN 16 COUNTRIES EXPRESSED AS RATIOS TO CORRESPONDING U. S. RATIOS—1955

| Industry | U.S. Earnings Ratio to All Manufacturing | Ratios to U. S. Ratios | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Argentina | Australia | Belgium | Canada | Denmark | France | West Germany | India | Italy | Japan | Mexico | Netherlands | Norway | Sweden | Switzerland | United Kingdom |
| Primary metals | 119.1 | — | — | 99 | 97 | — | 106 | 99 | 120 | 113 | — | 125 | — | — | 88 | — | — |
| Transportation equipment | 118.6 | — | — | — | — | — | — | — | — | 103 | — | — | — | — | — | — | — |
| Total metals & machinery | 111.7 | 96 | 88 | 97 | — | 91 | 117 | 94 | — | 100 | 104 | — | 92 | 92 | 94 | 90 | 93 |
| Nonelectrical machinery | 111.2 | 97 | — | — | — | — | — | — | — | 92 | 88 | — | — | — | 94 | — | — |
| Machinery & transportation equipment | 111.2 | — | — | 95 | — | — | — | 92 | — | — | — | — | — | — | — | — | — |
| Total machinery | 106.4 | — | — | — | — | — | 123 | — | — | — | — | — | — | — | — | — | — |
| Chemicals | 105.9 | 86 | — | 100 | 99 | 85 | 125 | 98 | 77 | 102 | 115 | — | 95 | 101 | 94 | 102 | 92 |
| Fabricated metal products | 105.3 | — | — | 92 | — | — | — | — | — | — | 93 | — | — | — | 96 | — | — |
| Instruments | 101.6 | — | — | 96 | — | — | — | — | — | — | 102 | — | — | — | — | 128 | — |
| Electrical machinery | 100.0 | 142 | — | — | — | — | — | — | — | 106 | 108 | — | — | — | 104 | — | — |
| Stone, glass, clay | 98.9 | — | — | 98 | 103 | — | — | 94 | 67 | 90 | 105 | — | — | 103 | — | — | 96 |
| Paper and products | 97.3 | — | — | — | 120 | — | — | — | — | — | 136 | — | — | — | 103 | — | — |
| Food, beverage, tobacco | 93.1 | 106 | 106 | 94 | 91 | 98 | 118 | 94 | — | 84 | 101 | 63 | 99 | 95 | 97 | 104 | 93 |
| Lumber & wood products | 89.4 | — | 113 | — | 100 | — | — | — | — | — | 73 | — | — | — | 101 | — | — |
| Furniture & fixtures | 86.2 | — | — | — | — | — | — | — | — | — | 82 | — | — | — | — | — | — |
| Leather & products | 75.0 | 146 | — | — | — | — | — | — | — | — | — | 105 | — | — | — | — | 123 |
| Textile mill products | 73.9 | 126 | 132 | 119 | 103 | 126 | 139 | 115 | 137 | 112 | 85 | 120 | 139 | 122 | 115 | 120 | 122 |
| Apparel and finished textiles | 71.8 | — | — | — | 94 | — | 126 | 125 | — | — | 73 | — | — | — | — | — | 129 |

*Source:* Derived from Table 7.

total ability to pay and the U.S. pattern of wage levels. Of the 108 industries for which data are available, more than half (56 cases) have wage levels which rank equal to or higher than the comparative figure for the United States. An additional 40 cases are less than 10 per cent below the comparable U.S. ratios to all manufacturing, and only 12 cases are more than 10 per cent lower than the U.S. ratios.[9]

Although this paper is not designed to go beyond the stage which has been reached in Table 8, the necessity of further research has been clearly indicated. The technique developed here has shown that there does not seem to be evidence of substandard wages as a major factor in the flow of most goods from the leading trading nations of the free world into the United States. It also provides a method of pinpointing those cases where there may possibly be some element of substandard wages involved and where intensive analysis of payments to labor in competitive industries might be productive. At this stage we can only indicate lines of attack further to refine the statistical analysis. One such is to obtain better data on nonwage costs. For example, consider a specific foreign industry which had nonwage costs averaging 20 per cent while the general average for all manufacturing in that country was 10 per cent and the nonwage elements in the United States were 10 and 12 per cent respectively. If the U.S./foreign industry ratio unadjusted for the differences in nonwage costs stood at 85, addition of these nonwage payments would raise the ratio to almost 95.

Possible further refinements would deal with the sex ratios in the domestic and foreign industries. If female workers are generally lower-paid than male workers, a heavier concentration of female workers in the foreign industry than in the domestic industry would create an unfavorable foreign/U.S. ratio despite the fact that the ratios of females/females and males/males might be favorable.

[9] Table 8 does appear to show some pattern indicating that the range of labor payments among industries is somewhat narrower in the countries studied here, than the range in the United States—that is, the ratio of ratios is high for the lower-wage U.S. industries and low for the higher-wage U.S. industries. How much of this comparatively narrower range is real and how much of it would be eliminated if it were possible to add all other payments not directly related to time on the job—bonuses, cost-of-living and family allowances, social insurances, etc.—is not determinable.

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# WINDFALL INCOME AND CONSUMPTION

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In this paper the reaction of consumer units to a temporary income change is explored. The data are used to test a critical strand of Milton Friedman's permanent income hypothesis, though the results are interesting by themselves. In the concluding section, an implication of the analysis for tax policy is also discussed.

## I. *The Permanent Income Hypothesis and the Proposed Test*

Friedman, in a recent monograph [2], has set forth a theoretical explanation of consumption behavior. Under his permanent income hypothesis, both actual consumption and actual income are broken up, at least conceptually, into permanent and transitory components. The permanent component of income is that portion of an individual unit's income that the unit regards, consciously or behaviorally,[1] as permanent. The transitory component, which may be either positive or negative, is that part of income produced by influences that the individual unit regards as random. Permanent and transitory components of consumption are distinguished analogously. It is assumed that the permanent component of income is uncorrelated with its

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This article is based on research undertaken in connection with a broad study of consumer expenditures, incomes and savings at the Wharton School of Finance and Commerce, University of Pennsylvania. The study, which is financed by a grant from the Ford Foundation, is based largely on the 1950 survey of the Bureau of Labor Statistics of 12,500 families in 91 representative cities. The principal purpose of this survey was the revision of expenditure weights in the Consumer Price Index. 1500 items of budget information have been tabulated in 18 volumes of statistical tables. The data are believed to be more reliable for middle-income than for the very lowest and the very highest, and for wages and salaries than for other types of income. In any statistical investigation, there are sampling, reporting, and processing errors. Hence analytical results, parameter estimates, and derived relationships must be interpreted cautiously. For more complete description and evaluation of the survey, see [7].

[1] The definition on a behavioral basis would appear to be a better definition in the real (uncertain) world. This would appear to be Friedman's view, as he asserts several times that the permanent component of income cannot be measured for the individual unit.

transitory component; this is principally a definitional postulate. A similar relationship is assumed to hold between permanent consumption and transitory consumption. Friedman asserts, on the basis of theoretical considerations, that permanent consumption is a constant function of the level of permanent income. This is the first major substantive conclusion of the hypothesis. (The constant connecting permanent consumption, $c_p$, with permanent income, $y_p$, depends upon several variables—the interest rate, $i$, the ratio of nonhuman wealth to total wealth, $w$, and the portmanteau taste variable, $u$—but it does not depend, in this model, on the level of permanent income.[2]

Friedman further postulates that transitory income and transitory consumption are uncorrelated. This is a crucial postulate, for it implies that transitory income does not give rise to consumption (of either type) on a systematic basis. Transitory income cannot give rise to transitory consumption, for the two are uncorrelated, nor to permanent consumption, for transitory income and permanent income (and hence transitory income and permanent income's constant multiple, permanent consumption) are uncorrelated. Hence consumption and transitory income are uncorrelated. The empirical evidence to be presented is directed toward this proposition.[3]

One point should be made clear. Consumption, according to this model, is defined as the value of services enjoyed by the unit during the period under consideration. Thus consumption becomes expenditure on nondurable goods plus the rental of the services of durable goods. In the customary one-year accounting period, purchases of durable goods would be mainly saving, not consumption, since only a small part of the services rendered by a durable good during its lifetime is consumed during the year. Thus expenditures on both durable and nondurable goods would give too high an estimate for a unit's consumption, as they would contain an element of saving. There is always an offset to this bias, because of services rendered by durable goods purchased in previous time periods. Still, during years when stocks of consumer durables are rising, as Friedman points out, the conventional definition of consumption expenditures overstates consumption as defined for his theory.

[2] In equation form, we have:

(1) $c_p = k \cdot y_p$, where $k = k(i, w, u)$.

[3] The first conclusion (that permanent consumption is a constant function of the level of permanent income) has already been subjected to a number of empirical tests. See [1], [3], [4] and [6]. In general, the evidence presented in these papers does not constitute strong support for the first substantive conclusion of the permanent income hypothesis. The second conclusion (that transitory income and consumption are uncorrelated) has been subjected to far fewer tests. The paper by Klein and Liviatan [5] is the best previous test of this second substantive conclusion of which I am aware.

In their paper [5], Klein and Liviatan used the 1953 and 1954 Savings Survey data for Great Britain to compute regressions of the savings-income ratio against several explanatory variables. One of these was $W/Y$, the proportion of total income received in the form of "windfall income." Windfall income consisted of life insurance benefits, gambling winnings, cash gifts, cash legacies, postwar credits, and other lump-sum transfers of money. The results are rather interesting. For retired and unoccupied units, the implied marginal propensity to consume out of windfall income is .92; for lower-income employees, this figure is .65; and for upper-income employees it is .74. If windfall income were defined in these regressions in the same way as in the permanent income hypothesis, this would constitute evidence tending to discredit this strand of the hypothesis.

In point of fact, no strong identification between these two concepts can be made. Friedman's definition of transitory income is income which is viewed by the recipient unit as the result of chance or accidental factors. Now life insurance benefits would not fall into this category. Similarly, there is every reason to expect that the expert gambler considers his gambling winnings "permanent income"; he is likely to count on these as a supplementary source of income. Heirs also have been known to anticipate or count on legacies. To the extent that this is the case, the legacies would not represent windfall income but would represent a final transfer of an asset that was a contingent asset for many years. Thus the results of this study, while suggestive, do not constitute evidence seriously damaging to the permanent income hypothesis.

We now seek to find a stronger test of the result that consumption and transitory income are uncorrelated. In November 1949 the Bureau of the Budget announced that National Service Life Insurance dividends would be paid out, in fiscal 1950, to veterans of the second world war who had held military insurance. Payments began January 16, 1950; three-quarters of the dividends had been paid out by the end of March. These dividends amounted to $2.8 billions; the average payment per veteran was $175 [8]. The payments were presumably made possible by more favorable mortality experience than had been allowed for in setting the premium levels. They would represent, from the point of view of the permanent income hypothesis, transitory income: before the dividend was announced, the dividend recipients had no inkling that such a payment would be made. The dividend payment represented a cost saving on an item of consumption (life insurance) for which an individual consumer would have believed that he had been charged adequately. Because of the short time-lag between the public announcement and the payment, there is no

reason to believe that this income was anticipated in prior years.[4]

The goal is to isolate the influence of this factor on the consumption behavior of the units fortunate enough to receive this additional source of revenue. In the 1950 Bureau of Labor Statistics Survey of Consumer Expenditures, data on individual household units showing the size of income primarily from this source alone have been collected. By computing multiple regressions, we can attempt to isolate the influence of these dividend payments[5] on the consumption of units receiving this windfall. In particular, we can compare the results obtained with the prediction that the permanent income hypothesis gives and with the prediction that another explanation of consumption behavior, an absolute income hypothesis, would give. The prediction on the basis of an absolute income hypothesis is relatively simple. From the work of various researchers in this field, we might expect that the marginal propensity to consume out of windfall income would lie between .6 and .9, if windfall income is substantially the same as regular income in its effect on consumption behavior.

On the other hand, a prediction based on the permanent income hypothesis is somewhat more difficult. In his book [2, p. 215] Friedman appears to agree that this dividend payment constitutes a definite test of the permanent income hypothesis. In discussing this test, Friedman argues that the regression coefficient (the marginal propensity to consume out of this transitory income) should be "small, about .3 or so." He arrives at this theoretical prediction in the following manner. From empirical data, Friedman estimates that the time horizon of the individual household units, in the real (uncertain) world, is approximately three years. Because of this, approximately one-third of the windfall will enter the unit's permanent income stream. Thus, average propensity to consume out of permanent income (which has been found to be roughly .9) multiplied by one-third the dividend

[4] Suppose the windfall receipt was anticipated at a prior point in time. At what point of time does this unusual source of income become permanent income? Friedman is not too clear on this point, but it would appear that he assumes that on a behavioral basis the unusual income receipt becomes permanent income only at the time of payment. Thus his theory would predict a normal marginal propensity to consume out of that portion of the unusual receipt that is deemed permanent income (typically one-third) in this case. On the other hand, if the unusual receipt becomes permanent income at the time when the recipient learns about it, then the results may be different. For then consumption expenditures might be expected to increase in time periods prior to the actual payment, in anticipation of this unusual receipt. But, in the period of payment itself, consumption expenditures might be expected to be not much above normal, as the unusual income receipt would probably be used to pay off debts or to replenish savings which the unit had, as it were, borrowed from itself. Hence the theory of consumption behavior under review might appear to predict in this case that for the period of payment, the unusual receipt would be, on the average, largely saved.

[5] These dividend payments were not uniform; some units received larger payments than others.

income will give an estimated MPC out of dividend income of .3.[6] The permanent income hypothesis does, however, allow for a wealth effect, which becomes appreciable if the windfall is large enough. Thus the ratio ($w$) of nonhuman wealth to total wealth (which may be interpreted as the ratio of nonhuman wealth to permanent income) may be increased, effecting a reduction in the urgency to save to provide a reserve against contingencies. Hence a higher average propensity to consume out of permanent income may result. However, the average value in the sample for dividend income received was approximately $250, an amount relatively small in terms of either permanent income or nonhuman wealth. Hence the wealth effect will be, in all likelihood, small, and only minor reservations are engendered by this possible complication.

## II. *Results of the Present Investigation*

A subsample of 1414 dividend recipients was selected. This subsample included only 2-4 person families, where the age of the head of the family was between 21 and 45, and it excluded entrepreneurs and the unemployed. This is a group whose consumption behavior, from the point of view of the permanent income hypothesis, should be relatively homogeneous. Exclusions were made on no other basis. Twin regressions were computed: for the first regression, consumption was defined to include expenditure on consumer durables. As mentioned earlier, this is probably too large a measure of consumption, as defined in the permanent income hypothesis, when the stock of consumer durables is increasing.[7] For the second regression, consumption was defined to exclude expenditure on consumer durables. This is too small a measure of consumption as defined in the permanent income hypothesis. Nevertheless, if we are willing to postulate continuity in our

[6] Irwin Friend has raised the question whether a different value of the MPC out of windfall income is theoretically more appropriate. The dividends were received in a single lump payment, and the recipient had no basis to expect that this source of income would ever be repeated. Consequently it seems a bit arbitrary to say that one-third of the dividend receipt entered the permanent income stream for the year in question, owing to the haziness of the future and the shortness of households' time horizon. A strict interpretation of the permanent income hypothesis might entail considering all of the dividend receipt as transitory income; in this case, the predicted marginal propensity to consume out of this windfall would be close to zero, with only random variation about this average figure. However, since Friedman's hypothesis was under consideration, Friedman's theoretical prediction was used as the basis for performing the test. It is worth observing that the two hypotheses offer widely differing values of the MPC in question (.3 as an upper limit for the permanent income hypothesis contrasted with .6 as a lower limit for an absolute income hypothesis).

[7] This assumption may break down for particular subclasses. Although the stock of consumer durables may be increasing for the group as a whole, there is no logical reason why it should be increasing for every subclass within the group. The error introduced is, however, likely to be small. Consequently the discussion will be based on the postulate in the text.

data, then we have a solution. Suppose both measures of consumption lead to the same conclusions. Then we can assert that the intermediate measure of consumption, giving intermediate results, will also lead to the same conclusions.

The following regression was obtained for the 1414 families in our sample:

$$c = \$964 + \underset{(76)}{} \underset{(.017)}{.747y} + \underset{(.145)}{.966d} \qquad S_u = \$968 \qquad R^2 = .601 \tag{2}$$

For this equation, $c$ is total family consumption including expenditure on durable goods, $y$ is family income after taxes and not including the $d$ variable, and $d$ is bonuses, mustering-out pay, war insurance refunds, and military unemployment insurance.[8]

The bulk of the $d$ variable is the National Service Life Insurance dividend payments. The average value of the $d$ variable in the sample was $255; the average value of the dividend payments in the universe was approximately $175. One possible explanation of this discrepancy, which is much too large to be attributed to sampling variability alone, is the method of selection of the subsample. Thus the size of the dividend payment to any individual was primarily an actuarial matter. It is quite likely that married dividend recipients carried greater amounts of insurance (or had them in force for a longer period of time) than single veterans. Hence there may have been a larger payment, on the average, to members of the subsample group than to single veterans. For this reason, one might expect to find a larger average dividend payment in the selected subsample. Furthermore, the data in the sample are on a household basis, while the universe average is a per-person figure. Hence the possibility that there might be more than one veteran per household, raising the sample average, exists. Of course, other miscellaneous items of military income included in the $d$ variable may also be contributing to increase the sample average. In any case, these considerations suggest that the greater part of the $d$ variable is the dividend payments previously discussed.

The average values in the sample of $c$ and $y$ are $4087 and $3853, respectively. The partial correlation coefficient between total family consumption and dividend income, holding the influence of family income net of taxes and net of dividend income constant, is .174. As this is 6 times its own standard error, this measure is statistically significant. It would appear that dividend income has a noticeable effect on families'

[8] $S_u$ is the estimated standard deviation of the residuals, while $R^2$ is the coefficient of determination and may be interpreted as the fraction of "explained" variance of $c$. The numbers in parentheses are standard errors of the respective parameter estimates. All the parameters are at least 6 times their respective standard errors and hence are statistically significant.

consumption behavior. In particular, the best point estimate of the marginal propensity to consume (including expenditure on durables) out of windfall income is .97, with a standard error of .15.

This result, however, is not necessarily inconsistent with the permanent income hypothesis. It is possible that the greater part of these consumption expenditures were purchases of consumer durables. In this case, the greater part of these purchases out of windfall dividend income would be saving, as defined for this hypothesis. Furthermore, the small fraction of these purchases which could be considered as immediate consumption might give a value which would fall in the 0-.3 range suggested earlier as hypothetical values of the MPC predicted by the permanent income hypothesis.

For this reason, we are interested in the results obtained when consumption expenditures are netted of expenditures on durable goods. $c'$ denotes total family consumption not including purchases of durable goods. The average value of $c'$ in the sample was \$3301, and the following regression was obtained for the 1414 families in the selected subsample:

$$c' = \$959 + .560y + .723d \qquad S_u = \$725 \tag{3}$$
$$(57) \quad (.012) \quad (.109) \qquad R^2 = .601.$$

Thus, the best point estimate of the marginal propensity to consume nondurables out of this windfall income is .72.[9] The best point estimate of the marginal propensity to consume out of this windfall income, where consumption is defined exactly in terms of the permanent income model, would be some number intermediate between .72 and .97. It would be reasonable to expect that the true universe value of this parameter would lie in this range also. But in any case, the best point estimate of this marginal propensity to consume, consumption being defined precisely in terms of the permanent income model, is much higher than .3, the result predicted by the permanent income hypothesis. If we make use of the estimated standard errors of the regression coefficients, this discrepancy appears to be highly significant statistically.[10] Furthermore,

[9] For this regression also the sample parameters are 7 or more times their respective standard errors. Hence they are statistically significant. Once again, the partial correlation coefficient between $c'$ and $d$, holding the influence of $y$ constant, is .174 and is approximately six times its own standard error. Hence it can be considered statistically significant.

[10] Suppose we wish to test whether the (unknown) sample estimate of the theoretically correct MPC differs from its theoretically predicted value, .3, by more than can be reasonably attributed to sampling variability. Now this theoretically correct MPC would probably lie closer to the marginal propensity to consume nondurables, as most of the purchases of durable goods for a particular year would be savings, according to this model. Hence, for the sample value of the theoretically correct MPC, assume a value .75, which is close to the sample value of the marginal propensity to consume nondurables. As the estimated standard error of this sample parameter, choose .15, the larger of the two estimated standard errors of the sample regression coefficients. Thus .75 is the estimate from the sample of the marginal propensity to consume out of windfall income, when consump-

an absolute income hypothesis has no difficulty in explaining the observed results; .72 and even .97 are plausible values for marginal propensities to consume. It would appear that an absolute income hypothesis serves as a superior explanation for this body of data.

### III. *Possible Reservations*

It might be argued that these results are peculiar to 1950; the influence of scare buying due to the onset of the Korean war stands out as an obvious factor. In terms of the permanent income hypothesis, there may have been a positive mean component of transitory consumption. But these influences, if important, should have made their effects felt on the constant term in these regressions and on the coefficient of regular income (the marginal propensity to consume out of $y$). It does not appear that even these unusual influences suffice to explain the differentially higher spending of those who received greater amounts of dividend income. On the other hand, one can never infer very much from a single sample at one point of time. Since this was an unusual period, due caution in asserting that these results are of general applicability is advisable.

It also might be asserted that these results are peculiar to the particular sample. Often there is nothing that the investigator can do about this possible source of error, since even random sampling can produce a most unrepresentative sample in particular cases. However, there is something that can be done here. We can compare the regression results of this subsample with those of a second subsample of family units which did not receive war insurance dividend payments. This second subsample is also drawn from the BLS survey of consumer expenditures for 1950. Like the first subsample, it includes 2-4 person family units, in which the age of the head is from 21 to 45, and it excludes entrepreneurs and unemployed.

The second subsample contained a relatively large number of extreme cases, with incomes either above $10,000 or below $1000. It thus turned out that, for this group, the regression containing all incomes classes did not accurately represent consumption behavior in the central ranges of disposable income. Hence regressions of consumption on income for after-tax incomes from $1000 to $10,000 were computed. For purposes of comparability, regressions of consumption on income (net of the $d$ variable) and on war insurance dividend income were also computed for the same income classes of the first subsample (the veterans).

tion is accurately defined from the standpoint of the permanent income model. .3 is the theoretical value of this parameter. The difference between .75 and .3 is .45, which is three times .15. A discrepancy as large or larger would be observed as the result of chance forces only 3 times in a thousand. The values chosen above are conservative and tend, if they give rise to any bias, to understate the level of statistical significance.

For the second subsample (the nonveterans), for income classes \$1000-\$10,000, we have:

$$c = \$1091 + .757y, \tag{4}$$

$$c' = \$1014 + .583y. \tag{5}$$

For the first subsample (the veterans), for income classes \$1000-\$10,000, we have:

$$c = \$858 + .774y + .968d, \tag{6}$$

$$c' = \$902 + .576y + .70d; \tag{7}$$

$c$, $c'$, $y$, and $d$ are defined as they were previously.

The difference in the intercepts (whether we are comparing the first or second set of regressions) reflects possibly the higher mean income in the nonveteran subsample and/or the larger average family size of this group.[11] Nevertheless, both the marginal propensity to consume (including expenditure on durables) out of regular income and marginal propensity to consume (excluding expenditure on durables) out of regular income do not differ significantly between the two samples. (This statement is made on the basis of the estimated standard errors.) Hence it would appear that the results discussed above are not, in all likelihood, the result of an atypical sample.

There is a possibility that these results may be due to the special character of the dividend recipients selected in the first subsample. On the whole, the dividend recipients are a relatively young group (even when compared with the nonveteran group); they are drawn from a stratum of the population known to have a high propensity to consume. Hence it might be argued that these results might not be applicable to other elements of the population. In terms of the permanent income hypothesis, they might represent a group with a high permanent income relative to current or measured income. This group might not be consuming at levels warranted by their levels of permanent income, since it is rather difficult to borrow (after a certain point) against higher income expected in the future. Consequently a receipt of this kind might temporarily release the members of this group from the year-to-year budget restrictions on their consumption. Thus the windfall might be responsible for the increased consumption of this group only in so far as it allowed the recipient units to spend up to their permanent income. But if this were the case, then these results might not carry over to other groups in the population. These possible strictures constitute another reason why the relations and parameter estimates discussed above must be interpreted with due caution.

[11] It may merely reflect sampling variability. The standard error of the constant terms in equations (4) and (5) is approximately \$100. Hence the discrepancy between the constant terms of equations (5) and (7) does not appear to be statistically significant.

One final and perhaps most serious reservation remains to be discussed. In commenting on an earlier version of this paper,[12] Friedman raised the question whether the regression coefficients of the *d* variable (the estimated MPC's out of dividend income) might be, in some sense, spurious. There is the possibility that the *d* variable could display a covariation with indicators of permanent income, such as age, education, or family size. (This seems plausible when one recalls that the size of dividend payments to individual recipients was dictated by actuarial considerations and hence depended primarily on the size of the policy and the length of time the policy was in force. Thus it is quite likely that (for example) older individuals—those with higher permanent incomes, *ceteris paribus*, in the theory under review—received higher dividend payments.) If the *d* variable were correlated with indicators of permanent income, the regression coefficient of the *d* variable might be expected to be high. In the permanent income interpretation, this result might occur, not because dividend income systematically gives rise to consumption but because we had unwittingly classified the units studied by a variable that serves as a proxy for permanent income. The causality would then be reversed—the units would not display relatively high consumption because they had relatively high dividend income; instead, they would have high consumption and high dividend income precisely because they had high permanent income.

Nevertheless, matters are not so serious as might first appear. The *y* variable is of course not the same thing as permanent income but it is highly correlated with it. Therefore, it might also be strongly correlated with the indicators of permanent income. Since the regressions of consumption (according to the two definitions) were run against both regular income and dividend income, establishing the existence of a correlation between the *d* variable and an indicator of permanent income is insufficient to establish a bias in the estimate of the MPC out of dividend income. Only the existence of a correlation between *d* and an indicator of permanent income, after the influence of measured income has been eliminated, i.e., the existence of a positive partial correlation coefficient, will suffice to establish a bias, as Friedman points out. If, for example, there is a positive correlation between dividend income and age and a correlation of greater magnitude between regular income and age, the partial correlation coefficient between age and dividend income could be small and the extent of the bias negligible. All of this discussion, however, merely constitutes conjecture. The appropriate manner of re-

[12] Milton Friedman's comments on an earlier draft of this paper were made at the Conference on Consumption and Saving, held at the University of Pennsylvania, March 30-31, 1959. Friedman of course bears no responsibility for anything which appears in this paragraph or the one immediately following, although I have attempted to represent his position accurately.

solving this difficulty is to compute the regression of consumption (defined in both ways) against regular income, dividend income, and several indicators of permanent income. This will be done and the results presented at a later date. If the regression coefficient were to decrease (and decrease sufficiently), this would constitute evidence favorable to this strand of the hypothesis. Still, granting that regular income is by itself a rather good proxy for permanent income, one might not expect the introduction of additional variables into the multiple regressions to yield substantially lower estimates of the MPC out of dividend income.[13]

One possible indicator of permanent income is the age of the household head; for the group studied, it is safe to assume that permanent income increases, on the average, with this variable. Accordingly, age of the household head, $A$, was introduced into the previously employed regression equations. The following results were obtained for the 1414 members of the veterans subsample:

$$(8) \quad \underset{(156)}{c = \$990} + \underset{(.017)}{.747y} + \underset{(.145)}{.968d} - \underset{(4.97)}{.944A} \qquad R^2 = .601, \quad S_u = \$968,$$

$$(9) \quad \underset{(116)}{c' = \$610} + \underset{(.013)}{.550y} + \underset{(.108)}{.707d} + \underset{(3.7)}{12.7A} \qquad R^2 = .604, \quad S_u = \$722.$$

A comparison of equation (8) with equation (2) and equation (9) with equation (3) suggests that the regression coefficients of $d$ (the estimated marginal propensities to consume out of windfall dividend income) are virtually unaffected. Hence it is not likely that dividend income received by an individual is serving as a proxy for that individual's permanent income status. Other indicators of permanent income ought to (and will) be tried, however, before this conclusion can be definitely accepted.

It is interesting to compare the marginal propensities to consume out of the windfall dividend income with the marginal propensities to consume out of regular income. (The possibility of bias in the estimates

[13] In his discussion, Friedman suggested that the best point estimate, from the data, of the marginal propensity to consume nondurables out of dividend income would be .48, after performing calculations designed to eliminate the possible bias discussed in the text. (Performing a similar set of computations lowers the estimate of the marginal propensity to consume both nondurables and durables out of windfall income from .97 to .85.) It should be pointed out that .48 is still much closer to .56 (the estimated marginal propensity to consume nondurables out of regular income) than it is to .30 (the hypothetical value predicted by the permanent income hypothesis, now taking all expenditures on durables as savings.) Still, this discussion need not be carried further. As indicated in the text, the best method of resolving this complication is to introduce additional variables into the multiple regression.

of the MPC's out of dividend income, discussed in the preceding paragraphs, is now neglected.) Two interpretations of these results are possible. The first is that there is a higher marginal propensity to consume, for the total population, out of windfall income than there is out of regular income. In one view, this is a reasonable result; no provision for contingencies or future expenditures required before the next income payment arrives need be made out of this income source. The second interpretation is that the dividend recipients merely received some additional income, some of which they spent. Reasoning in this manner, one would assert that the discrepancy between the two MPC's (from the same regression equation) merely reflects the chance variations of sampling. Hence a statistical test of significance is in order. For the first regression (where consumption is defined to include expenditure on consumer durables), the MPC out of the dividend income is .97, while the MPC out of regular income is .75. The difference between these two parameter estimates is, therefore, .22. The estimated standard error of the difference (taking account of an estimated covariance between these two parameter estimates) is .147. The numerical value of $t$, the ratio of the difference of these two parameter estimates to the estimated standard error of this difference is approximately 1.49. Chance variations would give rise to a discrepancy as large or larger 14 times out of 100.

If we use the second regression (in which consumption expenditures are defined to exclude expenditure on durable goods), the results are quite similar. In this case, the difference between the MPC out of the windfall dividend and the MPC out of regular income is .163. The estimated standard error of the difference is .111. The numerical value of $t$ is, accordingly, 1.47. Thus the level of statistical significance is nearly the same as before; in this case, the discrepancy is significant at the 14.2 per cent level. It would appear that we cannot conclude, on the basis of the data presented here, that in the entire population the MPC out of windfall income exceeds the MPC out of regular income. This might be merely due to an inadequate (for this purpose) size of sample. In any case, the observed difference between the two kinds of MPC's, while suggestive, does not appear to be statistically significant.

## IV. *Concluding Remarks*

In summary, there appears, in spite of the reservations discussed above, to be a strong tendency to spend windfall income. This propensity does not appear to exceed the marginal propensity to consume regular income. Nevertheless, the true (population) value of the marginal propensity to consume out of windfall income is, in all likelihood, quite high. This has rather interesting implications for fiscal policy. In

particular, it suggests that even a temporary tax cut would be effective in alleviating some of the hardships of a recession. This is a point that was widely debated in the 1957-58 downturn. The basis of this assertion is that these data suggest that a temporary tax rebate would have its desired effect, i.e. that it would induce about as much consumer spending as a decrease in taxes expected to be permanent.[14]

Finally, it would appear that consumption and windfall income are far from uncorrelated for this body of data. Hence an important theoretical result of the permanent income hypothesis appears to be seriously questioned. The marginal propensity to consume out of windfall income does not appear to be appreciably lower than the marginal propensity to consume out of regular income, a large part of which would be permanent income according to the theory under consideration.

This is not to assert that the distinction between permanent income and windfall income may not prove to be a fruitful one. Indeed there are both theoretical and empirical reasons for believing that consumers respond differently to different kinds of income. But the postulate that there is zero correlation between transitory income and transitory consumption may not be the most satisfactory way to introduce this distinction into a theory of consumer behavior. The relationships discussed above constitute evidence tending to cast doubt upon this particular formulation of the permanent income hypothesis.

[14] Parenthetically, it might be noted that the definition of consumption used in the permanent income hypothesis may lead to confusion with regard to fiscal policy. Both expenditures on nondurable and durable goods have expansionary effects on employment levels. Thinking of expenditures on durable goods as (largely) savings is likely to obscure this symmetry. For if savings are defined from the point of view of this model, an increase in "savings" which is mainly an increase in durable goods expenditures can lead also to increased national income and employment.

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# EUROPEAN INTEGRATION AND AMERICAN TRADE

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One of the reasons advanced by the Administration in 1958 for requesting an unprecedented five-year extension of the Reciprocal Trade Agreements Act was that such an extension would correspond well to the integration schedule of the European Common Market. This would enable the Administration to negotiate mutual tariff concessions with the European Economic Community within the framework of the General Agreement on Tariffs and Trade. In addition to keeping the Community's tariff low in the interest of world trade, mutual concessions would help to minimize discrimination against U.S. exports in the markets of the six Common Market countries. Indeed, the Congressional hearings preceding the extension of the Act served as a platform for interested parties to declare their opposition to the Common Market as a means of discriminating against U.S. products. It is therefore of interest to examine empirically the extent to which U.S. foreign trade stands to be affected by the moves toward European economic integration.

Within the theory of customs unions, this problem falls under Viner's discussion of trade diversion [8, pp. 41-55]. The diversion takes place when, as a result of the removal of trade barriers within a customs union, a member country replaces its imports from nonunion sources by imports from within the union, and shifts its exports from extra- to intra-union destinations. (These shifts, caused by the newly introduced trade discrimination, may operate contrary to the principle of comparative advantage, and result in malallocation of resources in the world.[1]) The diversion of European trade away from the United States would constitute an example of these shifts.

## I. *Data and Procedure*

In aggregative terms, about 16 per cent of U.S. exports are now destined to the six Common Market nations, and 29 per cent to the

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[1] See J. E. Meade's discussion of the "secondary repercussions" of customs unions [3]. While Meade lists eight possible secondary repercussions, he points out that the two most likely possibilities are those of trade diversion of exports and imports.

prospective Free Trade Area. These figures, usually referred to in discussions of European integration [1] [4], are not very revealing. The competitive impact of the European Community on U.S. trade cannot be determined from aggregate statistics. Trade diversion is a partial-equilibrium concept and must be analyzed in terms of individual commodities. To illustrate, the aggregative figure of 16 per cent would result either if each U.S. export industry marketed 16 per cent of its exports in the Common Market, or if industries accounting for half of total United States exports each shipped 32 per cent of its exports to the Common Market. Yet in the second case the economic dislocations in the United States would be more concentrated and therefore more severe. Disaggregation of the total trade to its commodity components is therefore essential.

Nor is the proportion of an industry's exports to the Common Market the only determinant of the impact of integration on its foreign transactions. Another important factor is the degree to which its products are competitive with European products that stand to gain a competitive advantage through integration. A third factor is the degree to which the six nations' adjustment to a common tariff is likely to increase the tariff incidence on imports from the United States. The latter possibility arises from the fact that the common tariff will be the *unweighted* average[2] of the rates now imposed on each commodity by the four customs areas,[3] and that U.S. exports are unevenly distributed among the constituent nations.

Available data are not completely adequate for examination of these three factors. First, the commodity trade statistics published by the United Nations and the Organization for European Economic Cooperation consist of 205 commodity groups, mainly, the three-digit[4] standard international trade classification (SITC); and some groups may include commodities which are not good substitutes for each other. The five-digit commodity classification, containing 570 items, would have been more useful, but such data are not available. Second, there are no figures on production and consumption of commodity groups equivalent to those of the SITC classification. The degree of European competition with U.S. exports, therefore, has to be estimated solely from trade

[2] It is true that GATT rules require compensation (somewhere in the customs union) whenever the adjustment to a common tariff changes the effective tariff rates charged on individual commodities. But such compensation may not be immediate.

[3] Belgium, Luxembourg and the Netherlands already form one customs area, namely the Benelux customs union. That leaves the six Common Market nations with four independent customs areas.

[4] There are 150 groups in the three-digit SITC classification. However, for certain commodity groups the OEEC statistics provide a further breakdown to the five-digit SITC classification, making a total of 205 groups.

statistics.[5] Third, some of the commodity groups in the three-digit SITC classification bear a wide variety of tariff rates, making the average incidence for the group a rough estimate. With these limitation in mind, data pertaining to the 1953-56 period will be used to assess the order of magnitude of the impact of the Common Market and the Free Trade Area on U.S. exports and imports.

## II. *The Common Market*

Disaggregation of U.S. exports into its commodity components shows that the European Economic Community is not a major export market for products accounting for 71 per cent of total U.S. exports. The six countries absorb: (a) less than 10 per cent of exported products which account for 60 per cent of U.S. aggregate exports, and (b) between 10 and 19 per cent of exported commodities accounting for an additional 11 per cent of the aggregate (Table 1-A). Most food items and manufactured commodities[6] fall in these two categories, and the impact of the Common Market on the products involved cannot be regarded as very severe.

For each product in the remaining 29 per cent an estimate was made of the degree of European competition. The estimates, derived from international trade statistics,[7] were based on the following criteria: (a) whether the commodity is an "export" or an "import" product for the Common Market and the United States (serving as an indicator of the comparative advantage of the two areas in producing the product); (b) the ability of the Common Market to satisfy its own import requirements for the product from within the Common Market; and (c) the share of the Community's imports supplied by the United States. In most cases all three criteria pointed in the same direction. They show that products accounting for 20 out of the 29 per cent of U.S. exports, many of which are not produced in large quantities in the Common Market, are only moderately competitive with European products. The

[5] The use of trade statistics for ranking the industries of each country in order of their comparative advantage, "renders unnecessary inter-country comparisons of efficiency or productivity [which] can be very difficult to make and very doubtful in value" [5, p. 141]. Scitovsky uses trade data to assess the welfare implications of the European Coal and Steel Community. Liesner [2] also makes use of trade statistics to avoid the need for productivity comparisons.

[6] Numbers 0, 1, 6, 7, 8 in the first-digit SITC classification.

[7] The method of deriving the estimates is illustrated in Table 2-A. Commodity number 231, for example, is predominantly an import product for the Common Market, its imports being 33 times the value of its exports. Furthermore, by directing over-one-third of their total exports of the commodity group to nations within the Community, "the six" are able to satisfy only 1 per cent of their import requirements. The United States, on the other hand, supplies 11 per cent of the Common Market's import needs. On the basis of this evidence, the degree of European competition in that commodity was classified as moderate.

remaining 9 per cent consist of commodities which are likely to face strong competition from producers within the Community.

Tariff increases in the Community are not likely to be an important factor operating against U.S. exports, since most commodities within

TABLE 1—THE COMMON MARKET AND AMERICAN EXPORTS

A. Effect of the Common Market on U.S. Exports

| U.S. Exports to the Common Market as Per Cent of Total U.S. Exports of Given Commodities (1956) | Estimated Extent of European Competition (Commodities as Per Cent of Total U.S. Exports) | | |
|---|---|---|---|
| | Moderate | Substantial | Substantial with a Prospective Increase in European Tariff Rates |
| Under 10 | ———60——— | | |
| 10–19 | 0.5 | 10.4 | 1.5 |
| 20–29 | 8.9 | 3.8 | 1.8 |
| 30–39 | 7.6 | 0.1 | |
| 40–49 | 2.6 | 4.2 } 9.2 | |
| 50–59 | 0.6 | 1.1 | |
| 60–69 | 0.6 | 0.0 | |
| | 20.8 | 19.6 | |

[*Table Reads:* .5 per cent of all U.S. exports consist of commodities in the case of which exports to the Common Market form 10–19 per cent of all exports of those commodities and which are not presumed to face serious competition in Europe after the establishment of the Common Market. 10.4 per cent of all U.S. exports consist of commodities in the case of which shipments to the Common Market form 10–19 per cent of all exports of those commodities and which are presumed to face strong competition in Europe after the establishment of the Common Market. For 1.5 of the 10.4 per cent there will also be an increase in the Common Market tariff rates brought about by the adjustment to the common tariff. For the remaining 8.9 per cent tariff rates will remain about the same (namely, will not change by more than two percentage points).]

B. Major U.S. Export Products[a] Likely to Be Affected by the Common Market

| SITC Classification | Description | Per cent of U.S. Exports Accounted for by the Commodity |
|---|---|---|
| 081 | Feeding stuff for animals | 0.4 |
| 311 | Coal, coke and briquettes | 4.0 |
| 512 | Organic chemicals | 1.3 |
| 533 | Pigments, paints, varnishes and related materials | 0.6 |
| 682 | Copper | 1.1 |
| 715 | Metal working machinery | 1.2 |

[a] Excludes commodities which account for less than .3 per cent of American exports.
*Sources:* This and the succeeding tables are based on data derived from:
United Nations, *Commodity Trade Statistics*, 1956; OEEC, *Statistical Bulletins*, Ser. IV, *Foreign Trade*, 1956; OEEC, *The Network of Intra-European Trade, Trade by Product in 1956;* OEEC, *The Six, Commodities Origin and Destination* 1951–1956.

the last group are unlikely to experience a considerable change in tariff incidence. Only 1.8 of the 9.2 per cent group of U.S. exports will face tariff increases in excess of 2 percentage points (last column of Table 1-A). This conclusion was reached by applying weights to the prospec-

TABLE 2—EXAMPLES OF HOW EXTENT OF EUROPEAN COMPETITION WAS ESTIMATED FROM TRADE STATISTICS[a]

A. The Common Market

| Commodity Group (SITC) | Total Imports[b] ($ millions) | Total Exports[b] ($ millions) | Exports to Other Nations in the Common Market as Per Cent of Total Exports[c] | Imports from within the Common Market as Per Cent of Total Imports[c] | Imports from U.S. as Per Cent of Total Imports[b] | Estimated Degree of European Competition |
|---|---|---|---|---|---|---|
| 533 | 38 | 58 | 49 | 27 | 27 | strong |
| 541 | 95 | 198 | 14 | 30 | 26 | strong |
| 282 | 272 | 84 | 90 | 43 | 52 | moderate |
| 283 | 386 | 19 | 50 | 6 | 3 | moderate |
| 284 | 109 | 21 | 82 | 19 | 32 | moderate |
| 231 | 331 | 10 | 35 | 1 | 11 | moderate |

B. The Free Trade Area[b]

| Commodity Group (SITC) | Total Imports ($ millions) | Total Exports ($ millions) | Exports to Other nations in OEEC as Per Cent of Total Exports | Imports from within OEEC as Per Cent of Total Imports | Imports from U.S. as Per Cent of Total Imports | Estimated Degree of European Competition |
|---|---|---|---|---|---|---|
| 091 | 69 | 56 | 54 | 43 | 51 | substantial |
| 533 | 69 | 133 | 39 | 73 | 27 | substantial |
| 541 | 181 | 328 | 28 | 73 | 22 | substantial |
| 599 | 427 | 572 | 51 | 64 | 26 | substantial |
| 663 | 70 | 106 | 52 | 75 | 17 | substantial |
| 711 | 347 | 672 | 40 | 76 | 20 | substantial |
| 714 | 140 | 195 | 46 | 68 | 27 | substantial |
| 715 | 326 | 406 | 44 | 52 | 29 | substantial |
| 716 | 1366 | 2308 | 45 | 79 | 19 | substantial |
| 861 | 165 | 301 | 43 | 78 | 20 | substantial |
| 862 | 55 | 100 | 47 | 78 | 20 | substantial |
| 012 | 247 | 209 | 97 | 84 | 1 | moderate |
| 043 | 328 | 80 | 95 | 24 | 20 | moderate |
| 044 | 359 | 6 | 97 | 3 | 46 | moderate |
| 271 | 162 | 11 | 87 | 10 | 9 | moderate |
| 282 | 347 | 90 | 97 | 32 | 52 | moderate |
| 311 | 1843 | 941 | 96 | 50 | 38 | moderate |
| 312 | 2317 | 5 | 84 | 1 | 2 | moderate |

[a] In some cases the estimate was verified for all subcategories of the commodity group involved (i.e., five-digit SITC classification).
[b] Data are for 1956.
[c] Data are for 1953–56.
*Sources:* See Table 1.

tive tariff changes in individual members of the Community, and computing a weighted average change for each product. The weights are the proportions of U.S. exports to the Community as a whole which are destined to each of the four customs areas. Prospective tariff changes were estimated on the assumption that the common tariff will be an unweighted average of existing rates.[8]

The 9.2 per cent group of U.S. exports which are likely to be strongly affected by the Common Market consist mainly of crude materials and chemicals (see Table 1-B). Close to half of the total is accounted for by coal (commodity 311). Since a common market for steel and coal is already in existence (the European Coal and Steel Community), coal exports should not be affected by the Common Market treaty. It may thus be concluded that only 5 per cent of U.S. exports are likely to be severely injured by the European Common Market.[9]

Similar analysis can be applied to determine the degree of prospective diversion of European exports away from the United States to European destinations, and the commodities which would be affected by such a displacement. Table 3-A shows that for products accounting for 78 per cent of total U.S. imports, the share of imports from the Common Market is less than 20 per cent. For each product in the remaining 22 per cent, an estimate was made of the prospective degree of diversion of European exports from U.S. to European markets. The estimates were based on the following criteria: (a) whether the commodity is an "export" or an "import" product for the Common Market; (b) the proportions of the Community's exports destined to the United States and to intra-Community destinations; and (c) the ability of "the six" to satisfy their import requirements for the product. The estimates show that products accounting for 14.6 of the 22 per cent group are likely to face substantial diversion away from the U.S. market.[10] Exclusion of iron and

[8] Approximate figures on ad valorem tariff rates levied in each customs area on each SITC commodity group were supplied privately by GATT and OEEC. The help of these organizations is gratefully acknowledged.

[9] This estimate is probably too high if the problem is considered in a broader context. Especially if the European economies are operating at full capacity, it is likely that some of their exports to non-European markets will be diverted to intra-European destinations. Certain U.S. export products, partly excluded from the Common Market, may thus be shifted to markets from which European exports are withdrawn.

[10] Table 4-A illustrates the method by which the estimates of possible diversion were derived from trade statistics. Commodity 821, for example, appears to be an export product for the Common Market, its exports being twice the level of its imports. However, only one per cent of the total Community's exports of the commodity group are destined to the United States. On the other hand, by shipping 31 per cent of their exports of the commodity group to intra-Community destinations, the six are able to supply 78 per cent of their total import requirements of the group. To the extent that diversion will occur at all, it is not likely to be away from the U.S. market. The degree of diversion was therefore classified as moderate.

steel would lower this figure to 12.7 per cent. (The figure would be further reduced if we assumed that at least in the case of certain products European nations will attempt to maintain the level of their exports to

TABLE 3—THE COMMON MARKET AND AMERICAN IMPORTS

A. Effect of the Common Market on U. S. Imports

| American Imports from the Common Market as Per Cent of All U. S. Imports of Given Commodities | Estimated Degree of Diversion of European Exports from the U. S. Market[a] (Commodities as Per Cent of Total U. S. Imports) | | |
|---|---|---|---|
| | Moderate | Substantial | |
| Under 10 | —70— | | |
| 10–19 | 3.4 | 4.9 | |
| 20–29 | 2.2 | 6.0 | }14.6 (20–89) |
| 30–39 | 1.0 | 1.5 | |
| 40–49 | 2.1 | 1.5 | 14.6 |
| 50–59 | 1.2 | 2.7 | |
| 60–89 | 0.1 | 2.9 | |
| | 10.0 | 19.5 | |

B. Major U. S. Import Products[b] Likely to Experience Considerable Diversion as a Result of the Common Market

| Commodity Number (SITC) | Description | Per Cent of Total U. S. Imports |
|---|---|---|
| 013 | Meat, canned and meat preparations | 0.9 |
| 521 | Mineral tar and crude chemicals from coal, petroleum and natural gas | 0.3 |
| 551 | Essential oils, perfume and flavor materials | 0.3 |
| 653 | Textile fabrics of standard type (not including narrow and special fabrics), other than cotton fabrics | 1.7 |
| 655 | Special textile fabrics and related products | 0.3 |
| 657 | Floor coverings and tapestries | 0.3 |
| 664 | Glass | 0.4 |
| 666 | Pottery | 0.3 |
| 671 | Silver, and platinum group metals | 0.3 |
| 672 | Precious and semiprecious stones and pearls | 1.4 |
| 681 | Iron and steel | 1.9 |
| 686 | Zinc | 0.5 |
| 687 | Tin | 1.2 |
| 699[b] | Manufactures of metals, n.e.s. | 1.1 |
| 861 | Scientific, medical, optical, measuring and controlling instruments and apparatus | 0.3 |
| 899 | Manufactured articles, n.e.s. | 0.9 |

[a] On the assumption that European countries will make no special efforts to maintain the level of their exports to the United States in order to earn dollars.

[b] Excludes commodities which account for less than .3 per cent of the U. S. imports. Among these are machinery items (number 7 in the first-digit SITC groups).

*Sources:* See Table 1.

TABLE 4—EXAMPLES OF HOW DEGREE OF DIVERSION OF EUROPEAN EXPORT FROM THE U. S. MARKET WAS ESTIMATED FROM TRADE STATISTICS

A. The Common Market[a]

| Commodity Group (SITC) | Total Imports | Total Exports | Exports to the U. S. as Per Cent of Total Exports | Exports to the Common Market as Per Cent of Total Exports | Imports from the Common Market as Per Cent of Total Imports | Estimated Degree of Diversion |
|---|---|---|---|---|---|---|
| | $ millions | | | | | |
| 024 | 94 | 109 | 14 | 45 | 53 | substantial |
| 521 | 25 | 24 | 40 | 34 | 31 | substantial |
| 612 | 7 | 17 | 28 | 26 | 67 | substantial |
| 657 | 38 | 95 | 22 | 23 | 58 | substantial |
| 687 | 61 | 83 | 39 | 34 | 42 | substantial |
| 012 | 7 | 37 | 0.1 | 12 | 23 | moderate |
| 042 | 46 | 53 | 1 | 25 | 28 | moderate |
| 531 | 41 | 80 | 3 | 17 | 36 | moderate |
| 629 | 52 | 150 | 4 | 18 | 59 | moderate |
| 652 | 91 | 309 | 3 | 11 | 44 | moderate |
| 821 | 22 | 51 | 1 | 31 | 78 | moderate |

B. The Free Trade Area[a]

| Commodity Group (SITC) | Total Imports | Total Exports | Exports to the U. S. as Per Cent of Total Exports | Exports to OEEC as Per Cent of Total Exports | Imports from OEEC as Per Cent of Total Imports | Estimated Degree of Diversion |
|---|---|---|---|---|---|---|
| | $ millions | | | | | |
| 013 | 228 | 184 | 27 | 51 | 42 | substantial |
| 055 | 89 | 94 | 12 | 55 | 64 | substantial |
| 112 | 418 | 469 | 24 | 39 | 38 | substantial |
| 012 | 247 | 208 | 0.2 | 97 | 84 | moderate |
| 073 | 35 | 54 | 0.7 | 57 | 96 | moderate |
| 552 | 55 | 153 | 3 | 34 | 91 | moderate |
| 591 | 18 | 57 | 2 | 28 | 92 | moderate |
| 864 | 81 | 73 | 19 | 38 | 99 | moderate |

[a] Data are for 1956.
*Sources:* See Table 1.

the United States, and make the diversion from other markets.) This is not a substantial proportion of U.S. imports. As shown in Table 3-B, the products which are likely to be affected are primarily manufactured commodities and machinery (items 6, 7, and 8 of the first-digit SITC classification).

Contrary to common apprehensions, it appears unlikely that the Common Market will cause serious disturbances in U.S. trade.[11] Al-

[11] This conclusion is consistent with estimates of the total trade diversion of the Common Market compiled by P. J. Verdoorn [7, pp. 482-500]. He used the two- and three-digit SITC commodity groups assuming a relatively high cross-elasticity within each group. See also Scitovsky [5, pp. 64-70].

though a few export industries may be affected, there will be a larger number of import-competing industries for which foreign competition may ease considerably. On the other hand, the data show little overlapping between the export- and import-competing industries which stand to be affected. Contraction and expansion will take place in different industries, and may necessitate a lengthy period of adjustment. This is consistent with the principle that in any one country there is likely to be considerable substitutability between the goods which it imports from various sources and between the goods which it exports to various destinations. There is unlikely to be such close substitutability between imports as a class and exports as a class.

## III. *The Free Trade Area*

While implementation of the Common Market treaty is already under way, the Free Trade Area is still in the negotiation stage. Nonetheless, it is of interest to assess its possible impact on U.S. foreign trade. It will be assumed that the Free Trade Area will consist of all OEEC member nations.

A free trade area, which does not require adherence to a common tariff, may not be as discriminatory against nonmember nations as a customs union. Unless import control through certificates of origin is adopted and strictly enforced, the lowest tariff that prevails on each commodity will tend to become the effective rate for the entire Free Trade Area.

Being considerably larger than the Common Market both in area and population, the Free Trade Area absorbs a much larger share of U.S. trade. The commodity composition of this trade reveals that products accounting for 58 per cent of U.S. exports are exported to the Free Trade Area in less than substantial proportions (i.e., less than 20 per cent; Table 5-A). On the other side of the ledger are 22 per cent of U.S. exports which consist of products that are likely to be significantly affected by the Free Trade Area. They are exported to that area in significant proportions, and are likely to face stiff competition from European producers.[12] For the most part they consist of food, crude materials and chemicals (Table 5-B). Using the same technique for the import side, Table 6 indicates that commodities accounting for 22 per cent of U.S. imports, consisting mainly of manufactured goods and machinery, are likely to be strongly affected by the Free Trade Area.

It appears that the effect of the Free Trade Area on U.S. foreign trade is likely to be much more widespread than that of the Common

[12] Table 2-B illustrates how the estimates of the degree of European competition were derived from trade statistics in the case of the the Free Trade Area. That degree was considered moderate for many commodities in the case of which most OEEC exports are already destined to intra-OEEC destinations.

Market. This is not *necessarily* the result expected on a priori grounds. Although the Free Trade Area accounts for a larger share of aggregate U.S. trade, it may be self-sufficient in many commodities and thus be less susceptible to trade diversion. Consequently, not all U.S. export

TABLE 5—THE FREE TRADE AREA AND AMERICAN EXPORTS

A. Effect of the Free Trade Area on U. S. Exports

<table>
<tr><th rowspan="3">U. S. Exports to OEEC as Per Cent of Total U. S. Exports of Given Commodities</th><th colspan="3">Estimated Degree of European Competition</th></tr>
<tr><th colspan="3">(Commodities as Per Cent of Total U. S. Exports)</th></tr>
<tr><th>Moderate</th><th colspan="2">Substantial</th></tr>
<tr><td>Under 10</td><td colspan="3">3.7</td></tr>
<tr><td>10–19</td><td>0.5</td><td>20.4</td><td></td></tr>
<tr><td>20–29</td><td>0.7</td><td>9.0</td><td rowspan="5">22.2</td></tr>
<tr><td>30–39</td><td>0.6</td><td>6.2</td></tr>
<tr><td>40–49</td><td>10.0</td><td>2.8</td></tr>
<tr><td>50–59</td><td>6.6</td><td>0.7</td></tr>
<tr><td>60–99</td><td>2.2</td><td>3.5</td></tr>
<tr><td></td><td>20.7</td><td>42.6</td><td></td></tr>
</table>

B. Major U. S. Export Products[a] Likely to Be Affected by the Free Trade Area

| Commodity Number (SITC) | Description | Per Cent of Total U. S. Exports |
|---|---|---|
| 053 | Fruits preserved and fruit preparations | 0.5 |
| 081 | Feeding stuff for animals | 0.4 |
| 091 | Margarine and shortenings | 0.4 |
| 121 | Tobacco unmanufactured | 1.8 |
| 122 | Tobacco manufactured | 0.3 |
| 211 | Hides and skins (except fur skins), undressed | 0.3 |
| 251 | Pulp and waste paper | 0.5 |
| 272 | Crude minerals, excluding coal, petroleum, fertilizer materials and precious stones | 0.5 |
| 313 | Petroleum products | 3.4 |
| 411 | Animal oils and fats | 0.7 |
| 412 | Vegetable oils | 1.2 |
| 511 | Inorganic chemicals | 0.6 |
| 512 | Organic chemicals | 1.3 |
| 533 | Pigments, paints, varnishes, and related materials | 0.6 |
| 599 | Miscellaneous chemical materials and products | 1.8 |
| 663 | Mineral manufactures, n.e.s., not including clay and glass | 0.3 |
| 682 | Copper | 1.1 |
| 714 | Office machinery | 0.7 |
| 715 | Metal working machinery | 1.2 |
| 931 | Returned goods and special transactions | 2.0 |

[a] Excludes commodities which account for less than .3 per cent of U. S. exports.
*Sources:* See Table 1.

TABLE 6—THE FREE TRADE AREA AND AMERICAN IMPORTS

A. Effect of the Free Trade Area on U. S. Imports

| U. S. Imports from OEEC as Per Cent of All U. S. Imports of Given Commodities | Estimated Degree of Diversion of European Exports from the U. S. Market | | |
|---|---|---|---|
| | Moderate | Substantial | |
| Under 10 | 5.4 (both columns) | | |
| 10–19 | 5.5 | 7.0 | |
| 20–29 | 1.6 | 3.6 | 22.1 (20–29 through 70–100) |
| 30–39 | 1.7 | 4.7 | |
| 40–49 | 0.0 | 3.4 | |
| 50–59 | 0.3 | 2.5 | |
| 60–69 | 1.3 | 3.5 | |
| 70–100 | 5.6 | 4.4 | |
| | 16.0 | 29.1 | |

B. Major U. S. Import Products[a] Likely to Experience Considerable Diversion as a Result of of the Free Trade Area

| Commodity Number (SITC) | Description | Per Cent of Total U. S. Imports |
|---|---|---|
| 013 | Meat, canned and meat preparations | 0.9 |
| 112 | Alcoholic beverages | 1.6 |
| 121 | Tobacco unmanufactured | 0.7 |
| 212 | Fur skins, undressed | 0.6 |
| 272 | Crude minerals, excluding coal, petroleum, fertilizer materials, and precious stones | 1.6 |
| 292 | Crude vegetable materials, inedible, n.e.s. | 0.7 |
| 521 | Mineral tar and crude chemicals from coal, petroleum and natural gas | 0.3 |
| 551 | Essential oils, perfume and flavor materials | 0.3 |
| 653 | Textile fabrics of standard type (not including narrow and special fabrics), other than cotton fabrics | 1.7 |
| 655 | Special textile fabrics and related products | 0.3 |
| 657 | Floor coverings and tapestries | 0.3 |
| 664 | Glass | 0.4 |
| 666 | Pottery | 0.3 |
| 671 | Silver, and platinum group metals | 0.3 |
| 672 | Precious and semiprecious stones and pearls, worked and unworked | 1.4 |
| 684 | Aluminum | 1.0 |
| 686 | Zinc | 0.5 |
| 687 | Tin | 1.2 |
| 689 | Miscellaneous nonferrous base metals employed in metallurgy | 0.6 |
| 732 | Road motor vehicles | 1.2 |
| 734 | Aircraft | 0.7 |
| 841 | Clothing, except fur clothing | 1.2 |
| 861 | Scientific, medical, optical, measuring and controlling instruments and apparatus | 0.3 |
| 899 | Manufactured articles, n.e.s. | 0.9 |

[a] Excludes commodities which account for less than .3 per cent of U. S. imports.
*Sources:* See Table 1.

products which would experience a diversion as a result of the Common Market would be affected by the Free Trade Area.[18]

## IV. *Concluding Comments*

The foregoing discussion was confined to the possible impact of European integration on American foreign trade. Because foreign trade plays a relatively unimportant role in the American economy, the impact of European integration on aggregate American production is not likely to be significant. This does not mean that no one industry will be seriously affected. But to study the impact of the Common Market on individual industries in the United States, one would need to examine the share of foreign trade in total production or consumption of the products which were found above to be significantly affected. Such a study would necessitate data on production by commodity groups which parallel those of the SITC classification. Indeed, the need for such data was quite apparent throughout this study, particularly in estimating the degree of European competition. Studies of regional integration must usually be couched in partial equilibrium terms, and the increasing emphasis on regional unions will undoubtedly result in greater use in research of the commodity composition of trade. A more detailed product breakdown than is now available, as well as parallel figures on production and consumption in the major trading nations, would add considerably to the usefulness of the trade statistics.

In addition, the investigator would benefit from having approximate figures on the ad valorem tariff incidence charged on each commodity group by each of the major trading nations. In the present study, for example, no use was made of U.S. tariff rates in examining the possible diversion of European exports away from the U.S. market. Yet a comparison of the U.S. and European rates would have been useful in assessing the degree to which European exporters would be tempted to

[18] U.S. coal (commodity 311) exports for example are likely to suffer more from European competition in the Common Market than in the Free Trade Area, even though the latter area absorbs 58 per cent of U.S. coal exports, as compared to only 48 per cent absorbed by the former. This is so because OEEC countries ship 96 per cent of their coal exports to intra-OEEC destinations, while the six Economic Community nations market only 70 per cent of their coal exports within their own area. Members of the Common Market would therefore be more likely than members of the Free Trade Area to transfer their exports from extra- to intra-union destinations, thereby displacing imports from the United States. Commodity 733 is a second example. OEEC nations receive 92 per cent of their import needs from within the area, and cannot increase that share substantially upon the establishment of the Free Trade Area. Their exports to the United States are therefore unlikely to be strongly affected. The Common Market countries, on the other hand, satisfy only 61 per cent of their import needs from member countries (by marketing there 16 per cent of their total exports), and are therefore more likely to divert their exports from the United States to European destinations.

prefer European over U.S. markets.[14] (Note, however, that because most of the affected import products are manufactured goods which are taxed both here and in Europe, with U.S. tariff rates being about the average of their European counterparts, this omission is unlikely to have introduced a significant bias in the results.) The rates charged by the various countries vary considerably within each commodity group, and only a detailed study can reveal the average incidence. An alternative, more laborious but more accurate, solution would lie in offering the trade statistics in such detail that the commodity breakdown would match more closely its counterpart in the tariff laws.[15]

[14] It can also be shown that comparable data on trade and tariff rates by commodity group would be useful for estimating the effect of regional integration on world welfare.

[15] The *1956 Economic Survey of Europe* [6, p. 12], for example, reports a summary of estimates made by GATT of ad valorem tariff incidence levied by European countries in 1952 on each of the 570 commodities in the five-digit SITC classification.

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# COMMUNICATIONS

## Price-Making in Forest Service Timber Sales

A practical study in price-making, with interesting and important implications for price theory, welfare economics, and public policy, appears in the sale of stumpage—representing the right to cut timber for private disposal in the market place—currently growing on the vast public domain of the federal government. While theoretical and applied economists have virtually ignored the elemental problems involved, several public bodies, including the United States Forest Service in the Department of Agriculture, the Bureau of Land Management in the Department of Interior, the Bureau of Indian Affairs, and the various state governments, have had to wrestle chronically with the basic issues. The Comptroller General's office, and the usual Congressional committees, likewise participate in forging the government's timber sales program.

This paper concentrates on the problems facing the United States Forest Service in managing public timber sales; it is hoped that this communication may prompt further inquiry into a subject which is important currently and destined to become even more pressing in the future.

*Forest Service Timber Sales.* The size and growth of Forest Service sales of stumpage, in value and volume, may be described briefly.[1] In value, sales have increased more than seven-fold since 1945, from $13 million in 1945 to $98 million in 1956; further growth has since been registered. In real terms the volume of cut timber has more than doubled over the same period, from 3.1 billion to 6.9 billion board feet. It is estimated that the Forest Service stumpage comprises about 10 per cent of the total timber cut in a calendar year. In some states its importance is, of course, larger than this national average; about 15 to 20 per cent of the timber cut in such states as Washington, Oregon, and California may come from the national forests so that federal forest policy is of vast significance in the economies of these states.

A strong upsurge in stumpage prices relative to output is implicit in the value and volume figures. According to the available indexes, stumpage prices in 1956 were at 241 compared to lumber prices at 127, with 1947-49 equal to 100, and all commodities (at wholesale) at 114. While the index of stumpage prices has greatly outpaced the other price movements, the index is not wholly comparable to the usual price indices, for it is computed merely by taking the Forest Service receipts for a given year, dividing through by the volume of board feet sold, and comparing the quotient to the base-year value.[2] Using board feet as the divisor helps cloak changes in the composition

[1] All data, unless otherwise specified, are taken from [5]. Much of the information gathered in this report was derived from the records of the Division of Timber Management in the United States Forest Service.

[2] Thus a typical price index will involve $[\Sigma P^1Q^0/\Sigma P^0Q^0]$ relationships. In the case of

of lumber sold, from low-grade or storm-damaged or rotted timber to higher priced species such as Douglas fir or southern and Ponderosa pine.

*Greater Variability of Stumpage Prices.* The principle of derived demand makes the wider range of stumpage price movements as against lumber price movements more intelligible. Stumpage is the raw material from which lumber is derived after tree-felling, bucking, transporting, and sawing. On the principle of derived demand the stumpage price is a reflection of the price of lumber after other processing costs are deducted. Hence, the same absolute variation in the price of the raw material as in the finished product will occasion proportionately sharper price movements of the former. As stumpage prices frequently account for about 1/10th of lumber prices, a 1 per cent lift in the price of lumber can mean a 10 per cent jump in stumpage prices.[3] The index number movements of the two series generally confirm this theoretical relationship.

*Stumpage Volume and the Principle of Sustained Yield.* While in the more typical pricing problem a firm must decide on its volume of output in the light of expected price-cost phenomena, in the case of Forest Service timber offerings the output aspect of the problem is eliminated. For the amount of "standing trees on the stump"—stumpage—that the Forest Service is willing to place under the axe is governed by a long-range operating policy whereby it proposes to trim the "tree population" as long as the volume of growing public and private timber retained in the forests promises to remain at least constant from year to year. This concept of (ultimately) maintaining the timber stock at a constant size is described as the principle of sustained-yield.[4]

Although a full discussion of alternate cutting principles would embody a study in itself, a word may be proffered on the notion of sustained yield. An economist might compare the rate of timber growth and the rate of interest in order to determine the cutting date. Application of the latter principle would place Forest Service operations on a private profit basis which may not be consistent with broader views of welfare; the Forest Service

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national forest stumpage the computation usually takes the form

$$[\Sigma P^1Q^1/\Sigma Q^1] \div [\Sigma P^0Q^0/\Sigma Q^0].$$

Even the more refined stumpage price series, which are confined to one species, have trouble overcoming the quality variations within the class.

[3] Thus if $P_a$ represents the final lumber price and $P_s$ the stumpage price, then the ratio of relative price changes is given by: $[P_a dP_s/P_s dP_a]$. Hence if $(dP_s/dP_a) = 1$, or if lumber and stumpage prices move by the same absolute amount, and if lumber prices are 10 times the stumpage prices, then the relative change in the stumpage price will be 10 times the relative change in the lumber price. Of course, this assumption of other processing costs holding firm is an extreme one; labor costs are particularly likely to fluctuate with the state of the lumber market.

[4] Because of the presence of a large amount of mature and overmature timber the principle of sustained yield represents merely the present goal of Forest Service policy. That the timber reserves will actually remain constant after the liquidation of the present stock of mature timber is at best a guess because of the inadequacies of inventories of forest size. Despite efforts currently being made to improve the inventory estimates, some skepticism may be expressed as to their ultimate reliability without excessive expense lavished on this bookkeeping item.

would have to make estimates of the annual growth rate of almost every tree under its domain, it would have to forecast *future* lumber prices and thus stumpage prices, and it would also have to select an appropriate interest rate. Besides the fact that public agencies are anxious to avoid forecasting responsibilities, it is doubtful that our understanding of welfare economics is conclusive enough to recommend one output policy as decisively superior to another, especially in a dynamic economy where important changes are expected to take place over time.[5] Further, much of Forest Service activity scarcely can be defined, much less calculated, in precise pecuniary terms. Its purposes and objectives are multidimensional, involving wildlife preservation, conservation of wilderness areas, provision of watersheds and recreational areas, checking insect depredations, etc. Because of the many facets of its operations it is difficult to envisage the optimal cutting policy that would be dictated by an economic calculus. Whatever the appeal of an alternative policy to economic specialists, there would still remain the sometimes awesome problem of convincing industry sources and Congress of its soundness. In any event, the principle of sustained yield is the present operative policy; its merits lie in its operational simplicity and in an acceptable moral basis involving the preservation for posterity of forest resources equivalent to those possessed by the present generation.[6]

Thus as long as the timber population promises to be sustained the Forest Service is willing to permit a stumpage cut. In recent years it has usually succeeded in hitting within about 70 per cent of its target; the discrepancy of actual as against permissive cut is due not to any particular shortcoming in pricing policies or any deliberate withholding, but because much of the timber listed for disposal is simply not economical from the standpoint of private enterprise. For example, much of it is located in areas to which access is difficult because of the lack of a road system and the failure of Congress to appropriate more funds for road construction. The Alaskan forests offer a good case in point; the transport costs and the lack of access roads would preclude a profit unless the operations were actually subsidized [5, p. 31]. Many other areas, apart from Alaska, differ only in degree.

To delineate the pricing problem that the present policy poses, the principle of sustained yield will be premised. Hence the market problem of the Forest Service, as currently conceived, is to price a given volume of stumpage.

*Appraisal and the Degree of Competition.* With a given volume of timber thrown onto the market it might be concluded that the problem facing the

[5] See [2, Ch. 6 and 10]. For the difficulties even under more stationary conditions, see [1].

[6] On the moral aspects of the concept of "maintaining capital intact," as the principle of sustained yield may be regarded, see [3, Ch. 4]. It might also be noted that an "economic" concept of timber-cut would have to take account of such silvicultural facts whereby thinning out a stand permits accelerated growth of the remaining trees. Besides this, and the other interdependent relations noted in the text, there would also have to be allowances for fires, storms, and insect damage, so that a whole host of factors would have to be forecast. While the entire subject deserves detailed and separate study it is undoubtedly a slippery and intricate problem.

TABLE 1—NUMBER OF BIDS RECEIVED IN 1957 ON VARIOUS SIZE STUMPAGE SALE OFFERINGS FOR REGIONS 5 AND 6

| Number of Bidders | Appraisal Class | | |
|---|---|---|---|
| | $20,000–$49,999 | $50,000–$100,000 | Over $100,000 |
| 0 | 2 | 1 | 10 |
| 1 | 17 | 24 | 48 |
| 2 | 9 | 10 | 17 |
| 3 | 12 | 9 | 12 |
| 4 | 6 | 6 | 4 |
| 5 | 6 | 0 | 5 |
| 6 | 7 | 3 | 2 |
| Over 6 | 9[a] | 4[b] | 1[c] |
| Total No. of Offerings | 68 | 57 | 99 |

[a] The breakdown was: 1 case of 7 bidders, 1 of 8, 2 of 9, 2 of 10, 1 of 11, 1 of 15, 1 of 16.
[b] The breakdown: 1 case of 7 bidders, 1 of 9, 2 of 13.
[c] The breakdown: 1 case of 14 bidders.
*Source:* Combined from Tables 27 and 32, in [5, pp. 104, 110].

government agency literally dissolves: the stumpage made available can be priced through the competitive processes. This certainly would be the outcome if there were the numerous buyers envisaged by the theory of pure competition; no further action would be required of the Forest Service than to offer the stumpage for sale.

Let us consider, however some typical cases of bidding for Forest Service timber. In Table 1 there is listed the number of bidders actually found for Forest Service stumpage whenever the appraisals ran to $20,000 or more in Forest Service regions 5 and 6, encompassing Washington, Oregon, and California.[7] As shown, the number of bidders is generally small, with 1, 2, or 3, and very seldom over 10 bidders. One-bidder situations in these two regions alone account for over $15 millions of Forest Service receipts, or almost 15 per cent of its total sales volume [5, ch. 5]. These instances would thus be appropriately labeled as monopsony and oligopsony on the purchase side, with one seller whose objective was to accomplish a sales goal rather than to maximize profits as in the textbook version of the typical bilateral monopoly problem.

In such circumstances it becomes imperative for the Forest Service to name an appraisal or floor price. Not only is this required by law but it is dictated by commonsense, for otherwise, in the one-bidder situations, valuable timber

[7] A word of explanation on the size of the sample involved: The total value of Forest Service sales involving an appraisal of $20,000 or more in regions 5 and 6 in 1957 amounted to $31.6 million, or about 30 per cent of the total of Forest Service receipts over all 10 regions. Further, if Region 8 (covering the southwest area) was included the total of Forest Service receipts involved would go closer to 40 per cent, with bidding experience roughly the same. Region 8 is omitteed from the tabulation only because the appraisal data for that area begins at $15,000.

from the public domain would be transferred to private operators at almost zero prices.[8]

Hence, in interpreting Table 1 it should be borne in mind that, unless the appraisal is set at a nominal figure, its effect is to eliminate some bidders. Thus if the appraisal is pitched close to the price that may be bid by the most fortunately situated bidder, say one willing to pay a maximum of $50,000, while the next highest potential bid is only $35,000, then an appraisal above the lower sum will leave only one bidder in the market.

A supplementary question connected with the appraisal is whether the floor price ought to be announced. While there would be no especial damage to the government's interests in announcing the appraisal when there are several bidders, the troublesome case arises when there is only one expected bidder in sight when the sales offering is prepared, the trees marked off for sale, and the date of the transaction publicized. The announced appraisal price then automatically becomes the sale price, providing a bargain in public timber to the buyer. Just such cases have prompted criticism of the Forest Service by the Comptroller General's office.

In some regions the Forest Service has followed the practice of posting its appraisal and then inviting sealed bids; more commonly, oral bids are entertained after the appraisal is announced, as in a typical auction sale. Results indicate that returns to the Forest Service are higher when oral bidding is permitted, as theory would lead us to suppose; sealed single bids, when several bidders are in the field, do not enable firms to revise their price evaluations in the light of their competitive position as revealed by the bidding of others. Where only one, two or three bidders are expected on a timber stand the question arises whether the appraisal, after being made, ought not to be concealed from the prospective purchasers. Thereafter sealed bids could be invited with subsequent oral auction bidding permitted only to those whose sealed bid was higher than the concealed appraisal: the sealed bid would thus constitute an entry blank to qualify in the final auction. In some small-number situations this market tactic would impart certain advantages to the Forest Service and insure a return more nearly equal to the outcome that might result from greater competition.[9]

*Appraisal Principles.* Accepting the proposition that appraisals are necessary, in principle as well as under present law, there still remains the question

[8] The operative law goes back to June 4, 1897 (16 U.S.C. 476) requiring that timber be sold "for not less than the appraised value." See the statement by Ira J. Mason, Director of Division of Timber Management [6, pp. 82-88].

[9] With a small number of bidders the prospect of collusion can never be entirely ruled out. If there are two or three prospective bidders it is always possible for them to agree, verbally or tacitly, that one is to bid for the January offering, another for a February sale, the third to enter the lists in May, etc. As there have been occasional charges of collusion, there may be some added merit in using a concealed appraisal procedure to frustrate "bargain hunting" at the appraisal level. With a concealed appraisal the qualifying sealed bid might at least assure a return to the Forest Service in excess of the appraisal price. This would be even more probable if an increase in the number of competitors could be fostered.

of the rules that ought to guide the appraisal determination.[10]

Historically, the Forest Service has interpreted the law requiring it to make an appraisal as signifying that it was incumbent upon it to name a "fair" price, construing this as "what a willing buyer and a willing seller would pay in a free market." Language of this sort has appeared in Forest Service manuals and explanations of its objectives and practices.

Needless to say, an economist finds this description distressingly vague and, to avoid the ambiguities involved, is inclined to emphasize the functional nature of the price mechanism. What functions are to be accomplished by an appraisal price for stumpage? An allocative objective is not involved since the volume of timber cut is at present decided by other criteria. Two functions of an appraisal seem paramount: (1) the price named must be low enough to move the timber offerings of the Forest Service into the hands of loggers; (2) the stumpage price must permit lumber operators to earn sufficient profits over time to remain in business and assist the Forest Service in executing its future sales program and design for timber use. At times these criteria may conflict, for the first one involves a shorter-run, and the second function, a longer-run view. If so, which should be sacrificed?

To illustrate, a contradiction between the functions may arise in actual appraisals because to move a stand of timber the Forest Service may be able to entice a buyer at, say $50,000 and yet, in the light of lumber prices and costs firms may not be able to realize a profit with stumpage in excess of $30,000. That is, merely to get the timber in order to continue operations and stay in business the bidding firms may be willing to sacrifice immediate profits. Which of these two figures ought to become the appraisal price?

Essentially, the conflict here is one of long-period goals versus shorter-run criteria: in the ordinary theory of the firm in imperfect markets it corresponds to the difference between a short- and long-run maximization policy. Considering the public responsibilities of the Forest Service, it would seem that it ought to embrace the longer outlook.[11] What would seem to be required is an erring on the side of a lower price, one which would permit greater profitability to potential bidders than they would be willing to accept for themselves if required to bid to the utmost in the short-period in order to acquire the timber for continuing operations. Conversely, if because of very limited competition the Forest Service appears to be faced with a coercive endeavor to force its appraisal below this long-run norm, the principle of a reasonable profit allowance ought to be enforced.

*Transactions Analysis.* Once some pricing objectives are established the next phase involves the actual mechanics of appraisal, a subject on which

[10] Industry representatives seldom contest the need for appraisals, concentrating their fire on the method used and the level attained, arguing invariably that the appraisals that result are excessive.

[11] Of course, if lumbering capacity exceeds the likely current and near future output needs more emphasis would be given to the shorter view. The same would follow if entry into the industry is easy and rapid. Both these elements are frequently encountered in the logging phases of the lumber industry.

analytical economics is indecently silent; appraisal practitioners show no such reticence even when their other qualifications are not wholly apparent. One obvious suggestion is that the appraiser compare the particular timber stand with other stands recently sold and decide on its relative value superiority or inferiority. While this appeal to comparability seems plausible it is hardly likely to assist the Forest Service appraiser for the criterion of comparability is merely a statement of the problem, not a guide to its resolution.

It is in the use of information amassed on previous sales that the Forest Service has developed a method which is both ingenious and immanently reasonable for accomplishing its appraisal purposes. This is a method often described as "transactions analysis" or "transactions evidence."[12]

If the determination of stumpage prices is viewed as a direct application of the theory of derived demand, the price of lumber is the important final determinant. In principle, the full costs of processing the stumpage should be deducted from lumber prices and thereupon, the residual constitutes the stumpage price. Actual computation, unfortunately, is less determinate, as illustrated by a trivial example.

Suppose that on a timber stand of 1,000,000 board feet (MMBF) the selling price for lumber, say, is $36 per 1,000 board feet (MBF). Suppose, too, that the ordinary operating costs, including labor, depreciation, transportation to market, local taxes, etc., amount to $24 per MBF. Subtracting the two, the difference of $12 is termed by foresters as a "conversion return." Thus:

| | Per MBF | Per MMBF |
|---|---|---|
| Lumber Price Receipts | $36 | $36,000 |
| Direct Costs | 24 | 24,000 |
| Conversion Return | $12 | $12,000 |

It is at this stage that most of the controversies between loggers and the Forest Service originate, though disputes also flare up over allowances for access road construction costs and over the use of original rather than replacement cost of loggers' equipment (in a period of inflation). In any event, whatever the resolution at the cost stage[13] there is almost inevitably a conflict over the appropriate profit margin: if profits can be isolated then the stumpage price can be derived as the residual. Unfortunately we have two unknowns, profits and stumpage, and only one equation stipulating that their sum equals the conversion total. Here then is an obvious source of acrimony and dispute.

[12] One of the early statements, which apparently influenced Forest Service practice, is the article by Rothery [4].

[13] Some of the direct costs, such as wage rates, may themselves be a reflection of the stumpage volume and lumber prices. I think these may be taken as practically constant in appraisal analysis, or based on an anticipation of events in labor markets for loggers. Errors here, I suspect, are rather small. Unless this proviso of "other costs constant" (or nearly so) is accepted, we have a usual general equilibrium problem which literally precludes appraisal work except by rather sophisticated-omniscient maybe-appraisers. In my opinion the practical problem does not require this degree of wisdom.

How should the conversion return be split? Not uncommonly in the past, as practical men are wont to do, the total has been split evenly between stumpage and profits, so that, in the sample problem, $6 per MBF would go for each.

In practice, to ascertain an appropriate profit margin two major choices are possible. First, studies might be made of the capital invested by the bidding firms and a computation made of a profit allowance that would yield, in that public utility phrase of monumental vagueness, "a reasonable return on prudent investment value." But as one reflects on the less than luminous history of public utility regulation, one is apt to shrink from a recommendation which would encumber the Forest Service with endless appraisals and interminable controversy in order to extract a simple mark-up factor or unit profit allowance per unit of sales. Substantially, the Forest Service has adopted, in my opinion, the more sensible mark-up approach.

To arrive at the appropriate mark-up, the Forest Service has relied on "transactions evidence" to yield a mark-up which appears appropriate in the light of available experience. To illustrate, suppose that the Forest Service includes a profit allowance of 15 per cent on lumber sales price as a necessary unit return. Thus in our example, $5.40 per MBF would be regarded as a profit allowance and the residual, $6.60 per MBF or $6,600 would become the appraised stumpage price for the full stand. Suppose that after oral auction bidding the timber stand is actually sold at a price of $9,000. Suppose that this turns out to be a fairly typical reaction: when a profit allowance of 15 per cent of lumber prices is built into the appraisal the ultimate market price for which stumpage sells indicates buyer willingness to accept a mark-up apparently below 10 per cent. If an examination of cases of actual sales (transactions evidence) indicates that appraisals are too low and that the Forest Service profit allowance is on the generous side, a reduction in the mark-up factor in subsequent appraisals appears appropriate. Conversely, if cases emerge in which several expected bidders fail to put in a bid, or if sales are occurring at prices very close to the appraisal figures, there would be good reason to reduce the floor prices by augmenting the calculated profit mark-up.[14]

Rather than move the profit mark-up precipitately, as in the example described where a descent from 15 to 8.3 per cent appeared justified—which also entails an implicit argument that those bidding for stumpage were using the same price and cost data as the Forest Service—it might be suggested that the

[14] It is in resolving this problem of the adequacy of the profit allowance that one might even make a stronger argument for employing concealed appraisals and sealed qualifying bids preparatory to auction sales. For in doing so information can always be derived on the basis of actual bids as to whether appraisals are too high or too low. With announced appraisals (as at present) when appraisals approach too closely to the ultimate market price the effect is to eliminate the bid experience of those potential buyers whose maximum offer lies below the appraisal price: the number of actual bids received will be few and hover about the appraisal. Only a very discerning market analyst could then tell whether potential bidders were eliminated because of the excessiveness of the appraisal, and estimate the price that excluded bidders might have been willing to pay.

Forest Service ought to employ its recent sales experience more tentatively. For example, if in examining recent bid experience it is found that overbids in excess of 20 per cent of appraisals are commonplace, then the calculated profit margin might be reduced by 1 or 2 per cent. On the other hand, if overbids appear within a range of 10 per cent or lower, the intimation would be that the Forest Service was pricing too tightly. In general, recent Forest Service procedure has appeared to involve comparisons of price to appraisal ratios before lowering or raising its mark-up allowance.

*Other Issues.* Besides the mark-up problem there are several other items to interest the economist. For example, what lumber prices are to be used in making the estimates of conversion returns? What cost data are to be used?

The easier problem concerns the use of lumber prices in the appraisal work. Here, actual market prices are important. Because of Forest Service access to bills of record for recent sales of firms bidding on Forest Service timber the available data is apparently highly reliable. While such data cover past sales an effort is made by the Forest Service to keep it current through price indexes or price intelligence gathered by its regional specialists. Some may argue that the relevant prices belong to the future, or those ruling *after* the timber is cut and processed. To eliminate a good part of the price risk over the contract period the Forest Service has undertaken to protect the successful bidder by allowing a stumpage price reduction to the extent of one-half the price fall in a particular species or recapturing the same portion of a price rise, so that much of the sting of turning lumber prices is removed.

The treatment of direct costs raises deeper issues. In many cases the logger is obliged to construct special roads (which remain after the timber is removed) to get work crews in and to haul out the felled trees. Copious engineering studies are available; despite intermittent disputes these can approximate the special charges. Likewise, continuous mill studies, and cost data from firms dealing with the Forest Service, are at hand to resolve cost aspects of the problem.[15]

The more perplexing questions are whether in making its appraisals the Forest Service ought to use a national, a regional, or a more peculiarly local average of costs. That is to say, how should it envisage the "representative firm" of Marshallian theory which bids for its timber? The notion of a national average seems doomed to futility: there is too much diversity between lumber firms to permit this. Too, firms that are independent of Forest Service timber in their operations would have to be excluded since good information on the cost aspects of the latter's operations are not available.[16] Even if the general average of costs is limited to Forest Service purchasers, appraisals on this basis in the one- or two-bidder cases, where the firms are distinctly of above average efficiency, would literally ensure bargains to the purchasers; the results would go far to validate the criticisms of the Comptroller General's

[15] Cost data at the Forest Service are in sufficient detail and quantity as to excite the interest of an economist always seeking, and literally starved, for such information. In my view the materials available would lend themselves to thorough study.

[16] Under existing law the Forest Service is privileged to examine the books of record of firms buying Forest Service stumpage.

office that the stumpage is being underpriced. In general it would seem that the appraisal must be directed to the costs of the "representative" firm of those expected to bid on the particular timber stand being prepared for sale, so that all appraisals must be somewhat individual and unique. In practice, too, the notion of a representative firm is likely to have to give way to some "average" firm created statistically by averaging the typical cost and sales data of prospective bidders unless some firm is clearly typical, as a median or mode, of the array of potential bidding firms.[17]

*Conclusions.* The problems of the Forest Service in appraising and, in so many cases, pricing government timber seems to have little parallel in discussions of welfare economics and the usual marginal-cost-equals-price optimal pricing rule. In the customary analyses the pricing mechanism serves an allocative function whereas in the stumpage case this is not the problem: the timber resources to be marketed are decided by both economic and extra-economic considerations, not least those of forestry management and silvicultural needs.

The real issue thus centers in income division, in how the producer-consumer surpluses are to be shared. On this matter contemporary welfare analysis is scarcely helpful. Inevitably, when the cutting edge of analysis is blunt, private pressure and political philosophy are likely to govern the outcome.

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[17] This places one added difficulty in the way of the use of a uniform profit ratio or mark-up. If in different locations there are important variations in the use of capital equipment relative to labor, then it is rather illusory to suppose that a common profit ratio can be sustained. Any attempt to do so will come to grief in view of the acknowledged greater "profit" needs of large firms using expensive equipment as against the small family-size operator. Hence profit ratios as well as price and cost data must be fairly individual, applicable as it were to each stand and group of prospective buyers rather than circumscribed for the whole industry.

* The author is professor of economics at the University of Pennsylvania. This article represents a development of ideas derived while serving as a consultant to the United States Forest Service. Needless to say, this article is not in any way a government paper; my report [5] was submitted in the summer of 1958.

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## The Cost of Capital, Corporation Finance, and the Theory of Investment: Comment

In their recent article [1] Modigliani and Miller develop a theory which essentially asserts that "the average cost of capital to any firm is completely independent of its capital structure" [1, pp. 268-69]. We question the validity of the theory because it is dependent upon the unsupported assumption that investors have available certain arbitrage opportunities. These opportunities in fact are not available.

Proposition I, which is basic to the theory, states that:

$$V_j \equiv (S_j + D_j) = \overline{X}_j/\rho_k \text{ for any firm } j \text{ in class } k$$

where $V_j$ stands for the "market value" of the firm; $D_j$ for the market value of its debts; $S_j$ for the market value of its common stock; $S_j + D_j$ for the market value of all its securities; $\overline{X}_j$ for its expected return, and $\rho_k$ for the capitalization rate "appropriate to its class" [1, p. 268].[1]

The "equivalent" of this proposition appears as:

$$\frac{\overline{X}_j}{(S_j + D_j)} \equiv \frac{\overline{X}_j}{V_j} = \rho_k \text{ for any firm } j \text{ in class } k.$$

In other words, "the average cost of capital to any firm is completely independent of its capital structure and is equal to the capitalization rate of a pure equity stream. . . ." It is the same for all firms in the same class [1, pp. 268-69].

The arbitrage opportunities, upon which the theory rests, are said to arise in the following manner. If $V_2$, the value of company 2, a levered firm (that is, a firm having debt in its capital structure) is larger than $V_1$, the value of company 1, an unlevered firm, an investor in the shares ($S_2$) of company 2 will sell those shares and acquire the shares ($S_1$) of company 1. He can acquire $S_1$ by utilizing proceeds from the sale of $S_2$ and by borrowing on his own credit, pledging the shares ($S_1$) as collateral. The assumption is that he can borrow at the same interest rate as company 2 incurs on its corporate debt ($D_2$). By thus supplying his own leverage through his personal indebtedness, the investor can increase his income as long as $V_2$ is greater than $V_1$. The sale of $S_2$ depresses its price and hence $V_2$, while the purchase of $S_1$ raises its price and $V_1$. When $V_2$ and $V_1$ are equal, there is equality of income from investments in company 2 and company 1, and at that juncture the cost of capital ($\rho_k$) to the two firms is also equal.

This arbitrage operation is admissible only if $S_1$ and $S_2$ represent the same thing. In fact $S_1$ and $S_2$ differ from each other. $S_2$ is junior to the debt of company 2, while $S_1$ is subject to no similar debt of company 1. It cannot, therefore, be logically assumed that the investor will sell one and buy the other even if he can increase his income by doing so. It is, of course, obvious that $S_2$ represents an investment of greater risk than does $S_1$; the authors,

[1] The theory assumes the division of firms into classes and applies to firms in any given class $k$. Firms in the same class have equivalent returns (not necessarily of the same scale) and are in all respects similar except in respect of capital structure [1, pp. 266, 267].

however, seem to regard risk as a subjective factor which should be excluded from consideration [1, p. 279], but even if risk is ignored, the arbitrage operation is inadmissible because $S_1$ and $S_2$ represent different investments.

Furthermore these two investments are associated with different *rates* of return, as the following illustrates: Suppose that the expected annual earnings of each of the two companies are $1000; that each is capitalized at $20,000; that company 2 has $10,000 in debt at 3 per cent interest and $10,000 in stock ($S_2$), while company 1 has $20,000 in stock ($S_1$) and no debt. Earnings applicable to $S_2$ amount to $700 while $1000 is applicable to $S_1$. Now suppose the market value of $S_2$ rises to $12,000.[2] At that value the yield is about 5.83 per cent. At the same time the yield on $S_1$ is 5 per cent. An investor may reasonably consider $S_2$ at the higher rate of return as desirable as $S_1$ at the lower rate. Surely it is a purely gratuitous and unsupported assumption that the investor will in this situation necessarily sell $S_2$ and buy $S_1$ even if thereby he can increase his income. To do so he is obliged not only to enlarge his aggregate investment in $S_1$ (as compared with his investment in $S_2$) but also to incur a personal debt, which is not associated with his investment in $S_2$. Thus there are no arbitrage opportunities and therefore no tendency toward equality of $V_1$ and $V_2$. The proposition that "the average cost of capital to any firm is independent of its capital structure" is devoid of proof.

The authors assert that their theory relies "merely on the fact that a given commodity cannot consistently sell at more than one price in the market . . ." [1, p. 279]. Apparently the "given commodity" referred to is the equity stream of the two firms. But it is not the equity stream which is the subject of the arbitrage operation, it is $S_2$ and $S_1$; and while the equity streams of the two companies may conceivably be regarded as the same "commodity," $S_2$ and $S_1$ cannot be so regarded.

A useful theory purporting to explain the impact (if any) of capital structure on the cost rate of capital must necessarily explain the influence (if any) of capital structure on the interest rate on corporate debt and on the earnings-price ratio of stock. The proposed theory offers no such explanation.

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[2] This assumption follows the illustration in the article offered as proof of Proposition I [1, p. 269].

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REFERENCE

1. F. MODIGLIANI and M. H. MILLER, "The Cost of Capital, Corporation Finance and the Theory of Investment," *Am. Econ. Rev.*, June 1958, *48*, 261-97.

## The Cost of Capital, Corporation Finance, and the Theory of Investment: Comment

In a recent contribution on security valuation and the cost of capital Franco Modigliani and M. E. Miller [11] (hereafter MM) have enunciated three

propositions that contradict widely accepted beliefs and some earlier conclusions of mine [2]. This paper will not attempt to deny these propositions in their own properly limited theoretical context. Instead, it will analyze MM's underlying assumptions, which are subtle and restrictive; and it will indicate some of the difficulties of using these assumptions to support [11, p. 264] "an operational definition of the cost of capital and a workable theory of investment."

Of MM's propositions the first is basic. Proposition I states that, in a perfect market, the total value of all outstanding securities of a firm is independent of its capital structure. More specifically, if firm $j$ has outstanding stock and debt instruments valued at $S_j$ and $D_j$, and if its expected average earnings are $\overline{X}_j$ before interest, then the total value of its securities [11, p. 268, equation (3)] is

$$V_j \equiv (S_j + D_j) = \overline{X}_j/\rho_k,$$

where $\rho_k$ is a constant capitalization factor for all firms in "equivalent return" class $k$. In such a class, which [11, p. 266] "plays a strategic role in the rest of the analysis," future net income per share of all firms, though subject to unknown variations, are certain to be perfectly correlated, each with every other.[1]

There are at least four devices that will aid in building a foundation under Proposition I. One is to assume that arbitrage is possible between securities in an equivalent return class. Another is to assume that a "firm" falls into none of the standard categories—proprietorship, partnership, or corporation—but is a sort of hybrid, having marketable securities like a corporation, proration of income like a partnership, and allocation of financial responsibility like neither. A third is to exclude risk. And a fourth is to assume a long-run equilibrium in which stocks sell at book value. But all of these devices are unrealistic, and MM have accepted none of them wholeheartedly. Thus, while they speak of arbitrage, MM describe a process that is not arbitrage at all, but a switch. Or again, they include all firms in one category—calling them corporations, but failing to endow them with distinctively corporate characteristics. They admit risk to the extent of minor uncertainties, but not major hazards. And finally, although they do not discuss the relation between stock prices and book value, some but not all of their treatment of dividend policy seems to assume that stocks sell at book value. This paper will expose the difficulties of justifying Proposition I for real corporations in a world where arbitrage is usually impossible, where substitutes for arbitrage are restrained and risky, and where stocks rarely sell at book value.

[1] MM offer a specific definition for an equivalent return class of debt-free firms [11, p. 266]: namely, "that the return on the shares issued by any firm in any given class is proportional to (and hence perfectly correlated with) the return on the shares of any other firm in the same class." Although MM are not equally specific for indebted firms, the obvious extension is that their returns are perfectly correlated ($Y = A + BX$) but not proportional ($Y = BX$).

### I. *Proposition I and Arbitrage: An Illustrative Example*

True arbitrage, when possible, is a powerful equalizer; but it is not ordinarily possible in corporate securities. To clarify the issue, this section presents an artificial example of an equivalent return class of corporations in which the possibility of arbitrage will practically guarantee Proposition I; subsequent sections will show how this example fails to account for the real problems of corporation finance.

Petrolease is a fictitious corporation whose business consists in leasing oil properties; it earns $10 per share on the average, all of which it pays out in dividends. Leverfund is a fictitious open-end investment trust, whose assets consist solely of Petrolease shares. It operates under the following conditions, some of which are unusual: (1) for every share of Petrolease that it holds, it must have outstanding one share of its own stock and one $100 bond paying interest at 5 per cent; (2) it incurs no expenses and pays out all earnings; (3) as an open-end fund, it will issue on demand one share and one bond while simultaneously acquiring, by purchase or exchange, a Petrolease share, or it will redeem on demand one share and one bond while divesting itself of a Petrolease share; (4) since it makes no loading charge, the buyer (or seller) of a bond and share pays (or receives) the same combined price that Leverfund pays (or receives) for a share of Petrolease; (5) in the open market, securities of both corporations trade without commissions. As a result of conditions (1) and (2), the income per share of Leverfund averages $5 and is exactly equal to the income per share of Petrolease minus $5. Thus, the incomes per share of Leverfund and Petrolease are perfectly correlated, and the two corporations must belong to the same equivalent return class.

The peculiarities of Leverfund and Petrolease guarantee that an approximation to Proposition I will hold between them; together a bond and share of the one must sell for about the same price as a share of the other. Any discrepancy will provide traders with opportunities for profit, the exploitation of which will tend to reduce, and perhaps eventually eliminate, the discrepancy.

The most spectacular opportunity is through arbitrage. If a bond and share of Leverfund are selling in the open market for more than a share of Petrolease, a short-term trader can realize a quick profit by buying, say, 100 shares of Petrolease and simultaneously selling short 100 bonds and shares of Leverfund. Then, at his leisure, he exchanges his long Petrolease shares for Leverfund securities and delivers the latter against his short commitment. A single aggressive arbitrager can thus exert terrific equalizing pressure, for he can continue his operations just as long as any discrepancy remains—even to the extent of buying up and exchanging all outstanding shares of Petrolease.

But note, the effectiveness of this arbitrage depends upon equivalence in exchange between a share of Petrolease and a bond and share of Leverfund, not upon equivalence of income.[2] If, for example, personal income from bond

[2] Arbitrage between Petrolease and Leverfund belongs to the category of "kind" arbitrage. According to Weinstein [14, p. 2], a transaction of this type is defined as "the simultaneous purchase and sale of equivalent articles or securities in the same or different

interest were taxed at a lower rate than dividends, astute investors would prefer a bond and share of Leverfund to a share of Petrolease and would regard a price discrepancy as normal. But the arbitragers could still profit by buying Petrolease and selling Leverfund.

MM repeatedly speak of one security as a "perfect substitute" for another merely on the grounds that it represents an equal amount of income. Now, in fact, a share of Petrolease is not a perfect substitute for a bond and share of Leverfund; for one thing, a Petrolease stockholder has no vote for Leverfund directors. But owing to the possibility of exchange, a share of Petrolease is a good enough substitute for a bond and share of Leverfund, and vice versa, to permit the arbitrage operations that assure the validity of Proposition I between the two corporations. The importance of these details becomes apparent if Leverfund is simply transformed into a closed-end trust—bearing, say, the name Closecorp. Then, even though a share of Petrolease still stands behind a bond and share of Closecorp—maintaining income equivalence as with Leverfund—exchange is no longer possible. Neither is arbitrage, and the realization of Proposition I must depend upon other equalizing operations.

One substitute for arbitrage, to be mentioned in passing, is what might be called a hedge position,[8] to provide income without investment. If Closecorp sells at a 5 per cent premium, an operator might sell short 100 bonds and shares, investing the proceeds in 105 shares of Petrolease; then, since the income from 100 shares of Petrolease would suffice exactly to cover interest and dividend requirements on his short position, he would derive as net income the dividends from 5 shares of Petrolease. But this sort of transaction may be hard to arrange, owing to the many restrictions on short selling; and it exposes the operator to numerous risks—including the risk of being caught in a corner (cf. Macaulay [10, pp. vii-viii, 19-20]). Although corners are less frequent than formerly, the recent episode of E. L. Bruce on the American Stock Exchange indicates that some danger is still present.

Another substitute for arbitrage is the switching of investment accounts from comparatively unattractive issues into others that seem to offer a higher return for the same risk. MM commit a common error in confusing switching with arbitrage, but the distinction is important. Arbitrage can be effective if only a few professional traders are alert and aggressive. Switching, however, to be equally effective, may require the active cooperation of a large body of investors.

As an example of switching, suppose that Closecorp stock is selling in the market at 68, Closecorp bonds at 100, and Petrolease stock at 160. According to MM a leverage-loving investor who holds 300 shares of Closecorp stock

markets." Here the meaning of equivalence is rather special, implying that the articles or securities are essentially different but, "through the terms of their issuance or through special circumstance, may become equal to each other." Later [14, p. 66], Weinstein specifies: "By equivalent securities is meant a convertible bond or stock, a right or option warrant, or a stock of one company which may be exchanged for the stock of the same or another company."

[8] Perhaps the use of the term "hedge" is a little loose in this context (cf. Weinstein [14, p. 76]).

outright should sell it and buy Petrolease on margin. A switch arranged according to the table below will achieve an increase of $200 in

| | | | |
|---|---|---|---|
| Before Switching | | | |
| 300 Closed Corp. | at $ 68 | value $20,400 | yielding $1500 |
| After Switching | | | |
| 340 Petrolease | at 160 | 54,000 | yielding 3400 |
| Margin loan at 5% interest[4] | | 34,000 | costing 1700 |
| Net investment | | $20,400 | yielding $1700 |

net income with no loss of stability; for, by arranging to pay in interest exactly $5 of his dividend receipts from each share of Petrolease, which is what Closecorp pays, the investor assures himself that his net income will exhibit exactly the same percentage fluctuations as Closecorp dividends. From such considerations MM [11, p. 270] "conclude therefore that levered companies cannot command a premium over unlevered companies because investors have the opportunity of putting the equivalent leverage into their portfolio [sic] directly by borrowing on personal account." But this is only a limited opportunity for most investors, who are deterred from aggressive margin buying by the legal and institutional restrictions placed upon it (see Section II) or by its intrinsic risks (see Section III).

Because of these deterrents, Proposition I can be no more than an inequality for Petrolease and Closecorp. In simplest terms, Closecorp securities enjoy a wider market than does Petrolease stock. Almost any investor who would buy a share of Petrolease should find a satisfactory—though not perfect—substitute in a bond and share of Closecorp, and he should willingly switch if the substitute fell to a discount.[5] Thus, as long as investors are fairly alert, Closecorp cannot sell much below Petrolease. But the reverse does not hold. Closecorp stock has a special appeal to risk-takers, and its bonds have a special appeal both to the safety-minded and to those barred from buying stock. For Closecorp to command a premium, it suffices that this specialized demand for its bonds and for its shares should exceed the available supply of these securities. After all, the essential difference between Closecorp and Leverfund is the limitation on the amount of securities that the former can issue in the short run. When Leverfund securities command a premium, the arbitragers can immediately increase the supply by buying up and exchanging Petrolease shares; they cannot do this for Closecorp.

In the long run, of course, Closecorp can expand, issuing more bonds and high-leverage shares to meet the specialized demand for these instruments; and in taking this step it will reap the benefits of low cost capital—that is, until it issues enough securities to satisfy the demand. Here, then, is another

[4] MM [11, p. 268] assume that the investor pays the same rate on his "bonds" (i.e., margin loan) that Leverfund pays on its bonds.

[5] There will be a few exceptions—institutions like the College Retirement Equities Fund, which are required to invest only in common stock.

substitute for arbitrage, which MM have neglected—the financial operations of corporations.

Here, also, is a paradox. If those of us who doubt the existence of equilibria advise corporate managers to remain alert and to exploit every opportunity to reduce their cost of capital by adjusting capital structure, we shall help to establish MM's equilibrium; whereas MM, by offering assurance that these opportunities do not arise, are sabotaging their own goal. Perhaps a realistic resolution to the paradox would be to recognize that any particular functional relation between total security value and capital structure is unlikely to remain stable over the long run—especially if it affords corporations an obvious opportunity to minimize capital cost by adopting one particular structure. Once the relation is widely recognized, everyone will attempt to exploit it; and a new relation, though not necessarily equilibrium, will result.

In practice this means that those who seek to reduce capital cost by adjusting capital structure must ascertain conditions in the current market, and act rapidly to exploit them. For instance, the stock market decline in September 1946 inaugurated a period of several years in which bond yields were low and stock yields were high. For the first quarter of 1950, when [2] was in preparation, Standard and Poor's reported yields of 2.6 per cent for high-grade utility bonds as against dividend yields of 5.35 and earnings yields of 8.1 per cent for utility stocks. Those levels offered corporations a golden opportunity to finance with bonds. Nine years later, in the first quarter of 1959, bond yields were up to 4.3 per cent, while dividend yields and earnings yields were down to 3.9 and 5.4 per cent. The ratio of bond yields to stock yields had more than doubled. Thus, utility companies that availed themselves of the opportunity to sell bonds on comparatively favorable terms in 1950 and stock on comparatively favorable terms in 1959 must have acquired their capital at appreciably lower cost than those selling stock in 1950 and bonds in 1959.

### II. *Market Imperfections: Restrictions on Margin Buying*

MM visualize a highly competitive market, almost but not quite free of restrictions. Specifically, they write [11, pp. 280-81]:

> Those who hold the current view—whether they realize it or not—must assume not merely that there are lags and frictions in the equilibrating process—a feeling we certainly share, claiming for our propositions only that they describe the central tendency around which observations will scatter—but also that there are large and *systematic* imperfections in the market which permanently bias the outcome.

Between the "current view" and their own, MM have left much middle ground. Whenever the market deviates from equilibrium, it provides someone with an opportunity for profit. But whom? MM's development of Proposition I, whether they realize it or not, accords investors exclusive rights to profit by giving them full responsibility to correct deviations from equilibrium. But why discriminate against the corporations? One likely reason, not mentioned by MM, is that market imperfections prevent corporations from issuing or

redeeming securities as fast as investors can switch accounts. But even so, market deviations may resist investors' corrective actions long enough for corporations to partake of the profits, unless the investors' speed is coupled with adequate volume; and a host of institutional restrictions limit the volume of switching operations that investors can arrange on short notice—especially switching from high-leverage stocks held outright into low-leverage stocks on margin, which is MM's prescribed corrective when high-leverage stocks command a premium.

Margin requirements under Regulation T can place a very substantial legal limitation on the volume of corrective margin switching that investors can generate. In the previously mentioned switch from 300 Closecorp outright at 68 into 340 Petrolease on margin at 160, the investor's equity was but 36 per cent of the total holding, which would be legally insufficient under the 90 per cent limitation in effect when this comment was prepared, or even under the 50 per cent limitation when MM published [11]. Of course, an investor with a large portfolio could legalize the transaction by hypothecating some of his other securities against his loan—but only at the expense of reducing his ability to take advantage of similar switching opportunities between other pairs of stocks.

Moreover, even the small amount of margin buying permitted some investors under Regulation T is withheld from many of our largest investors, the institutions. Mutual funds, fire and casualty companies, closed end trusts, life insurance companies, and most personal trust funds are prevented from buying stocks on margin—either by direct prohibitions in their charters or by the rather general acceptance of the prudent man rule. Together these institutional investors command a tremendous volume of investable or invested funds, most of which are simply not available for the purchase of low-leverage shares on margin, even when they fall to a discount.

MM suggest [11, pp. 278-81] that the restrictions under which institutional investors operate are, in effect, only a vehicle through which individual investors express their subjective risk preferences; cautious individuals commit their funds to conservative financial intermediaries. But this is only half the story. Nowadays, investment trusts are set up to pursue a variety of goals. An investor who wishes to plunge can entrust his savings to a high-leverage trust. The trust achieves its leverage not by buying on margin, but by selling bonds; and although the two operations are equivalent in many respects, there is an important difference. A bond issue requires time to arrange! So, if the market remains out of equilibrium long enough to provide investment trusts with extraordinary opportunities to profit by selling bonds, nonfinancial corporations can enjoy the same opportunities.

To sum up, those of us who take a middle-of-the-road view believe neither in a permanent equilibrium nor in a permanent and consistent departure from equilibrium. We suspect that switching operations by investors—hampered as they are by margin restrictions, brokers' commissions, tax considerations, and other institutional limitations—are insufficient in volume to maintain the market anywhere near equilibrium; and we regard the financing operations of nonfinancial corporations as an integral part of any

equilibrating process that may be in operation. Even though corporations cannot act with the speed of a floor trader or of an aggressive investor, they can deal in large sums when they do act. Indeed, in June 1958, when MM's paper [11] appeared, margin requirements were at a 3-year low; yet the volume of loans by all commercial banks for purchasing or carrying securities ($5.6 billion reported by the Federal Reserve Board) was slightly less than the funded debt of just one corporate system ($5.7 billion for American Telephone and Telegraph).

### III. *The Risks of Margin Buying*

MM would argue [11, p. 269] that the investor who switches from an outright position in Closecorp to a margined position in Petrolease incurs no additional risk, because he merely exchanges "one income stream for another stream, identical in all relevant respects but selling at a lower price." But this argument does not apply to corporate stockholders in a world of high risk, though it might apply either to stockholders in a world of low risk or to limited partners in a world of high risk. If Petrolease earnings were absolutely certain to remain above $5 per share, fluctuating only slightly about the $10 average, the income stream from Petrolease on margin would certainly equal the stream from Closecorp outright, and neither the margin lender nor the Closecorp bondholder would run any risk of default. Possibly MM had such safety in mind when they wrote [11, p. 268]: "All bonds (including any debts assumed by households for the purpose of carrying shares) are assumed to yield a constant income per unit of time, and this income is regarded as certain by all traders regardless of the issuer." But this is a strange statement for anyone who addresses himself to the question [11, p. 261]: "What is the 'cost of capital' to a firm in a world in which funds are used to acquire assets whose yields are uncertain, . . .?"

In a world where yields are really uncertain, Petrolease income may fall below $5 per share. Indeed, it may cease entirely if the oil wells run dry. If Closecorp were specially chartered as a limited partnership or joint venture with pro-rata allocation of responsibility, the outright holder of Closecorp stock would owe, in the event of financial disaster, as much to Closecorp bondholders as the margined holder of Petrolease would owe to his bank or broker. But as a corporate stockholder, the leverage-loving investor with 300 shares of Closecorp at 68 enjoys most of the benefits of a levered position in Petrolease without all of the attendant risks; for he has limited his liability to the amount of his investment, $20,400. If, however, he follows MM's advice and switches into 340 shares of Petrolease on margin at 160—regulations permitting—he incurs a liability of $34,000 for his margin loan, and his maximum loss increases to $54,400. In practice, of course, he runs little risk of losing the entire $54,400; his bank or broker, in self defense, will sell him out before Petrolease becomes worthless. But this protection against maximum loss greatly increases the risk of lesser loss. In the days when promiscuous margin buying was permitted, a temporary price decline in a generally rising market often resulted in the liquidation of over-extended accounts.

### IV. *The Problem of Retention and Growth*

In the switching example, income equivalence between Closecorp and Petrolease depended on the assumption of 100 per cent dividend payout. This would be completely realistic for partnerships, since the law considers that a partner receives his pro-rata share of the firm's income, whether paid out or not; and it is almost realistic for such corporations as regulated investment trusts. But it will not do for corporations at large. MM do not actually assume 100 per cent payout; instead they say [11, p. 266]:

> As will become clear later, as long as management is presumed to be acting in the best interests of the stockholders, retained earnings can be regarded as equivalent to a fully subscribed, pre-emptive issue of common stock. Hence, for present purposes, the division of the stream between cash dividends and retained earnings in any period is a mere detail.

But what do "present purposes" include? A little thought will show that they do not include the delineation of an equivalent return class, which is the cornerstone of MM's argument. Suppose, for example, that two equity-financed corporations earn regularly a definite rate $\rho^*$ on assets of A, and that they retain no earnings. Then the assets of each will remain constant at A, and the earnings at $A\rho^*$ per year. Earnings per share will be proportional, and the two corporations must belong to the same class. But as soon as one corporation starts to retain earnings and to reinvest them profitably its assets and earnings will begin to increase; and this will suffice to transfer it into a new class, since its earnings per share will now be imperfectly correlated with those of the other corporation.

MM's proposal to regard retentions as a fully subscribed, pre-emptive stock issue will not avoid this difficulty unless stocks sell at book value. Before the corporation of the preceding paragraph started to expand, it earned $A\rho^*$—or $A\rho^*/N$ per share on $N$ shares having book value $B_o = A/N$. With a market capitalization rate of $\rho_k$, the stock had a price of $P_o = A\rho^*/\rho_k N$ and a ratio to book value of $P_o/B_o = \rho^*/\rho_k$. Now, in the first year of its expansion, the corporation retains and reinvests $I = A\rho^* X$ to yield the same rate $\rho^*$; and this enables it to earn $A\rho^*(1 + \rho^* X)$. MM wish to regard $I$, the amount retained, as the proceeds of a stock sale, but they neglect to set the price $P_f$ at which this hypothetical transaction is to take place—a detail, perhaps, but an important one! Except when $\rho^* = \rho_k$ and $P_o = B_o$, no single price $P_f$ will meet both of two requirements: first, to maintain earnings per share unchanged and thus keep the corporation in the same class; and second, to provide a genuine equation between the amount retained and the hypothetical stock issue.

A price of $P_f = B_o$, for example, will meet only the first requirement. At this price the corporation must issue new shares numbering:

$$\frac{I}{B_0} = \frac{A\rho^* X}{A/N} = \rho^* N X, \tag{1}$$

and these together with $N$ old shares will show the same earnings,

$$\frac{A\rho^*(1 + \rho^*X)}{N(1 + \rho^*X)} = A\rho^*/N, \tag{2}$$

as before expansion. But the price $P_f = B_o$ will not meet the second condition, because the hypothetical subscriber to this stock issue is, in effect, forced to acquire shares at $B_o$ when the market price is $P_o$; and he must either enjoy a profit $P_o - B_o$ or suffer a loss $B_o - P_o$.

Finding a price $P_f$ to meet the second requirement, by equating a dollar's worth of retentions to a dollar's worth of issued stock, is difficult. Such a price will depend, when $\rho^* \neq \rho_k$ and $P_o \neq B_o$, on the entire future growth of the corporation; and to obtain a solution, one must stipulate an arbitrary growth pattern. From the standpoint of algebra, one of the more tractable patterns is uniform growth in perpetuity, resulting from the retention and reinvestment each year of a fraction $X$ of earnings to yield precisely $\rho^*$. Then the required price is given by:

$$P_f = \frac{A\rho^*(1 - X)}{(\rho_k - \rho^*X)N}, \tag{3}$$

which is a standard actuarial formula for the present value of an income stream starting at $A\rho^*(1 - X)/N$, growing at a rate $\rho^*X$, and discounted at $\rho_k$ (cf. Todhunter [13, pp. 48-49]). Clearly $P_f$ is independent of retentions only when $\rho^* = \rho_k$; then $P_f = A/N \equiv B_o$. But when $\rho^* > \rho_k$, (3) gives $P_f > B_o$, with $P_f$ approaching infinity as $\rho^*X$ approaches $\rho_k$. Thus in addition to resting on highly artificial assumptions, (3) sometimes leads to absurdities (cf. Durand [5]). Williams [15, pp. 89-94, 129-34] suggests other growth formulae, which are also artificial but avoid the absurdities.

Whether the corporation fails or succeeds in finding, for its hypothetical stock issue, the exact price $P_f$ that meets the second requirement, any choice of $P_f \neq B_o$ will affect its earnings per share and transfer it into a new class.[6] Hence, if MM wish to include in their equivalent return classes companies expanding at various rates, or if they wish to reduce the division of corporate income between dividends and retained earnings to a mere detail, then they must assume some sort of long-run equilibrium in which $\rho^* = \rho_k$ and $P_o = B_o$. Indeed, they have explicitly [11, pp. 288-91] assumed $\rho^* \geqslant \rho_k$, on the grounds that investments yielding $\rho^* < \rho_k$ are detrimental to the stockholders. They could equally well have ruled out $\rho^* > \rho_k$ on grounds of competition; if an area of investment opportunity yields $\rho^* > \rho_k$, it will attract additional capital until the discrepancy disappears. Such an argument should be no great hurdle for those who believe that the presence of "large and *systematic*

[6] Possibly this argument contains a subtlety that requires explanation. A corporation can, of course, start in a given equivalent return class, go through a period of expansion, and return to the same class at the close of its expansion—even if it earns $\rho^* > \rho_k$ on its investments. But this is not the point. It is the prospect of expansion, not the act itself, that causes the difficulty. Oddly enough, MM recognize [11, p. 290, fn. 50] that a proposed expansion may, because of its potential effect on earnings per share, influence stock prices; but they seem to have overlooked the havoc that this kind of anticipation plays with their concept of an equivalent return class.

imperfections in the market which permanently bias the outcome . . . is an assumption that economists, at any rate, will instinctively eye with some skepticism" [11, p. 281].

Although an outright assumption that $\rho^* = \rho_k$ and $P_0 = B_0$ would have done much to shore up MM's theoretical argument, it would have done nothing to establish this argument as realistic or operational. In the operating world stocks do not sell at book value—not even approximately. A list recently released by the New York Stock Exchange [16] shows great variation in popular stocks—ranging from New York Central, at about one-tenth book value to International Business Machines at about seven times. This is one kind of evidence of market imperfection that is easy to obtain.

In an expanding economy where stock prices deviate from book value, the interaction between the growth rate and the ratio of price to book value is an important dynamic factor bearing on security values and the cost of capital. Writing on the utility industry, where the problem is well recognized, Tatham

TABLE 1—REGRESSION ANALYSIS OF THE STOCKS OF 25 LARGE BANKS OUTSIDE NEW YORK CITY, EARLY 1953

| Regression Equations | Residual Sum of Squares |
|---|---|
| $\log_{10} P = .07 + .97 \log_{10} B$ | .151298 |
| $\log_{10} P = .54 + .49 \log_{10} B + .54 \log_{10} E$ | .094283 |
| $\log_{10} P = .96 + .22 \log_{10} B + .91 \log_{10} E + .70 \log_{10} D/E$ | .036081 |

[12, p. 36] points out that utility stocks in recent years have generally commanded a premium over book value, thus assuring investors of a "potential increase in value and earnings during a period of expansion," which "provides one of the basic attractions of utility common stocks for investment purposes." By this "potential increase" Tatham refers specifically to growth in earnings per share resulting from successive issues of stock above book value; but he could have extended his argument to include expansion financed by borrowing or by retained earnings.[7] All this means that although retentions can often be regarded as a fully subscribed, pre-emptive stock issue, the division of earnings between dividends and retentions is anything but a mere detail when stocks do not sell at book value.

That investors do not in fact regard dividend policy as a mere detail is now attested by considerable empirical evidence (cf. Durand [3] [4], Gordon [6], Graham and Dodd [8, Ch. 34], Johnson, Shapiro, and O'Meara [9]). Table 1 summarizes a regression analysis of price $P$ on book value $B$, earnings $E$, and dividends $D$, for a group of 25 bank stocks—one of six groups analyzed recently.[8] The special arrangement in the table is designed to show the

[7] In addition to Tatham, Durand [4, Ch. 4] and Gordon and Shapiro [7] describe the mechanism by which this "potential increase" is realized and discuss its implications for the analysis of cost of capital. MM [11, pp. 288-91] describe the mechanism very clearly, but neglect the implications.

[8] For further analysis, see Durand [4]. The original data for this example appear in [4, p. 29, Table 5].

successive reductions in the residual sum of squares due first to earnings, $E$, and then to the ratio of dividends to earnings, $D/E$. In particular, the reduction due to $D/E$ (i.e., .094283 − .036081 = .058202) is far too great to attribute to chance (since the $F$-ratio .058202 × 21/.036081 = 33.87 far exceeds even the .001 point for 1 and 21 degrees of freedom) and indicates that the payout ratio exerts a significant influence on price—even after the combined influence of book value and earnings have been taken into account. Table 2 summarizes regression analyses of four groups of public utility stocks.[9] Here the ratio of price to book value, $P/B$, is related to earnings over book value, $E/B$, and dividends over earnings, $D/E$. The reduction in the sum of squares is highly significant in the first two groups, but not at all significant in the last two. The combined experience of all groups taken together is significant.

MM are inclined to scoff at evidence like the above, arguing that [11, pp.

TABLE 2—REGRESSION ANALYSIS OF FOUR GROUPS OF PUBLIC UTILITY STOCKS (20 STOCKS PER GROUP), EARLY 1955

| Group | Regression Equations | Residual Sum of Squares |
|---|---|---|
| I. Northeast | $\log_{10} P/B = 1.18 + .96 \log_{10} E/B$ | .031033 |
| | $\log_{10} P/B = 1.32 + 1.04 \log_{10} E/B + .60 \log_{10} D/E$ | .017997 |
| | $F = 17 \times .013036/.017997 = 12.314$ | .013036 |
| II. Midwest | $\log_{10} P/B = .90 + .70 \log_{10} E/B$ | .019481 |
| | $\log_{10} P/B = 1.16 + .90 \log_{10} E/B + .46 \log_{10} D/E$ | .011770 |
| | $F = 17 \times .007711/.011770 = 11.137$ | .007711 |
| III. West | $\log_{10} P/B = .97 + .77 \log_{10} E/B$ | .042596 |
| | $\log_{10} P/B = 1.11 + .87 \log_{10} E/B + .28 \log_{10} D/E$ | .040625 |
| | $F = 17 \times .001971/.040625 = .825$ | .001971 |
| IV. South | $\log_{10} P/B = 1.13 + .90 \log_{10} E/B$ | .049213 |
| | $\log_{10} P/B = 1.27 + 1.00 \log_{10} E/B + .30 \log_{10} D/E$ | .041994 |
| | $F = 17 \times .007219/.041994 = 2.922$ | .007219 |

287-88] "in a world in which the policy of dividend stabilization is widespread, there is no simple way of disentangling the true effect of dividend payments on stock prices from their apparent effect, the latter reflecting only the role of dividends as a proxy measure of long-term earning anticipations." Are not MM trying to extinguish the fire by pouring on more fuel? In addition to the true effect, they introduce an apparent effect, which may also influence stock prices and the cost of capital—presumably by creating or correcting wrong impressions. In the earlier and less responsible days of Wall

[9] These analyses come from a larger study on public utility stock prices and the cost of capital, financed by a grant from the Sloan Research Fund of the School of Industrial Management, Massachusetts Institute of Technology. The main results have yet to be published. The computations were performed on the I.B.M. 704 computer at the M.I.T. Computation Center.

Street, an unscrupulous insider, like General Daniel E. Sickles of the Erie (cf. Dewing [1, p. 744, fn. 6]) might often attempt to mislead the public by manipulating dividends and issuing false earning statements. And even today, when blatant manipulation is far less common, psychological influences remain. If a conscientious corporation manager believes that the public is underestimating the earning power of his company and that a dividend change might improve the public's estimate, should he not make the change? There are many ways, some devious, in which dividend policy can influence stock prices and the cost of capital; and the available evidence indicates conclusively that at least some of them are effective.

Even if the available evidence does not suffice to disentangle all these influences, it offers some interesting hints. The bank stocks covered by Table 1 had an average price about 5 per cent above book value—with a range from 22 per cent below for The National Shawmut to 83 per cent above for the Bank of America. The utility stocks covered by Table 2 all sold above book value—with a range from 11 per cent for consolidated Edison and Pennsylvania Water and Power to 222 per cent for Scranton Electric. With stocks selling above book value, a dollar retained is worth more than a dollar paid out—as implied earlier in this section—and investors seeking long-term appreciation should prefer stocks paying low dividends. If these investors dominate the market, negative regression coefficients for $D/E$ are the natural result. The positive coefficients in Tables 1 and 2 imply dominance by another type of investor—possibly one who looks at dividend data instead of analyzing the earnings account, but more probably one with a genuine need for regular dividend income (cf. Durand [4, p. 47]). After all, there is a difference between capital gains and income—and especially between unrealized capital gains and realized income. In effect, our evidence on dividend policy is just good enough to be frustrating; it leaves us no doubt whatsoever that dividend policy exerts an influence on stock prices and the cost of capital, while failing to explain precisely how the influence is exerted.

## V. *Problems of Empirical Analysis*

The empiricist who would investigate the cost of capital to corporations will encounter a host of obstacles, and among the first will be the gathering of reliable and pertinent data. He will find price quotations sometimes hard to acquire and often erratic or nominal—particularly quotations for corporate bonds, most of which are rather inactively traded nowadays, and some of which, the so-called private placements, are not traded at all. He will find dividend rates, although easy enough to ascertain for regular payers like American Telephone and Telegraph, very troublesome when irregular payments, stock dividends, and extras are the rule. And he will find earnings even harder than dividends to ascertain precisely, for they are subject to the vagaries of accounting practice as well as the vicissitudes of business conditions.

Another obstacle, which is crucial for MM's approach, is the difficulty of assembling a sample of corporations capable of supporting a comparative, or cross-section, type of analysis. The empirical analyst will be unable to assemble any sample meeting the rigid requirements of MM's equivalent class;

but this in itself is hardly disastrous, since samples showing no variation in dividend policy and growth rate will not yield much information. The real difficulty is to find samples that are reasonably homogeneous in most respects, and yet show enough variation in growth rate, capital structure, and the like to bring out the influence of these factors. One can often find two or three, and sometimes more, corporations with characteristics sufficiently uniform to bear comparative analysis—an approach long known and used by security analysts. But if an analyst restricts his samples in order to keep them homogeneous, he must perforce keep them small; and if he attempts to expand them to the point where they are numerically satisfactory, he must pay a price in lost homogeneity. This problem arose recently in a study of 117 bank stocks (Durand [3] and [4]). A sample of 117 would be large enough to support a respectable cross-section analysis—if it were homogeneous. But a division of this sample into six subsamples, coupled with some tests for heterogeneity, revealed striking differences. Within the subsamples, moreover, there was further evidence of heterogeneity; in fact, a surprisingly large number of banks exhibited characteristics that rendered them virtually unique [4, pp. 19-20 and 60-62].

Taken all together these obstacles of sparse quotations, uncertain dividends, ambiguous earnings, and heterogeneous stock groups rather narrowly limit the ability of the empirical analyst to detect, let alone measure, the various factors affecting cost of capital. Indeed, only the strongest factors are likely to be discernible through the haze of unwanted perturbations; the subtler ones easily remain unnoticed. To date, in fact, dividend payout is one of the few factors that have proved strong enough to be repeatedly discernible; yet its specific influence is neither clear-cut nor easy to interpret. The available evidence appears inadequate to answer most of the interesting questions. Does the typical positive correlation between price and payout imply that investors are using dividend data to forecast company prospects or merely expressing an honest preference for cash income? To what extent is the price-payout relation obfuscated by growth-conscious investors, who prefer retentions to income? Can one measure reliably the price-payout relation for individual companies? And how does the conscientious manager set dividend policy to accommodate a medley of present and potential stockholders in greatly varied personal circumstances and tax brakets?

The influence, if any, of leverage on cost of capital has so far escaped detection in cross sections—both in the oils and utilities mentioned by MM, and in the bank stocks. But in view of the difficulties of empirical analysis, this is merely evidence of lack of evidence. On MM's scatter diagram [11, p. 283, Fig. 3], relating cost of capital to financial structure for 43 utilities in 1947-48, the ratio of "total earnings after taxes" to "market value of all securities" (i.e., cost by MM's definition) ranges roughly from 4¼ to 7½ per cent, with well over a third of the observations falling outside the range from 5 to 6½ per cent. In the face of so much scatter, could anyone be assured of detecting a consistent variation of, say, ¼ per cent? And a variation in capital cost of this magnitude would not be financially insignificant to a corporation manager or a public utilities commission. On assets of

$1,000,000,000 savings of ¼ per cent would amount to the tidy sum of 2,500,000 per year. I submit that MM's apparently negative cross-section evidence is essentially inconclusive—especially when history provides positive evidence of periods like 1948-50, which were unusually favorable to bond financing, and others like 1958-59, which were unusually favorable to stock financing. The real significance of the lack of evidence in these cross-sections is to warn us that many important questions in corporation finance, the cost of capital, and the theory of investment are not easily answered with available data.

## VI. *Conclusion*

MM have cut out for themselves the extremely difficult, if not impossible, task of being pure and practical at the same time. Starting with a perfect market in a perfect world, they have taken a few steps in the direction of realism; but they have not made significant progress, considering their avowed purpose of achieving an "operational definition of the cost of capital." Their treatment of risk affords, perhaps, the clearest example. In allowing corporate earnings to fluctuate somewhat—presumably about a fairly definite central value—MM have postulated a world that is not 100 per cent riskless; but it is a remarkably safe world—being free from the risk of bond default, margin calls, foreclosures, or major disasters of any sort. In a world so safe, the effect of risk on the cost of capital, corporation finance, or the theory of investment is not apparent.

Or again, MM's treatment of the equilibrating mechanism in an imperfect market is unrealistic and also inconsistent. MM have endowed investors with unrestricted freedom to switch accounts whenever the market deviates from equilibrium; but for undisclosed reasons they do not extend equal freedom to corporations. If MM wish to assume a perfectly free market, they should realize that corporations unhampered by the practical costs and delays of issuing or redeeming securities can immediately exploit any departure from equilibrium, and it is then arguable that investors can never profit by switching accounts because corporate financing activities will maintain an equilibrium in which profitable switching is impossible. To be realistic, we must recognize that switching operations by investors, financing operations by corporations, and even arbitrage operations where possible, are all subject to restrictions—though not the same ones—and that each of these operations will exert some leveling effect on the market in spite of restrictions. A conscientious equilibrium theorist should advise, nay urge, all interested operators—investors or corporations—to exploit their available opportunities as vigorously as possible. MM, however, advise corporations that there is no opportunity to reduce cost of capital by judicious adjustment of capital structure; and thus they promote complacency, a form of market imperfection.

Finally, and most important, MM have underestimated the difficulty of setting up an equivalent return class, which is the cornerstone of their theory. To the practically minded, it is unthinkable to postulate the existence of two or more separate and independent corporations with income streams that can fluctuate at random and yet be perfectly correlated from now until doomsday;

and the artificial example of Petrolease and Closecorp, which are not completely independent, provides no exception. But the difficulty goes much deeper. The concept of an equivalent return class, derived from notions of static equilibrium, is not adaptable to a highly dynamic economy in which stocks do not sell at book value.

Indeed, MM's approach to the cost of capital, as a ratio of current earnings to market price, is essentially static. Dynamically speaking, the cost of capital should measure the inducement—in terms of current earnings *plus long-term growth potential*—required to attract new investment fast enough to meet the needs of an expanding economy. This is the approach for a public utility commission desirous of assuring service in a growing community, for a bank supervisor worried lest the deterioration of bank capital ratios may jeopardize the ability of banks to finance expansion, and certainly for anyone concerned with the ability of this nation to maintain its position of economic leadership.

It is not easy to formulate an operational definition of the cost of capital for a dynamic economy where markets are imperfect, where price-to-book-value ratios vary from one-tenth to seven, and where investors and their advisory services discriminate between income and appreciation. Indeed, it may turn out to be impossible. But we shall make more progress in the long run by frankly recognizing the obstacles to achievement than by accepting false goals attainable by ignoring these obstacles.

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## The Cost of Capital, Corporation Finance, and the Theory of Investment: Reply

In this reply to the two preceding comments, we shall concentrate on certain issues raised by David Durand. To J. R. Rose we can only apologize for having led him astray by our failure to adjust explicitly the definition of a "class" [3, p. 266, par. 2] when we introduced debt financing (Sec. I.B). We should have said more clearly, in the very beginning perhaps, that what determines membership in a class is the stream generated by the *assets* held by the firm, not the stream accruing to the *shares*. The two streams, of course, happen to be completely equivalent in our first special case of no borrowing, but only in that case. We hope the above emendation plus a study of Durand's numerical parable of Petrolease, Leverfund and Closecorp will clear up the misunderstanding.

We do not propose to go over Durand's comment point by point partly for reasons of space, and partly because on many issues we have little to add to (or retract from) what we originally wrote. There are, however, four issues where Durand's comments (plus correspondence we have had with many others) have led us to believe that some further elaboration of our model and our approach to the empirical problems might serve a useful purpose.

Before proceeding to this task, however, we should like to remind readers of the considerable areas of agreement between Durand and us which are easy to overlook in critical exchanges of this kind. Despite his sternly critical tone, he agrees (1) that our conclusions, which "contradict widely accepted beliefs" can be regarded as valid at least "in their own properly limited theoretical context" (see p. 640); and (2) that for all the attention that leverage has received in the literature of finance "the influence, if any, of leverage on the cost of capital has so far escaped detection" (p. 652). Not only is there agreement on these basic matters, but Durand's paper represents the kind of

thoughtful and thought-provoking response we hoped we would get to the invitation in the closing paragraph of our paper. He has probed carefully to find inadequacies in our treatment of perfect markets, and he has endeavored to explore the implications of certain market imperfections for the usefulness of our approach. We feel, however, that he has not been conspicuously successful on either front, largely because he has focused on the apparent limitations of the perfect market model instead of trying to surmount these limitations by extending our basic approach.

## I. *The Ability of Investors and Speculators to Enforce Proposition I*

Section I of Durand's note is largely devoted to denying that the behavior of investors and speculators can be counted on to enforce Proposition I. Some of the issues raised in his discussion are largely semantic, notably whether certain transactions can or cannot properly be called arbitrage. In our paper, we chose to include under the general heading of arbitrage, the operation of simultaneously selling a commodity and replacing it with a perfect substitute because it has, in common with dictionary arbitrage, two essential features: (1) simultaneous purchase and sale of (2) perfect substitutes. We regret that our stretching of the language has annoyed Durand, who would prefer to confine this word to cases where the commodities bought and resold were identical, rather than merely perfect substitutes. Unfortunately, Durand's stretching of the word "switch" annoys us just as much because it obscures the distinction between our transaction in perfect substitutes—let us call it a "roll-over" for the sake of harmony—and ordinary switches by investors between different securities in response either to changes in tastes or to shifts of the budget line. As we tried to warn readers in our paper [3, especially n. 13] the failure to appreciate this distinction has been responsible for much of the confusion which has surrounded the analysis of capital markets.

Reservations might also be entered about his use of the word "hedge" to describe the operation, closely akin to arbitrage à la Durand-Weinstein, whereby any investor—and not the holders of the overpriced shares only—can exploit a price discrepancy by simultaneously going short on the overpriced shares and long on the undervalued ones. There is little point, however, in pursuing these terminological issues. For, whatever the nomenclature, Durand does not seem to disagree that in perfect markets[1] enough mechanisms are available to investors and speculators to prevent value discrepancies in a class from being more than ephemeral. And, on our part, we entirely agree with him that real world markets are never perfect and hence that conclusions based on the assumption of perfect markets *need not* have empirical validity.

In particular, we have always been fully aware of the fact that, because of various "imperfections" and institutional limitations, "home-made" leverage obtained, for example, from margin buying was not identical with corporate

[1] The term perfect market is to be taken in its usual sense of implying perfect information and absence of transaction costs. In addition to these standard attributes, we also require for a perfect capital market that the rate of interest (or, more generally, the rate of interest function) be the same for all borrowers and lenders. See [3, p. 268].

leverage.[2] Such home-made leverage may well be inferior in some respects, though there are other respects in which it would seem to have positive advantages over corporate leverage.[3] But, in contrast to Durand, we saw little value in enumerating imperfections or trying to weight the relative merits of the two kinds of leverage. No amount of a priori speculation, we felt, could ever settle the issue of how close the substitutability is between home-made and corporate leverage, to say nothing of how close it would have to be to prevent any significant discrepancies in values from emerging. As is true elsewhere in positive economics, the most effective method of testing alternative assumptions is to test their consequences. If home-made leverage were as poor a substitute for corporate leverage as Durand and traditional doctrines (by implication) suggest, then levered companies would command a substantial premium in the market at least over some not insignificant range of capital structures. "Noise" in the data, of course, may well obscure this premium in particular samples; but if the cost advantages of (permanent) corporate borrowing were as large as traditional discussions suggest, they could and would be detected.[4] All we can say is that so far they haven't been detected; and consequently, implausible as it may seem to finance specialists, the assumption that home-made leverage of one kind or another is serving as a substitute for corporate leverage cannot yet be rejected in discussions of the cost-of-capital problem.

## II. *The Role of Financial Operations by Corporations*

To the above conclusion Durand raises the objection that Proposition I may hold not through the behavior of investors, but as a result of financial

[2] That the concept of home-made leverage as a substitute for corporate leverage is at least not unknown to a large number of investors would seem to be indicated by the reference to it in a popular investment manual [1]. We might remind potential arbitrageurs also that margin buying is not the only way to participate in the equilibrating process. See [3, p. 262, n. 19].

[3] While an individual who pledges securities as collateral runs the risk of losing more than his original equity in the securities, the fact that his general credit stands behind the loan is one reason why interest costs on such loans are as low as they are. By the same token, the existence of limited liability forces lenders to corporations to expect somewhat higher rates and to impose more numerous and severe restrictions on corporate financial (and even operating policies) than would otherwise be the case. Not only are such restrictions unlikely to enhance the earnings prospects of levered corporations, but precisely because they cannot protect the bond-holders against temporary adversity by drafts on the wealth of the stockholders, levered corporations expose their stockholders to the risk of "gambler's ruin." See [3, p. 274, n. 18]. Similar kinds of offsetting advantages and disadvantages arise, for example, from the circumstance that the borrowing in one case may be short-term and in the other, long-term.

[4] It is important to stress that the essentially zero correlation we found between leverage and cost of capital cannot be attributed solely to noise in the data. If noise alone had been responsible, we would not have been likely to get the results we did for our tests of Proposition II (see pp. 285-286) or for the supplementary tests described in note 55. In connection with the interpretation of the empirical findings, it also strikes us to be a matter of little consequence whether the premium on levered capital structures has not been detected because it is not there or only, as Durand suggests, because it is too small to be noticed.

operations by corporations. That is, if some financial structures were to command a premium over others, this, he argues, would afford "corporate managers an opportunity to reduce their cost of capital by adjusting capital structures"; and in exploiting such opportunities, they would tend to cause the discrepancies in valuation to disappear. Hence the fact that Proposition I holds need not mean that the cost of capital is independent of financial arrangements; on the contrary, it may tend to hold because the very opposite is true.

The first point to note in connection with this line of reasoning is that it offers no support whatever for the traditional U-shaped cost-of-capital curve, the main target of our paper. For these temporary premiums, arising because the process of adjustment to equilibrium takes time in less than perfect markets, are of an entirely different sort from the permanent premiums on mixed over pure equity structures which are presumed to arise, in the traditional view, from the fact that debt money is always "cheaper" than equity money. In fact, these temporary premiums imply no lasting shape for the market cost-of-capital function, let alone the orthodox U with its unique "optimum." Depending upon where along our equilibrium horizontal line the adjustments happen to be lagging at the moment, we may have a U, an inverted U or any other shape, regular or irregular, namable or unnamable.

The next point that must be stressed is that Durand has failed to indicate explicitly what can be gained by whom and how, through adjustment of capital structures to exploit temporary bumps; or how this adjustment affects our conclusions concerning the cost of capital. As a matter of fact, he has even failed to explain what *he* means by the cost of capital—although, if taken literally, he would seem to be concerned with the cost of capital to management, whatever that might be. Nor has he provided any concrete illustrations to help us understand just what he has in mind. The only illustration given refers not to the exploitation of "bumps" prevailing at a given point of time, but to the exploitation of changes *over time* in bond and stock yields—a quite separate issue which we shall consider later. This failure to be specific is not altogether surprising. Systematic analysis would have disclosed that temporary deviations from the relation between market value and leverage implied by Proposition I, do not significantly affect our conclusions concerning the cost of capital (as defined by us), or otherwise provide management with significant opportunities to benefit stockholders through adjustment of capital structures.[5]

By way of illustration, suppose that in a given class, total earnings were generally capitalized at 15 per cent and the market rate of interest were 5 per cent. For some reason, however, companies with leverage ratios of around 20 per cent suddenly became so popular with investors that their stocks sold

[5] While we doubt that even extremely "alert" managements, for reasons to be discussed, can reap important gains for their stockholders by exploiting the bumps, we have no doubts whatever that nonalert managements can produce losses for their stockholders. On this point, we actually took some pains in our Section II to warn managers that our statement that capital structure was a matter of indifference did not mean that they should become so complacent as, say, to float stock when the price of their shares was temporarily depressed, or to borrow at interest rates above the minimum obtainable in the market.

at a premium. Instead of yielding the 17 per cent indicated by our equation (8), these stocks are bid up to the point where they yield only 15 per cent. This, in turn, implies that the ratio of expected return to total market value, $\overline{X}/V$, for such companies is only 13.3 per cent as compared with the 15 per cent prevailing for all other members of the class. Durand would presumably argue that companies with leverage ratios above 20 per cent could raise the total market value of their securities by selling stock and retiring bonds; and vice versa for companies with leverage below 20 per cent. Such operations would increase the supply of stock in the popular leverage range. This, in turn, would tend to satisfy the "specialized demand" and contribute to the disappearance of the premium.

But how and to what extent would this benefit the stockholders of the adjusting corporations? The effect of these manipulations would be to raise the price of their shares for as long as the premium happened to last. Such a temporary price rise, however, could offer a real advantage only to those stockholders who, like our arbitrageurs, were wise enough to dispose of the stock before the premium disappeared. The faithful stockholders would reap no gain at all (and might even be inconvenienced). It is not obvious, therefore, that a management which passed up this kind of opportunity would be derelict or complacent, especially when we take into account the cost and time lags of such capital structure adjustments and their possible conflict with the other policy considerations we mentioned [3, pp. 292-93, Sec. IIB]. And since there are no clear or strong incentives for undertaking these adjustments, little, if anything, would seem to be lost in not considering adjustments on the supply side a significant part of the equilibrating mechanism underlying Proposition I.

We may next inquire: what in our example would be the cost of capital as we have defined it, that is the minimum rate of return required for an investment to be advantageous to the stockholders? The answer to this question may appear, at first sight, to depend on the initial capital structure and on the form of financing to be adopted. The cost of capital would seem to be 13.3 per cent for companies which already have the optimum structure and propose to finance new investment with the currently popular 20 per cent debt —80 per cent equity mixture. For companies with no debt currently, a cut-off rate as low as 5 per cent might seem justified (in the sense that it would raise the market value of the shares) if the firm could, by the debt-financing of such projects, get into the popular leverage range.[6] These appearances, however, are deceptive. For, in so far as debt-free companies are concerned, unless the yield of the investment is sufficiently greater than 15 per cent, it would be possible to increase the market value per share even more by using the debt issue to buy the company's own stock. More to the point, if the bonds are used to finance new investments, then unless the yield of the projects is at

[6] If the investment yielded 5 per cent + $\varepsilon$ ($\varepsilon$ being any positive number), and were financed by bonds at 5 per cent interest, then the expected return to the stockholders would increase precisely by $\varepsilon$. Since the new structure would be in the premium range where the capitalization rate for common stock is 15 per cent, i.e., the same as the rate prevailing for the original unlevered stock, the undertaking of the investment would increase the market value of the stock by ($\varepsilon$/.15).

least 15 per cent, the value of the stock will actually fall, once the temporary premium disappears, to less than it would have been if the investments had not been undertaken at all. Under these conditions, would Durand really be prepared to conclude that the appropriate cut-off rate for debt-free firms was anything but the same 15 per cent which would prevail everywhere in the class after the random bumps had disappeared?

For firms already in the premium zone, and thus able to finance new investment partly with "overpriced" stock the situation is more complicated. Here it can be shown that any investment yielding less than 15 per cent, but at least 13.3 per cent, might actually be slightly advantageous to the old stockholders (provided they were shrewd enough not to acquire the new issue) even after the temporary premium had disappeared. Their gain, however, is made *entirely* at the expense of new stockholders to whom the management sold the temporarily overpriced shares.[7] Whether or not it is incumbent on management to engage in this sort of "exploitation" is, fortunately, an issue we need not face here since so far no one has been able to point to value discrepancies for an alert management to exploit.[8]

There remains finally, to consider the issues raised by Durand's only concrete illustration. These issues are of an entirely different nature from the ones just discussed. They have nothing to do with the market imperfections which are supposedly Durand's main concern nor with the U-shaped cost-of-capital curve which was ours. They involve rather, the dynamic aspects of the investment and financing problem which, as repeatedly stated, we did not pretend to cover in our original paper.[9]

In the limited space available here, the most we can do is to point out that the basic conclusion of our static analysis with regard to the cost of capital will remain essentially valid even when we take into account changes

[7] If the new stock issue is acquired by the old owners in proportion to their ownership, the operation will turn out to be damaging to them, the moment the bump disappears unless the investment yields at least 15 per cent. More generally, if the sale of overpriced stock is regarded as undesirable from an ethical and goodwill point of view, recourse must be had to bond or other temporary financing, just as in the reverse case of a temporary undervaluation of the stock discussed in our paper [3 p. 292]; but this course can be justified only if the investment yields no less than 15 per cent. Thus even for companies already in the premium range, there are good reasons for regarding 15 per cent as the appropriate cut-off rate or cost of capital.

[8] Durand's remarks sometimes suggest that he is concerned with another type of market imperfection, the premium attaching not to particular financial structures, but to the securities of individual companies. In this case, the securities of at least some firms would have no "perfect" substitutes. Such firms would be confronted with a partly isolated and protected market entirely analogous to those underlying the theory of monopoly and of monopolistic competition; in fact, the notion of a class would then play the same role, and be subject to the same limitations as the notion of a group in the original Chamberlinian construction. Even for this type of imperfection it is not clear that the interests of the stockholders would be served by exploiting an existing premium through further issues as Durand seems to suggest (p. 643), since such a policy would reduce the market price of the shares.

[9] See e.g., [3, p. 273, n. 16]. We did, however, touch briefly on a certain class of essentially dynamic problems [3, p. 292] with a view not to solving them, but rather to showing how the analysis could be extended to handle them.

over time in the capitalization rates—in so far as these changes are anticipated by the market and hence reflected in the current capitalization rates. That is to say, in general, an investment will be worth undertaking from the point of view of the current owners, if and only if its yield, $\rho^*$, is no smaller than the *current* capitalization rate for the class $\rho_k$.[10] Furthermore, this is so regardless of financial arrangements, in the sense that the criterion will lead to the highest (equilibrium) current market value for the shares no matter how the investment is financed.

Of course, future changes in interest rates and the over-all cost of capital are not always correctly anticipated by the market. Hence even though all capital structures are equally good in terms of *current* market valuations, when unanticipated changes do occur some capital structures will turn out *ex post* to have been preferable. Preferable not in the sense of enabling the *company* to secure its capital at a lower cost—for this is an essentially meaningless concept—but rather in the specific sense of being more advantageous to stockholders who bought in before the change occurred. To illustrate, a 10 per cent fall over time in the market capitalization rate $\rho_k$ in a given class —interest rates and income expectations constant—will increase the total market value, $V$, by 10 per cent; and since the market value of debt is constant, it will increase the market value of the stock, $S$, of levered companies by even more than 10 per cent to an extent increasing with leverage.[11]

But it is one thing to say that if management can successfully guess an unexpected fall (rise) in $\rho_k$ it can reap gains for the stockholders by adopting a levered (unlevered) capital structure, and quite another to suggest that, because of this possibility, management should be encouraged, as it were, to speculate on these changes with the stockholders' money. For such speculation can also reap losses when the guess is wrong. We cannot help feeling that Durand's stress on the desirability of speculative adjustments is at least partly the wisdom of hindsight. It is easy in 1959 to call attention to the gains that management might have reaped for the stockholders by having a large proportion of debt in 1950 when interest rates were only 2.6 per cent and average stock yields 8.1 per cent, knowing that stock yields fell thereafter to 5.4 per cent and bond yields rose to 4.3 per cent. Apparently, however, it was not so easy in 1950 to see that bond yields were about to rise and stock yields about to fall (or else these changes would have taken place then and there).[12]

[10] The main qualification needed as a result of dynamic considerations is that when the capitalization rate is expected to fall, the static criterion is necessary but not sufficient. There are certain circumstances in which it may be preferable to postpone an investment, if such a postponement is feasible, thereby rejecting the investment now even though it currently meets the static test.

[11] It can be similarly verified that a rise in interest rates—$\rho_k$ constant—will increase $S$ for companies with long-term debt issued before the rise.

[12] Nor does it follow, as Durand suggests, that managements choosing to issue stock rather than bonds in 1959 have advantaged their stockholders, for that will depend on the *future* course of bond yields and capitalization rates. Actually, to be certain that levered shares fared better than the others between even 1950 and 1959, we should look at the behavior of $\rho_k$, not of the average current yields of shares. From just the information provided by Durand, one cannot rule out the possibility that $\rho_k$ in fact rose in this period,

There is abundant evidence showing how hard it is for anyone to outguess the market consistently and it is obviously impossible for a majority to do so. Consequently, we, at least, are not yet convinced that corporation managers, in choosing their capital structures and planning their investment programs, should give major consideration to the possible windfall gains (or losses) they may earn for the stockholders if the current market consensus about the level of future yields should turn out to be wrong.[13]

## III. *Dividends, Growth Opportunities and the Theory of Share Prices*

Durand is quite correct in pointing out that we nowhere provided an explicit description of how our model would explain relative share prices in the face of differential opportunities for growth by firms. Our omission, plus his own efforts at filling our gap, have apparently convinced him that our model cannot accomplish this task.

The fact is that the analysis can be extended in fairly straightforward fashion to accommodate growth without requiring any essential modification in the conclusions already established with the simpler model. To have developed this generalization in adequate detail, however, was impossible within the space limitations imposed on the original paper. Nor did it seem wise, in view of the controversial nature of the leverage issue, to open up a dividend policy dispute at the same time. Although, again for space reasons, many facets of the problem must be deferred to a forthcoming paper, we shall try to sketch out here at least enough of the theory to make it plain that no inconsistencies or peculiar assumptions about book-values are involved in the generalization.

Consider first the concept of a risk-equivalent "class," the analogue in our analysis to the "industry" of ordinary price theory. In the more general model, two companies will be said to be in the same class at a specific point in time, $t$, if the elements of the streams generated by the physical assests each holds *at time t* are perfectly correlated and proportional. Membership in the class at each point in time is thus to be determined only by reference to the assets held at that point. Hence, there is no contradiction in the fact that firms in the same class are adding to their assets and income at very different rates over time.[14]

Having attached a meaning to the notion of risk-equivalence of differently

the lower average yield of shares reflecting merely the rise in interest rates. If so, there might have been no net advantage to the stockholders of levered companies since the rise in $\rho_k$ might have offset the favorable effect of rising bond yields!

[13] Note that corporate managers (at least in nonfinancial corporations) who failed to engage in this type of speculation because they felt that it was not properly their function or because it was a task at which they had no comparative advantage, would not be depriving their stockholders of a potential source of gain by their failure to speculate. For, unlike the case of the "bumps," where the gains, if any, could only be reaped by financial adjustments at the corporate level, the shareholder always has the option of speculating on future market movements by arranging his own leverage position.

[14] Note also that our definition of a class [3, p. 266] reduces to the broader definition above, in the special case in which (a) all firms in the class can acquire future income at the same terms (equivalent growth potentials) and (b) all the managements act in the best interests of the stockholders and hence pursue equivalent investment policies.

growing streams we can consider the relative valuation of shares of "equal risk" but different growth potentials.[15] In order to handle, one at a time, the issues raised by Durand, we shall take for granted, until the end of this section, that investors behave rationally and that capital markets are perfect. These assumptions imply, among other things, that investors are concerned only with the total income they receive from a security and are indifferent as to whether this income takes the form of dividends or of capital gains. When such is the case, the market value of any firm will depend only on the earning power of the assets currently held and on the size and relative profitability of the investment opportunities that the firm is expected to undertake in the future.[16] More specifically, let:

$\overline{X}_j(t)$ = the expected return (in the sense of our note [3, p. 265, n. 6]) of the assets held by firm $j$ at time $t$;

$k_j(t)$ = the expected volume of purchases of new assets by firm $j$ in period $t$, expressed for convenience as a percentage of the expected return on the assets held at $t$;

$\rho_j^*(t)$ = the expected rate of return on the assets acquired at $t$;

$\rho_k$ = the capitalization rate in class $k$ for the uncertain, but "non-growing" stream $\overline{X}_j(0)$ (i.e., one for which $k_j(t) = 0$ for all $t$). Alternatively, $\rho_k$ may be thought of as the yield investors would earn on the securities of a company in the class with no differential earning opportunities (i.e., one for which $\rho_j^*(t) = \rho_k$ for all $t$ regardless of $k_j(t)$).

It can then be readily shown (although the formal proof must be postponed to a forthcoming paper) that the market value of the firm will be given by:

$$(1) \quad V_j(0) \equiv S_j(0) + D_j(0) = \overline{X}_j(0)\left[\sum_{t=0}^{t=\infty}\left(1 + k_j(t)\frac{\overline{X}_j(t)}{\overline{X}_j(0)}\cdot\frac{\rho^*(t) - \rho_k}{\rho_k}\right)(1+\rho_k)^{-t}\right].$$

[15] In the matter of our treatment of the risk (or more properly the uncertainty) surrounding equity streams, we should like to take strong exception to Durand's observations at several points but especially in his concluding comments on page 653 that our model assumes "a remarkably safe world" and that that is why the "effect of risk on the cost of capital . . . is not apparent." We cannot see how anyone reading our discussion [pp. 265-66] could infer that we are only "allowing corporate earnings to fluctuate somewhat—presumably about a fairly definite central value" or that we have somehow ruled out "major disaster of any sort." We did, of course, assume in our first model that *bonds* were completely riskless and perhaps this is the basis of his objections. We felt, and still feel, however, that this is an entirely satisfactory first approximation because, if for no other reason, the quantitative restrictions typically imposed by lenders to reduce their risk have in practice been remarkably successful.

Durand's impatience with our notion of a risk-equivalent class even at the level of theory (p. 653) is also puzzling. We hope that those who have tried to face up to the logical difficulties involved in applying the ordinary present-value apparatus (which is certainty analysis) to uncertain streams will recognize some merit in our risk-equivalent class as a method of dealing with some of these well-known difficulties.

[16] See, in this connection, footnote 21 below.

It follows further from the first three definitions given above that

$$\frac{\overline{X}_j(t)}{\overline{X}_j(0)}$$

can itself be expressed entirely in terms of the quantities $k_j(\tau)$ and $\rho^*(\tau)$, $\tau=0, 1, \cdots, t-1$ by means of the recursive relation:

$$\text{(2)} \qquad \overline{X}_j(t) = \overline{X}_j(t-1)[1 + k_j(t-1)\rho_j^*(t-1)] \qquad t = 1,2, \cdots, \infty.$$

Hence, the value of the infinite summation in the right-hand side of (1) is, in the final analysis, a function of all the $k_j(t)$, $\rho_j^*(t)$ and $\rho_k$ and can be conveniently denoted by $1/\psi_j(k_j(t); \rho_j^*(t); \rho_k)$. We can then rewrite (1) as:

$$\text{(1')} \qquad V_j(0) = \overline{X}_j(0)/\psi_j(k_j(t); \rho_j^*(t); \rho_k)$$

where the function $\psi_j$ is defined by (1) and (2).

Equations (1) and (1′) should make clear what is involved in extending our original analysis to encompass growth. Instead of the single capitalization rate for current expected earnings in the class,

$$\frac{\overline{X}_j(0)}{V_j(0)} = \rho_k \text{ for all } i$$

—this rate being also the same as the cost of capital in the class—we now get a multiplicity of composite capitalization rates

$$\frac{\overline{X}_j(0)}{V_j(0)} = \psi_j.$$ [17]

But, and this is the important point, all these composite capitalization rates for current earnings are reducible to the single cost of capital in the class $\rho_k$, and the parameters $\rho_j^*(t)$ and $k_j(t)$ which characterize the opportunities expected to be available and to be exploited by the firm for investing funds at a rate of return higher than the cost of capital, $\rho_k$.[18]

[17] The function $\psi_j$ can, of course, be specialized by making some definite assumptions about the nature of the $\rho^*_j(t)$ and the $k_j(t)$. If, for example, one assumes $\rho^*_j(t) = \rho^* =$ a constant, $k_j(t) = k =$ a constant for all $t$, and if one neglects leverage then (1) reduces to:

$$\text{(2)} \qquad S_j(0) = \overline{X}_j(0)\frac{1-k}{\rho_k - k\rho^*} \qquad (\rho_k > k\rho^* \text{ or } \rho^*, \text{ whichever is smaller}),$$

the valuation formula given by Durand. In using this simple specialization one need not be unduly concerned about Durand's "growth stock paradox" (see his reference [5]). The case in which $k\rho^* \geqq \rho_k$ is not a *substantive* paradox at all, but an artifact attributable solely to the partial equilibrium nature of his analysis. In a general equilibrium framework (which we shall present in still another forthcoming paper), $\rho_k$, the capitalization rate, is not an independently given constant as in (1) [or the special case (2)], but a *variable*. Its actual value will be whatever is necessary to clear the market, given among other things, the growth opportunities available. If it were really true that for some corporation $k\rho^*$ was expected to remain indefinitely at say, 50 per cent per annum ($k$ being less than one), the $\rho_k$ for all stocks of "equal risk" would have to be at least 50 per cent. From what one knows about stock yields and growth potentials, this hardly seems an event worth worrying about; but it is in no way paradoxical.

[18] The use of the words "rate of return *higher than* $\rho_k$" in the above sentence is correct

Equations (1) and (1′) may also be helpful in making clearer the precise meaning of our assertion [3, p. 266] that, in perfect markets, dividend policy is a "mere detail." This statement has been misunderstood by some readers because they make the tacit, but unwarranted, assumption that the ability of the firm to exploit profitable opportunities is limited by—or at any rate intimately connected with—its dividend policy. This assumption is unwarranted because dividend retention is but one of the many sources through which expansion can be financed. Furthermore, in the absence of market imperfections such as flotation costs, and institutional factors such as tax laws, this source has no advantage or disadvantage over other sources from the point of view of the cost of funds. Hence, the possibility of exploiting opportunities is independent of dividend policy. Once a decision has been made as to which opportunities are to be exploited, the only role of dividend policy is to determine what proportion of each year's investment $k_j(t)\overline{X}_j(t)$ is to be financed from retained earnings—the equivalent of a fully subscribed preemptive issue—and what proportion from outside sources.[19] Similarly, from the point of view of the stockholder, the only significance of dividend policy is to determine how much of the earnings of the firm will accrue to him in the form of cash, and how much in the form of capital gains (or losses). We need hardly add that this explication of the "irrelevance" of dividend policy is in no way dependent, as Durand repeatedly suggests, on any special assumption about the ratio of price to book value.[20]

as long as we can expect all firms to follow an optimal investment policy, for then no firm will ever exploit opportunities yielding less than $\rho_k$. If so, the market price will reflect only the availability of high-yield opportunities and the capitalization rate $X_j(0)/V_j(0)$ will never be larger than $\rho_k$. If, however, we want to take into account the empirically relevant possibility of firms being expected to exploit "unprofitable" opportunities, then the words "higher than $\rho_k$" in the above sentence should be replaced by "different from $\rho_k$." For firms expected to adopt unprofitable investments, the market capitalization may, of course, be higher than $\rho_k$. On this point, see also footnote 19 below.

[19] Admittedly, this is not what most people have in mind when they bristle at our assertion that dividend policy is a mere detail under perfect capital markets. Their thoughts usually leap immediately to cases of the Montgomery Ward type, in which supposedly everyone but the management felt that a cessation of the hoarding policy and a more generous dividend would have raised the price of the shares. To reason this way, however, is to forget the vital *ceteris paribus*, namely *given the investment policy*. In our terminology, what was at fault was not the dividend policy as such, but the decision to invest in cash (i.e., $\rho^*$ was too low). We doubt, in other words, that the stockholders would have been better served had management decided to finance *the same volume of hoards* not out of retained earnings, but out of new stock issues.

[20] The relation between the market value of a firm and its book value—the latter being defined not in the accounting sense, but in the economic sense of the reproduction cost of a firm's assets—will depend on many things. Some (such as drastic revaluations of a firm's prospects) do not lend themselves readily to further analysis; others (such as the relation of internal yields [the $\rho^*(t)$] to external yields [the $\rho_k$] can be more systematically explored by means of various specializations of equation (1). If, for example, one assumes that $\rho^*_j(t) = \rho_k$ for all $j$ and all $t$ then, with certain minor supplementary assumptions, one can obtain Durand's case of price equal to book-value throughout the class. This case, however (which stands in roughly the same relation to our general formulation as the "no-rent" case stands to the general theory of supply functions) is merely one of a large number of interesting possibilities.

## IV. *The Effect of Dividends on Stock Prices: The Empirical Findings and Their Interpretation*

We hope that the discussion in the previous section has been sufficiently explicit, even though the formal proofs have been omitted, so that it no longer requires any great act of faith to accept our original conclusion, *viz.*, that in a world of perfect markets and rational behavior, a firm's dividend policy, other things equal, will have no effect either on the value of the firm or its cost of capital. We can then take the next step and enquire whether this conclusion is a valid or useful approximation in real-world capital markets.[21]

On this issue, Durand cites a number of recent empirical studies and presents some results from his own researches which, he claims, leave "no doubt whatsoever that dividend policy exerts an influence on stock prices and the cost of capital." We have to enter a strong dissent; for, as we warned in our paper [3, p. 287 and 288, n. 43) having precisely the studies he mentions in mind, the existing empirical tests are hopelessly inadequate for determining the effect, if any, of payout policy on stock prices or on the cost of capital.

What is the nature of this supposedly irrefutable evidence? It consists, by and large, of cross-section studies in which price is correlated in various ways with dividends and with *current income*. In general, dividends turn out to have high gross and net correlations with price. From this, it is concluded that our valuation formula (1), which involves earnings but not dividends, cannot be an adequate representation of reality. Note, however, that the earnings variable in our equation is not *current* earnings, $X$, but $\overline{X}_o$, the *expected value* of the (uncertain) earnings of the assets currently held. This difference may seem a small one at first glance, but it actually holds the main clue to the puzzle.

For if there is one thing we can assert with confidence about the firms in any sample, it is that the earnings they report for any short period like a year are affected by a great many random disturbances and temporary distortions. (See, in this connection, Durand's comment p. 651.) To the extent that these temporary disturbances are recognized as such, they will, of course, be discounted by investors and will not be reflected in market prices. Current income, in other words, is at best only an approximate and often very imperfect measure of $\overline{X}_o$, the "noise-free" earnings potential upon which rational investors would base their valuations. Furthermore, and this is what causes most of the trouble, there are many other variables which, like $X$, are cor-

[21] The issue under discussion—whether and to what extent market valuation is affected by current dividend policy—which is a very real issue, should not be confused with a different and entirely empty issue. This is the question whether what is capitalized is the stream of earnings and earning opportunities ratable to a share or a stream of revenues accruing to the holder of the share and consisting of cash dividends plus capital gains. This is an empty issue for it can be readily shown that both of these views, when properly stated and understood, lead to identical implications; in particular, they both imply that, given the investment policy of the firm, market valuation in perfect markets is independent of current dividend payments or long-run payout policy.

related with (i.e., contain information about) the unobservable but crucial $\bar{X}_0$. In particular, whenever corporations follow a policy of stabilizing dividends—and the excellent studies of Lintner (see, e.g., [2]) leave no doubt that the majority of the publicly held corporations usually do—dividends will contain considerable information about $\bar{X}_0$, possibly even more than $X$. Hence, when one runs regression of price against dividends, either alone or in combination with $X$, significant positive coefficients would result even in a world in which we knew for certain that $\bar{X}_0$ alone was being capitalized and that dividend policy had no independent effect whatever on price.

The following example may clarify the nature of the difficulty. Suppose to take the simplest case, that we have two (unlevered) corporations with $\bar{X}_{01} = \bar{X}_{02} = \$5$ per share and with identical long-run payout policies of 40 per cent. Let us suppose that the market price of each is determined exclusively by "noise-free" earnings. If then, the capitalization rate is .1, both shares sell at $50. Imagine now that firm 1 suffers a run of bad luck during the current year—or merely that its accountant decides to write off some assets—so that current income falls momentarily to $3. Since management recognizes the situation as temporary, it does not take the drastic step of cutting its dividend which remains $2. Firm 2, on the other hand, has had some temporary good fortune which pushes its income up to say $8. Again, since the extra income is in the nature of a windfall, management does not raise the dividend above $2, since that would not be maintainable given its 40 per cent payout target. Suppose now, given this situation and the data we have generated, we conduct one of the popular tests and see whether dividend policy affects stock prices. If we compare, say, price-earnings ratios with current dividend-payout ratios, we find:

$$\frac{P_1}{X_1} = \frac{50}{\bar{X}_{01} - 2} = \frac{50}{3} = 16.667 \qquad \frac{D_1}{X_1} = \frac{2}{3} = .667$$

$$\frac{P_2}{X_2} = \frac{50}{\bar{X}_{02} + 3} = \frac{50}{8} = 6.25 \qquad \frac{D_2}{X_2} = \frac{2}{8} = .25$$

One would certainly be tempted, from such striking results, to draw the conclusion, which we know to be false in this case, that the payout ratio (dividend policy) does have a marked effect on stock prices.[22] We get these striking results, of course, only because dividends here contain information about $\bar{X}_0$.

It is, of course, one thing to say that the informational content of dividends *could* fully account for the empirical correlations of Durand and others, and quite another to say that it *does*. At the moment, the evidence is insufficient

[22] For the benefit of sophisticates who know all about the dangers of spurious ratio correlation, we hasten to point out that it is not the use of ratios per se that makes dividends appear to influence price. As we shall show in more detail in our forthcoming paper, in any sample containing substantial numbers of dividend stabilizers, dividends will, in general, appear significant in relation to $X$ regardless of the form of the test.

to settle this question, as Durand himself eventually concedes (p. 651). We have some new, and we hope much sharper, tests under development, and we hope others will join us in our attempt to disentangle the two effects. But the task is by no means a simple one and definitive results may be a long time in coming.

But even if more conclusive tests were to provide adequate evidence that the apparent effect of dividends is entirely attributable to the information they convey about long-run earnings prospects, would we not, in any event, be forced to abandon our position that dividend policy has no effect on the cost of capital? That such is not the case can perhaps best be seen by considering another variable frequently used in statistical tests, to wit, the book value per share. Like dividends, this variable shows up well in correlations with $P$ (often even when both $D$ and $X$ are included in the relation). But, unlike $D$, there seems to be little question that the effect of $B$ is wholly informational. Or, to put it in a slightly different way, few specialists these days would argue for book value, as they do for dividends, that because of the demonstrated high correlations with price, a firm could count on raising the price of its shares permanently, simply by writing up its book value, if nothing else has changed in the situation. The word *permanently* is the important one in this context. For, in view of the customary informational content of book value, we cannot exclude the possibility that its manipulation will temporarily succeed in misleading the market.

What has been said for book value can be repeated for dividends. Because changes in dividend are usually an indication that other things have changed, they may temporarily affect the market even when other things have, in fact, not changed. Indeed, we are quite ready to believe that as many investors were gulled by Daniel Sickles' watered dividends as by Daniel Drew's watered stocks! All we argue is that changes in dividends will have such an effect only in so far as they are not perceived as manipulations and that the effect will be temporary unless the message is confirmed by deeds.

In summary, pending adequate evidence, we are not willing to accept the proposition that dividend manipulations can be exploited to lower permanently the cost of capital. For this proposition would imply either that investors are incurably irrational or that corporate managers really can fool all of the people all of the time.

## V. *Concluding Remarks*

We are grateful to Durand for giving us this opportunity to clarify certain points in our paper and to show that our approach is actually a good deal more general than he and others seem to have realized. We have been the first to stress that our paper was intended to be no more than the beginning of the attack on the cost of capital and related problems; and we have indicated areas both fundamental and applied in which the implications of our model remain to be totally or partially explored. We are as aware as Durand of the obstacles in the path. But we hope to have shown that Durand's nihilism is premature; that the framework developed in our paper has al-

ready permitted some progress; and that it represents at least a promising point of departure for a further systematic attack on the many remaining problems.

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## Pricing Objectives in Large Companies: Comment

In R. F. Lanzillotti's "Pricing Objectives in Large Companies," [1, p. 922] A and P is considered "at the other extreme" from price leaders, and to "face so many competitors . . . that in spite of their absolute size they are very far from being able to make decisions for the market, and do not think of competition in terms of actions of one or a few competitors." But they are 'not simply meeting competition—they are preventing it" [1, p. 935]. Ability to "prevent" competition means effective control of a market. One statement or the other is wrong. The next sentence: "This [prevention of competition] appears to have been the purpose of A and P in localizing price cuts to make matters difficult for a competitive store on its opening day, or General Foods in reducing the price of [pectin] in the Northwest . . ."

General Foods was a near-monopolist of pectin (originally by patent) who could reasonably hope to confine a small competitor to one region, and strove hard and successfully to do so [2]. In terms of rational profit-seeking behavior, what made sense for General Foods made no sense for A and P. This suggests a closer look at Lanzillotti's evidence. The only explicit fact is as follows [1, p. 935, n.]:

> An A and P official of the Atlantic Division, for example, said, "It might be necessary for us to operate unprofitably for several weeks . . . reducing our line of [sic] 10 per cent several weeks prior to the time the competitor plans to open so that people in the community will be impressed with our low prices. . . .

Lanzillotti has repeated and enlarged an error of the District Court, who joined together, and presented in a single passage, two extracts from two separate documents; in addition to making the passage a single sentence, Lanzillotti has omitted significant language from the second extract. The first

sentence, from Govt. Ex. 3209: "It might be necessary for us to operate unprofitably for several weeks *until sufficient volume is gained to reduce our expense rate*" (Court's omission italicized). The "us" refers to the whole company and its constituent parts. There is no reference to "preventing competition" or even embarrassing a competitor, or to any particular store; the letter sets forth a policy of continuous profitable low prices. The second part of the quotation is from a letter (Govt. Ex. 3263) written two months later to a different man on a different subject, the quoted sentence concluding as follows: ". . . impressed with our low prices *and will continue to shop with us after the competitor has opened shop*" (Lanzillotti's omission from the Court's opinion italicized). The competitor, far from being "prevented," would be in business. There is no suggestion of selling at unprofitable prices. Lanzillotti also cites two pages in the Government Brief (909 and 931), neither of which say anything about selective price reductions against new stores.

The point is important as a reminder that in the pursuit of knowledge we cannot do without the constant cross-check of theory and apparent "fact" lest we commit glaring errors of observation. In theory, because of easy entry and numerous competitors, reasonable profit-seeking men would not consider an impossible policy of "preventing competition," except in the popular sense that continuous low prices are an "unfair" preventive. The facts accord, once we bother to check them, including second-best examples belatedly offered.

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## Pricing Objectives in Large Companies: Comment

It is rash to try to distill from a compendium of case studies broader and simpler generalizations than the compilers themselves seem to think the evidence justifies. However, there is also the opposite danger, that the sheer volume, complexity and apparent noncomparability of the information collected may produce a misleading impression of diversity where in fact there is underlying uniformity. The latter possibility is particularly acute when the raw materials consist essentially of discursive statements of opinion, elicited in personal interviews with interested parties, as in the Kaplan, Dirlam and Lanzillotti *Pricing in Big Business, A Case Approach* [4]. In summarizing these findings [5] Lanzillotti has not completely escaped that pitfall; the character of the information presented has, I think, misled him into deprecating unduly the validity and usefulness of the economist's traditional assumptions about the objectives of business policy and the influence of market structure on the methods by which the firm pursues them. In contrast with his interpretation, the evidence presented seems to me to lend strong support to

the assumption that even giant corporations do try in their pricing to maximize profits, in a meaningful sense, and that their prices therefore reflect largely what the objective circumstances of the market will, or in a world of uncertainty will seem to, permit.

Extensive reference to the voluminous literature on this subject is inappropriate here, except to point out that it amply demonstrates how complex is the seemingly innocent question: Do large businesses price to maximize profits? It is possible of course to answer the question affirmatively by stretching the concepts of "maximization" and "profit" so wide in time, space and content as to say no more than that the community of managers and owners does act, and in so doing presumably tries to serve what it somehow conceives to be its best collective interests. The information in the Brookings monograph, I suggest, lends support to a much narrower and more workable conclusion: that these large corporations typically price to maximize monetary profits—not day-by-day, to be sure, but to a large extent year-by-year and certainly over a fairly brief period of years. The evidence does not, I suggest, justify Lanzillotti's generalization that "these corporations tend to behave more and more like public utilities" [5, p. 940], if one refers not to pricing formulas, techniques or retrospective rationalizations, but to "pricing objectives," which are the subject of his article.[1]

### I. *Pricing to Yield a Target Return on Investment*

Among the "pricing goals" cited by company executives, the authors encountered "perhaps most frequently"—in about half the observations, and increasingly over time—mention of pricing to achieve a target return on investment [5, pp. 923, 929]. The prevalence of this phenomenon, which is really an aspect of full-cost pricing,[2] provides their principal evidence against profit-maximization in general[3] and the marginalist description thereof in particular. The misconstruction is in confusing procedures with "goals." Actually, the target return seems above all to reflect what the executives think the company can get; and to the extent actual earnings diverge from the target, it is because the market turns out to allow more or less.[4]

1. This interpretation is supported first by the differences between the targets set by the various companies interviewed. The first and most frequently

[1] The difference between our interpretations is one of degree and emphasis only; but the difference is important, to economics and to public policy.

[2] See the excellent analysis and survey of the literature by Heflebower [3, particularly p. 364]. In the sale of consumer durables, like appliances and automobiles, where competition year-by-year takes the form largely of changes in product design, the procedure is to some extent better characterized as "price-minus costing" than "cost-plus pricing." An acceptable consumer price—say $299.50 for a standard washing machine—is first determined, and the manufacturer then decides what "features" it can afford to build into the product. But the build-up of costs in this fashion includes the target or normal rate of return. See especially the discussion of General Electric [4, pp. 58, 61]; on automobiles, see [4, pp. 49-53].

[3] It is "especially the target-return-minded companies" that, according to Lanzillotti, "tend to behave . . . like public utilities" [5, p. 940].

[4] Cf. Lanzillotti [5, p. 929]: "The evidence on actual profit rates, taken in conjunction with the targets mentioned, raises serious questions whether these companies are attempting

cited rationalization of the particular target rate selected, "fair or reasonable return" [5, p. 931], openly invites the analogy to public utility pricing. Yet the authors offer us no reason to believe that the decision-makers of International Harvester (who aim at 10 per cent on investment after taxes) are more public-spirited than those of General Motors (who aim at 20 per cent); of Alcoa (about 10 per cent), than du Pont (no specific figure given, but the company averaged 25.9 per cent in the period 1947-55); of U.S. Steel (8 per cent), than General Electric (20 per cent); the compilation of these targets and actual profits data by Lanzillotti [5, pp. 924-27] is extremely illuminating. It would seem that what is "fair and reasonable" varies from industry to industry. The most plausible explanation of the differences is to be found in the divergent objective facts of the market place which determine what the traffic will bear—in particular the ease of entry, number of sellers, independence of competitive action, standardization of products, degree of patent protection, and so on.[5]

2. The maximization hypothesis seems even more clearly supported by the widely varying investment-return components of these companies' prices on different products. Wide variations in what the market will bear frequently produce corresponding divergent "target" as well as actual rates of return, some above, some below the company-wide target;[6] in some instances, the costs of joint products are themselves allocated on the basis of what the market will allow.[7] When, therefore, in discussing the tendency of these com-

to 'maximize' profits on a year-to-year basis. Moreover, to construe the actual profit rates (as against target rates) as evidence of a long-run maximization policy would require the demonstration that the prices charged were based not upon the targets but on what the firms believed they could get as a maximum. In any event, for this sample of firms and for this time period, there are limitations upon profit maximization as an adequate explanation of the relationships between profit targets and actual profit rates." I have no serious quarrel with the doubts he expresses about maximization "on a year-to-year basis." But, as will appear, I do submit that the divergences between target and actual rates are indications of a policy of profit-maximization; and deny that the target rates are set primarily, as in the case of public utilities (see note 3 above), on the basis of considerations of fairness or reasonableness [cf. 4, p. 931].

In his review of the Kaplan, Dirlam and Lanzillotti book [6] Jesse Markham makes much the same observations as follow here as points 2 and 3. The discussion here, originally prepared before the Markham reviewed appeared, has been compressed to avoid undue repetition of his analysis.

[5] For an exposition of full-cost pricing in which the allowance for net profit added to standard cost is explicitly related to what the market will allow, see Andrews [1, pp. 163-80]. And of course Bain [2, Ch. 7] has done pioneer work in statistically verifying the relationship between profitability on the one hand and concentration and barriers to entry on the other.

[6] See, for example, the cases of Alcoa [4, p. 31], International Harvester [4, pp. 72-74], Union Carbide [4, pp. 112-13; compare here particularly the pricing of ethylene glycol, the raw material, and Prestone, pp. 116-17, 265], and du Pont [4, p. 151]—all companies avowedly pricing on the basis of cost plus a target rate of return. As Ronald Coase commenting on Heflebower's paper observed [3, pp. 392-93], it is very difficult to avoid marginalist explanations of price discrimination; and multiproduct sales at different mark-ups are a similar phenomenon from the standpoint of the seller.

[7] See, for example, the case in meats [4, p. 47], and of du Pont [4, p. 152, note 25]. Swift, it is true, does not price on a target-return basis, but its inability to do so is

panies to price as stably as possible over the business cycle, Lanzillotti observes that "in pricing different items in the product line, there will be an effort (sustained in individual cases by the pricing executive's conscience) to refrain from exploiting any item beyond the limit set by cost-plus" [5, p. 932], an approach to profit-maximization at least over a period of years is typically already built into either the "cost" or the "plus," and most often the latter.[8]

3. Rationality of pricing policy, as the economist has traditionally defined it, is suggested also in the divergences of actual company returns from their respective targets—above it for extended periods of time, where the market permits; below it where the market requires. As for the latter instances, the target evidently represents a mixture of expectation and hope: the case studies abound with instances of companies accepting less where they had to, with pricing on a virtual incremental cost basis if necessary.[9] International Harvester's failure to achieve even the modest 10 per cent goal in the 1947-55 period (its actual average return was 8.9 per cent) suggests that the modesty of the target was an evidence not of restraint on profit maximization but realism; and that the company must have been trying to get all it could. More significant in the present context is the persistence of actual returns markedly above the target. General Motors has shown no tendency to alter its pricing

itself a reflection of the inhospitality of meat markets to this kind of pricing. So the fact that some companies can and others find they cannot base pricing on full-cost or that some products permit it and others do not is itself a reflection of what the market will allow in the way of a profit-maximizing policy. See, e.g., Kaplan's explanation of why that kind of pricing is impossible in fresh meats, possible in automobiles, possible for Alcoa, decreasingly so for Standard of Indiana, and impossible for Kennecott [4, pp. 47, 86-91, 131, 142, 176-77, 185]. See also the discussion in section II below.

[8] It is difficult to be certain how meaningful is Lanzillotti's observation that "individual products, markets, and pricing are not considered in isolation . . . . pricing is frequently done for product groups with an eye to the over-all profit position of the company" [5, p. 939]. If this means anything in the context of our present discussion, it must be that companies will typically charge on their high-margin lines only enough to raise aggregate company profits to the target level. This would be difficult to prove in the case of du Pont, since that company specifies no over-all target; targets evidently are set product by product only, and all together produced an average return of 25.9 per cent after taxes in 1947-55. If instead my points (1) and (2) are correct, Lanzillotti should have gone on as follows: "However, individual products and markets contribute to that over-all profit largely on the basis of their varying elasticities of demand, and the aggregate profit itself seems closely attuned to what in fact the company finds it can stably earn."

It appears from the context in which the foregoing statement appears, however, that Lanzillotti may mean only that costs and revenues for individual products and markets can seldom be meaningfully estimated in isolation, because of the prevalence of joint costs and benefits. This is of course quite true. For example, Johns-Manville and International Harvester are forced by their desire to offer a full line into accepting returns well below the target on certain products [5, p. 160]. Yet if the two companies' faith in the full line is justified, the returns on the "line-filling" items are only nominally low; and failure to drop them may well reflect the maximizing calculation that the incremental costs of carrying them are lower than their fully allocated costs, and the incremental revenue losses from dropping them greater than their nominal returns. The authors specifically point this out in the case of International Harvester [4, pp. 73, 76-77].

[9] For examples of the latter practice, see the cases of International Harvester, just cited, and of Esso Standard [4, p. 82].

formula in view of its having earned an average 26 per cent after tax rather than the "fair and reasonable" target 20 in the 1947-55 period.[10]

4. In many situations, target-return pricing simply does not make sense except as an *ex post* rationalization of profit-maximization. Explications of the method by company officials tend irresistibly to proffer "good" rather than "real" reasons—although considerations of fairness, or defensibility before Congressional investigating committees (or against wage demands) undoubtedly play some role in pricing decisions, in some companies more than others.

For example, the policy sometimes proclaimed by du Pont officials of determining the prices of new products with reference to the research and developmental expenses they have involved would actually be an irrational means of recouping those expenses. Here is the company's avowed explanation:

> If our contribution of an improved new product is an exceptional achievement because of long and expensive research and development and a high permanent investment hazard, and if it affords profitable opportunities to consumers or converters, we feel we are entitled to an exceptionally good return and we ask a corresponding price for it. If our contribution has been only a moderate one, then we determine upon a price that will give us a profit consistent with our work, effort, and risk [5, p. 153].

The statement is equivocal; yet clearly, among other things, it implies that the "value of the contribution" that helps determine price is determined in large measure by its cost, including the risks it has entailed. But it makes no sense to try to recoup research costs by charging a cost-based price that either exceeds or falls short of the profit-maximizing level. Sunk costs may be used to *justify* a price embodying a high mark-up over out-of-pocket expenses, for public relations purposes; but it cannot *explain* it, unless the responsible officials have thrown rationality to the winds. The only connection in which they might intelligently relate price to sunk costs *ex ante* would be if they *reduced* or voluntarily accepted less than a profit-maximizing price that threatened to recoup too much (by standards of reasonableness or whatever) —never if they increased or exceeded a profit-maximizing price that threatened to recoup too little. So, references to heavy research and developmental expenses are not convincing as explanations of high prices and profit margins; and that is what they are usually adduced to explain.

That du Pont does not in fact behave in this irrational manner is suggested by the monograph's story of nylon-pricing—an account the authors preface with the (irrelevant) statement that research in developing the fiber "cost the company $6 million,"[11] and with the implication that this fact had

[10] See the authors' mild expression of skepticism about the relevance of General Motors' pricing formula in the light of its actual profit experience [4, pp. 55, 134].

[11] The authors add that "21 million was spent on manufacturing facilities before commercial operations and sales began" [4, p. 103]. Since the financial dimensions of du Pont's risks in the development of nylon have become part of the folklore on the relationship between business size, patent protection and innovation (for example, the *Report* of the Attorney General's National Committee to Study the Antitrust Laws [10, p. 225, n. 8]

something to do with the way it was priced. The company's explanation of the postwar pricing history of nylon [4, pp. 105-7] runs in terms of increasing production costs and inelastic demand (except for instances when, in time of extreme shortage, the company refrained from charging anything like what the grey market was paying); research and development costs are not mentioned. The one deliberate price reduction mentioned was for 840 denier yarn, used in tire cord, a cut "aimed at further penetration of this market at the expense of rayon" [4, p. 107]. Again, the authors can explain du Pont's apparently more aggressive price reductions in the corresponding early history of cellophane only in terms of differences in their respective costs and price elasticities of demand; the former are not defined to include research costs [4, p. 254]. And of course the company's own statement of policy, quoted above, can similarly be interpreted in marginalist terms: the value of the "achievement" is, clearly, defined largely in terms of elasticity of demand. In the end, after once again implying that research expenses must be allocated among products and so enter their prices on some "fair share" basis (which may well be true as a matter of accounting procedure), the authors conclude "the company comes close to charging, over the long run, what the traffic will bear" [4, pp. 154, 155]: research cost-plus pricing is not mentioned.[12]

There are other instances in the monograph where alleged nonmaximizing considerations in fixing prices would have been irrational and seem moreover to be refuted by the evidence. For example, Alcoa officials asserted that the company's "pricing of aluminum ingot, the pivotal price for aluminum products, has been based on promotion of the use of aluminum," and that in so doing the company "has consistently refrained from charging all the traffic will bear" in this market [4, pp. 31-32]. Surely, first, a rational monopolist seeking to promote the use of aluminum could better strike the profit-maximizing balance in the light of varying elasticities of demand for different aluminum products not by taking a low price on all sales, but, instead, by holding up the price of ingot on sales to fabricators, and, integrating forward into the price-elastic lines, accepting low nominal fabricating margins in those lines. Second, Wallace concludes that profits at the monopolistic ingot level were much higher than at the competitive fabricating levels, and high also in comparison with American industry generally, for the very reason indicated [9, Ch. 11]. The Alcoa statement quoted above refers, it is true, mainly to

repeats Judge Ryan's figure of $45 million spent on research as an example of the "substantial investment at considerable risk" required for innovation today), Willard E. Mueller's devastating analysis of what the Company's own president referred to as a "$27,000,000 gamble" [7, p. 84] should be required reading for economists. Mueller shows that only about $787,000, spent on research, really involved "the big gamble"; another $391,000 went into the pilot plant. All the additional millions of dollars were for constructing a commercial plant, after the technical and commercial attractiveness of nylon had been clearly established.

[12] As this conclusion indicates, the authors are themselves skeptical that du Pont really prices new products to any significant extent on the basis of research and developmental costs plus a reasonable return. They nowhere state that it does.

the period after the second world war. In this event, it is a reasonable question whether the low price policy on ingot might not have reflected a decline in the company's monopoly power and the prohibition by antitrust decree of the kind of margin-squeezing promotional pricing described above [see 4, pp. 30, 142-43], just as Wallace had advocated long before [9, p. 395].

It is precisely in the case of new or distinctive products, where, according to the authors, companies have the widest discretion and are most apt to resort to formula methods in general and target-return pricing in particular [5, p. 930], that profit maximization stands out most clearly in these studies. One final illustration is General Electric's description of the considerations that went into pricing its new portable dishwasher: its unwillingness to use price as a selling point because it believed demand inelastic at the outset; its expenditure of "another $5.00 . . . on streamlining and in embellishment" which "made it look like another $30 to the customer" [4, pp. 59-60]. It is here that Lanzillotti's emphasis on the nonmaximizing characteristics of target-return pricing, with "fairness" and "reasonableness" as its prime determinant, seems most clearly inappropriate.[13]

5. These contentions derive some indirect support, finally, from Lanzillotti's observation that investment policy is itself a vital long-range aspect and determinant of short-run price policy [5, p. 940]. The rates of return pricing can seek and hope to elicit are themselves conditioned by the relationship of capacity to demand; and the choice among investment projects that give rise to production capacity is concededly made primarily on the basis of the respective rates of return promised. Even the biggest, price-leading companies profess to be powerless to do more than seek out what the market will take—an admission that is in any event clearly in conflict with the "public utility" pricing analogy; but what the market will take is itself heavily influenced by the investment decisions of the firms enjoying access to it.

## II. *Other Pricing Formulae*

Apart from the prevalence of full-cost pricing in one form or another, the authors tend to reject the assumption of maximization because of the variety of pricing methods different companies employ. It is difficult to disagree with Lanzillotti's conclusion that "no single motivational hypothesis such as profit-maximization . . . is likely to impose an unambiguous course of action . . ." [5, pp. 938-39]. But it does not follow, as he suggests, that "pricing policies are in almost every case equivalent to a . . . choice from among competing *objectives* rather than policies tested by any simple concept of profits maximization" [p. 939, stress supplied]. The one statement merely recognizes different possible methods for achieving the goal of maximization; the other demotes the goal itself to one among a host of "competing objectives." The first observation seems clearly justified by Lanzillotti's summary of the Brookings studies, to the effect that "one company will prefer stability, another will seek to expand its market share, or to engage in continuous dis-

[13] It is here that maximizing considerations stand out most explicitly in his own discussion as not merely the primary but virtually the exclusive determinant [see 5, pp. 930-31, including note 10].

covery and pre-emption of new fields, while others will be content to meet competition" [5, p. 939], that some companies will emphasize product quality, others service, still others price, and so on. The second observation is not.

It would take a more intensive analysis than is possible here of the reasons the various companies offered for using different methods of pursuing different proximate goals in their pricing to support this contention that the ultimate objectives might none the less have been the same. A few citations must suffice. The discussion of the strategy of the big meat packers, where price policy on the buying side is directed primarily at preserving market shares [4, pp. 46-47], runs clearly in terms of rational oligopsony.[14] The necessity for Standard of Indiana to abandon its hitherto rigid formula pricing methods after 1948, in order to preserve its shrunken market against competitive inroads, again hardly requires complex or novel explanation, any more than does the consistent price-following policy of Gulf Oil and National Steel. The fact that American Can aims at a fixed margin above the cost of tinplate, preferring to compete primarily on the basis of service; that Kennecott, by far the largest seller of copper to independent fabricators [4, p. 179], tries to stabilize market price; that companies with large shares of their market try to avoid price cutting as a competitive strategy except where demand appears to be very elastic—these various policies hardly vitiate, on the contrary they seem explicable primarily in terms of, the assumption of profit maximization. That the goal calls for different strategies and suggests different rule-of-thumb procedures in different market contexts does not disprove the uniformity of objective.

The fact that oligopolists usually refrain from increasing published prices except in response to increases in direct costs is often cited as evidence of a failure to maximize. It may indeed be, except in some rather attenuated, long-run sense. However, it is not difficult to reconcile such behavior with pure marginalism: if the degree of monopoly power (i.e., the inelasticity of demand) is (or seems to the oligopolist) unchanged, the market will in fact bear (or seem to bear) a fixed percentage mark-up over marginal costs. The profit-maximizing price will therefore change only with changes in direct costs and not with changes in demand, so long as marginal costs are constant within the relevant range [8, p. 459]. Hence cost-plus pricing, which tends to produce price stability and the appearance—indeed, it can hardly be doubted, at times like 1946-48 an actuality—of charging less than the traffic will bear, is not necessarily inconsistent with even short-run profit maximization.[15]

[14] Conceivably, as Lanzillotti points out [5, p. 934, n. 13], the long-run investment policies that must also be explained if one is to explain the long-run stability of market shares are irrational; but the monograph offers no evidence on this score.

[15] It is not clear how heroic are the assumptions required to equate short-period profit maximization with cost-plus pricing in this fashion. However the two crucial assumptions —that marginal costs be constant and the elasticity of demand unchanging (at least so far as business men can tell or it is feasible for them to tell) within the relevant range—seem realistic for manufacturing. The discussion of American Can in the Brookings study seems to illustrate some such situation, emphasizing as it does the importance of direct raw material cost in the final price of the cans, and the reported lack of interest of canners in the absolute level of can prices. So the authors refer to American Can as the "transmission belt for passing material and labor cost on to consumers with an inelastic demand"

It is of course impossible to state to what extent these variant pricing procedures and proximate goals have in fact been explicitly and rationally related to profit calculations; it seems certain that they have not been uniformly and consistently. But the evidence in the Brookings monograph certainly does not justify the opposite conclusion—that they represent company objectives alternative, and equal in importance, to profit maximization. It seems to me they are best explained, primarily, as alternative roads to the same destination.

III. *Conclusion*

Information of the kind collected in the Brookings study is extremely valuable but difficult to interpret. The underlying significance is seldom apparent on the surface; and it is very easy to confuse rule-of-thumb procedures with underlying policies, self-justifying *ex post* explanations with actual objectives. These instructive case studies enrich the abstract concepts of market power and its exercise; but they do not seem to me to undermine the economist's traditional concern with that power, and his corresponding assumption that to the extent such power exists it will be exercised in the interest of those who possess it.

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[4, p. 256; see also pp. 260-61]. Incidentally, however, the "apparent" unconcern of canners with "price reductions" [p. 261] may have been nothing more than the reflection of their lack of price alternatives and the fact that most of them were forced to buy cans under long-term requirements contracts tied in with the lease of can-closing machinery; the authors themselves later comment on the increased buyer interest in price since the antitrust decree [p. 267].

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## Pricing Objectives in Large Companies: Reply

I welcome the comments of Professors Adelman and Kahn; the questions they raise should serve both to elucidate some of the propositions set forth in my article and to refine some of its interpretations and conclusions.

### I. *A & P's Pricing Policies and Practices*

Adelman contends that my article on pricing objectives in large companies: (1) makes two inconsistent statements concerning A & P's pricing policy and practices; (2) has "repeated and enlarged an error of the District Court," and (3) offers scant evidence to support the conclusions on A & P's pricing practices.

Regarding point (1), the difference between the two statements is the difference between *over-all* policy and *selective* policy. The first reference to A & P [9, p. 922] is included in a general description of the importance of the twenty companies in their respective industries and the extent to which they "dominate a whole industry and are price leaders." The second reference [9, p. 935] has to do with the pricing practices in specific markets of several companies, including A & P. In the first context, it is my position that A & P does not dominate nor make decisions for a whole industry in the sense that U. S. Steel does. However, the District Court decision is replete with evidence of how A & P brought its absolute resources to bear against actual and potential competitors in particular localities—a pattern of behavior which A & P has rationalized as "meeting competition."

On the issue of the A & P case in question, Adelman is rolling a rusty hoop. If, as Adelman asserts, A & P's objective in operating unprofitably for several weeks was purely and simply to reduce its "expense rate," one is entitled to inquire: Why was this effort to reduce expense rates not undertaken earlier? Was it mere coincidence that A & P's prices were lowered *the day before* the opening of a Giant Eagle store in Pittsburgh, that *six weeks following* the opening of a Food Fair store in Richmond a "special competitive compaign" was inaugurated by A & P, and that "special programs" were set up *three weeks ahead of* the opening of other Food Fair stores (see below)?

The A & P cases and Adelman's defense of A & P have been so widely read and extensively reviewed [1] [2] [3] [4] [5], that it seemed unnecessary to evaluate once again the facts and evidence upon which the District Court based its decision, upon which the Circuit Court of Appeals affirmed the decision, and upon which A & P had to make its determination not to appeal further. However, since Adelman's observations would leave the impression with the reader that I had only very skimpy evidence for my conclusions on A & P's practices, some of the numerous examples of A & P's policy of selective price cutting, *planned* losses, and subsidy policy must be mentioned. Many examples could be cited, but because of space limitations I am not able to provide all of the relevant quotations from the decisions of the courts.

Many units of the company operated at a loss over extended periods of time [13, pp. 664-69], e.g., Boston and Providence from 1934 through 1941;[1]

[1] Even after all credits, the Boston and Springfield units lost money for five years in succession, and the Los Angeles unit lost $1,406,194 over the 1932-1940 period [4, p. 18].

Toledo, 1932-1938; Indianapolis, the same years except 1936; and Detroit and Cincinnati, 1932-1937.[2] Particular stores were placed in special price zones allowing them to sell at lower prices than other stores. The best example of this practice concerns a store in the Pittsburgh unit in 1939:

> . . . A & P lowered the gross profit rate in markets adjacent to the Streamline Markets, recouping losses with profits from other division markets not in proximity to Streamline and from profits passed on from headquarters. An attorney brought this to the attention of John Hartford, threatening suit if it continued, saying: 'On Friday, Mr. King, Vice President of your company called on two executives of the Streamline Markets, in one of their new stores . . . and in a loud voice, publicly, before the employees and customers, threatened to commence a campaign to bring about the financial ruin of Streamline Markets'; that King boasted of 'past achievement of financial ruin' to other competitors; and said that he was going to cut the meat and produce to cost in A & P stores nearest the Streamline Markets; that he would 'put a store in Kittanning that will take care of that store of yours. I will put one in Butler right opposite your store that will finish that store. I am going to turn all my guns on you.' Rezoning followed, 'In the case of Streamline we set up a special price zone called zone 'M'. We zoned all of the Streamline points, every one of them, and the prices in the zone 'M' were lower than in any of our regular supermarket zones' [13, p. 667].[3]

The two pages cited from the Government Brief [15, pp. 909 and 931], which Adelman states: "say nothing about selective price reductions against a new store," do say a good deal about how those selective price cuts were made possible, namely, the company's "subsidy" practice.[4]

[2] "In 1936 it was decided that all units in the [Central Western] division should operate with a profit except Detroit, which was permitted to plan operations at a loss of $3,000 per week for 20 weeks. . . . The policy was expressed by [John A.] Hartford [President and Chairman of Divisional presidents] and [J. M.] Toolin [President of Central Western Division] as follows: 'experience . . . has always shown that an aggressive sales policy with a steady increase in volume of business turns the expense rate in the right direction and, *with the generous attitude of headquarters giving us permission to, literally speaking, swap dollars*, the organization should be imbued with a new spirit of confidence.' Toolin wrote May 28, 1941 to Indianapolis, *'we were successful in staying off the other competitor there whose days now I think are numbered.'* On cross-examination he stated that this was a figure of speech" (italics added) [13, p. 665].

[3] It should be noted that Hartford was upset about King's acts, stating that "A & P could better afford the loss of $500,000,000 in sales than to let the Streamline story be made public, and a recurrence of such a thing would simply be ruinous beyond measure." Hartford attempted to correct the situation, but King remained in the employment of the company [13, p. 667].

[4] Not all the operations that were in the black after allocation of "headquarter's profits" were innocent in the sense that any resultant squeezes mirrored only the acceptable benefits of integration. As Dirlam and Kahn point out, "Entirely apart from the fact that many of the profits were the product of coercive bargaining, it is questionable whether all of them should for analytical purposes be distributed among the retail units before deciding whether any of the latter operated at losses. . . . To the extent that the retail stores suffered bookkeeping losses before allocation of that portion of the profits

These are illustrative of the practices of A & P vis à vis potential competitors.[5] Whether the cited examples of operations at a loss constitute a deliberate policy to that effect can only be determined from an examination of A & P's *entire* course of action. But, even granting (a) there is evidence that some of A & P's losses were forced upon the company, and (b) the difficulty of attributing the demise of competitors or the maintenance or increase in A & P's market share to a single causal factor (indeed, this is difficult even in the "obvious" cases of the old predatory monopolies such as the Standard Oil trust), the numerous instances of A & P officials threatening or attempting to drive specific rivals out of business, provide a reasonable basis for the conclusion that A & P employed predatory tactics. In the instances cited, A & P officials were *consciously* reducing prices below levels they thought desirable or had set as a norm—otherwise, why the necessity of requesting permission from headquarters to sell below cost (however defined) for specific periods of time? It seems to me that in these instances A & P had a deliberate price policy; it was not simply "meeting competition."

Finally, there is Adelman's statement: "In theory, because of easy entry and numerous competitors, reasonable profit-seeking men would not consider an impossible policy of 'preventing competition,' except in the popular sense that continuous low prices are an 'unfair' preventive." Evidently A & P officials' concept of business behavior does not coincide with Adelman's explanation of rational action. Moreover, what may seem like "easy entry and numerous competitors" from the Olympian heights of an economist fails to appear in this context at the level where a few months is the long run, and stores do fail. Economists call this pure competition, but the "running-in-the-red" policy of A & P was of great concern to the position of individual firms immediately, and not-so-immediately involved. This is not to deny that A & P's policy might be characterized, broadly, as one of attempting to achieve large volume through low prices; but this is irrelevant for my conclusion "It seems also that in some cases the companies are not simply meeting competition—they are preventing it" [9, p. 935].[6]

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which headquarters investments could have earned even though not integrated with retailing, non-integrated competitors were being subjected to unfair competitive pressures unrelated either to the relative inefficiency of their retailing or to the superior efficiency of integrated operations" [3, p. 213].

[5] Similar practices, including permission to units to sell with abnormally low or nonexistent gross-profit rates and use of A & P employees to buy up "specials" of competitors, were followed in other cities in the New England division; in Birmingham and Dallas of the southern division; in Buffalo, Columbus, Youngstown, Oberlin, Monongahela, and Altoona of the central division; in Scranton, Washington, Endicott, Kingston, and Stroudsburg of the Atlantic division; and Murphysboro in the middle western division. For a complete listing of the practices and areas affected, see [15, pp. 628-986].

[6] In this connection, it seems to me that substantial similarity exists between the conclusion of Phillips' article on General Foods cited by Adelman, and the A & P situation, i.e., in those cases where A & P responded vigorously to local price cutters. There, General Foods was attempting to demonstrate to rivals the advantages of recognized interdependence! "The deals of the 1940's were a clear sign that General Foods wanted to halt the drop in its market share. If it could convince competitors of the futility of aggressive

In view of space limitations here, I would invite the reader to examine the decisions of the District Court [13] and the Circuit Court of Appeals [14], and the other literature cited, especially [3] [4] [5] and [8], after which he can make a determination concerning Adelman's reference to "glaring errors of observation," as well as the locus of myopia in the A & P case.

## II. *Pricing Objectives and the Profits-Maximization Hypothesis*

Kahn has cast his net over a rather wide range of topics; his comments are directed in significant part at various sections and conclusions of the Brookings book *Pricing in Big Business,* as well as my article on pricing objectives. I shall limit my reply primarily to specific questions Kahn raises about the analysis and interpretations in my article, but inevitably must touch on some of the other issues he raises. In doing this I wish to make clear that I am not in a position to speak for Dirlam and Kaplan, and I cannot necessarily expect them to support my personal views on the varied comments Kahn makes.

As I understand Kahn, he believes the data set forth in my article are consistent with profits maximization—not the conventional profits maximization of marginal price theory, but what he calls a more "workable" hypothesis: ". . . that these large corporations typically price to maximize monetary profits—not day-by-day, to be sure, but to a large extent year-by-year and certainly over a fairly brief period of years." On the other hand, in footnote 4, Kahn states ". . . I have no serious quarrel with the doubts he expresses about maximization on a year-to-year basis." It seems to me that Kahn should (1) define with greater specificity his own modified "workable" concept of profits maximization, and (2) if the definitions are substantively different, he should indicate whether his profits maximization concept will perform the same function as the classical one. If he can draw a meaningful, and certainly a more precise, distinction between his concept and that of marginal theory, we are probably not very far apart in our thinking. But, if Kahn does not draw that distinction and, in effect, is at one with traditional marginal price theory, the task of reconciling his position with the empirical evidence becomes difficult indeed.

My position, in brief, is that whatever profits maximization may be construed to mean, it does not prove helpful in understanding pricing policies of large corporations. The concepts of target return and target market share do seem useful both to an understanding of pricing policies and for predicting the price behavior of these types of enterprises. Thus, the issue at hand is essentially whether by stating that target return is really another name for profits maximization Kahn has explained anything about the pricing policies of large corporations.

Two points need to be clarified before proceeding further. First, some man-

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behavior, the most likely pattern of 'competition' would indicate less activity, less virility." Also, "General Foods was in a position to maintain its share of the market, both national and western, if it chose to do so. This conclusion holds true with or without price discrimination though, of course, price discrimination would make things easier and more profitable" [11, pp. 693 and 696].

agement statements unquestionably should be taken as rationalizations, as my article indicates, especially those made in connection with "meeting or matching competition" (if any doubts still remain on this subject) and the administration of prices through price leadership "as a means of approximating market equilibrium" [9, pp. 922, 934-35, and 937-38]. However, I do not classify profits targets and market-share targets as mere rationalizations. These objectives provide management with a set of meaningful, useful, and rational yardsticks.[7]

Second, it seems essential to emphasize that the basic issue here concerns the determinants of *short-run* pricing decisions of oligopolistic corporations. Kahn has attempted to integrate, or at least to relate in some meaningful manner, the pursuit of target return and other long-run pricing objectives with his concept of profits maximization. It is not enough, however, to state that the objectives of management are *ex post* rationalizations or self-justifying explanations of price behavior; nor does it suffice to argue that the pricing objectives, whatever management states, amount to what the pricing executives believe the objective circumstances of the market will bear. Pricing decisions made on the basis of the company objectives cited can be related to the well-being or "utility" of the corporation, the long-run collective interests of management and stockholders, or some level of satisfactory profits (e.g., a specific target rate of return), but Kahn has not shown us how they are related specifically to *monetary* profits maximization. In order to reconcile the pricing objectives of the firms studied with even Kahn's attenuated form of the profits-maximization assumption, it is essential to demonstrate a functional relationship between maximum profits in the Kahn sense and various degrees of realization of the specific objectives cited. Reconciliation of target return with profits maximization requires, among other things, some rather stringent assumptions regarding costs and demand elasticity, as well as proof that firms were driving for maximum profits with full knowledge. If one wishes to assume that marginal costs remain constant and that the demand curves facing the firms are iso-elastic, secularly and cyclically, perhaps profits maximization can be reconciled with profits targets, stable margins, and other objectives; but this is genuinely Pickwickian.

Kahn has demonstrated that one can make even his modified profits-maximization concept so plastic that it is difficult to imagine any behavior which is not consistent with his hypothesis, thus making it incapable of refutation. He is not disarmed by anything; no event is inconsistent with his theory. In short, Kahn's main point is that variability in rates of return demonstrates profits maximization and that differences in rates of return merely reflect differences in charging what the traffic will bear. But what predictive value does such an hypothesis have for the types of corporate enterprises in question? Kahn nowhere argues that such an assumption would permit him to predict

[7] These objectives promise "reasonably satisfactory profits in the long run and a maximum of stability in [the firm's] relations with customers, suppliers, and competitors. These conditions suggest that many businessmen are likely . . . to substitute the principle of satisfactory profits for that of profits-maximization" [7, p. 271], see also [6, pp. 142-74].

that U. S. Steel would raise its prices sharply in the midst of a recession, that Ford would raise its prices to the level of General Motors' in the fall of 1957, or that during the period of grey markets in automobiles and steel following the second world war, the manufacturers would set prices below what on the basis of market considerations alone unquestionably could have been possible.[8] Certainly this behavior falls outside of the concept of profits maximization of marginal theory. Does it also fall outside of Kahn's definition?

If Swift's management were really attempting ultimately to maximize profits in the monetary sense, it seems that—despite the limitations imposed by the 1920 consent judgment— they would have reinvested the company's earnings and depreciation funds much more in some other lines, perhaps even move completely out of or at least substantially reduce the present size of their meatpacking operations. If A & P were really attempting to maximize profits in the same sense, it long ago should have relaxed its pressures on competitors. Other retail food chains, such as Food Fair, First National, Jewel Tea, and Grand Union for years have averaged considerably higher ratios of net operating incomes to sales than A & P [12, p. R79]. Beginning in 1937, and for many years thereafter, A & P evidently was interested in earning a predetermined dividend of $7.00 per share [15, pp. 85, 177, 631, and 637], and with the public distribution of the company's stock a different over-all company goal likely will emerge as it did with Ford.

Furthermore, if U. S. Steel and General Motors were attempting to maximize profits in the monetary sense advanced by Kahn, then seemingly these firms have a large number of alternative policies which could be pursued. For quite an extended period of years U. S. Steel was inefficient (just as many, perhaps most, firms have various kinds of inefficiencies that persist), and by eliminating them (via the Ford, Bacon and Davis report) the corporation's profits were increased. Was U.S. Steel maximizing in the Kahn sense before? Is it now? Moreover, using Kahn's concept, is it possible to predict where General Motors will set its prices, and once established, how and on what basis prices are revised? To a limited extent this is possible by considering the previous price, knowledge about the cost of style changes, and the company's standard cost and target return—in other words, the procedures.

Thus, while Kahn is correct in stating that the subject of my article is pricing objectives and not procedures, objectives and procedures are very closely interrelated; and in some companies, in fact, procedures to a large extent determine price policy [9, p. 932]. I believe this is due largely to the problems of internal control; but whatever the reasons, the pricing executives in some of the companies studied seldom look beyond the particular formulae on

[8] In response to the suggestion that in view of G.M.'s very high rate of return in 1955 some of the gains from that year's exceptionally high volume might have been passed on to buyers in the form of lower prices, G.M. President Curtice replied: "They [our car prices] are as low as they can be *and still produce the indicated return on the net worth at the standard volume*. We can never be sure whether we are going to exceed the standard volume or whether the market place will be such that we will sell less than the standard volume" (italics added) [17, p. 3609]. In effect, G.M. had to get its target return on net worth.

which they base their pricing decisions, let alone consider the alternatives which the marginalist approach would ascribe to them. Pricing officials of these large corporations feel they do not need to have precise estimates of the price sensitivity of demand nor detailed information on current costs. They vastly prefer to use standard cost methods— systems which they feel provide them with more security and stability—and, annually, or semiannually, revisions can be made in these standards in the light of actual experience.[9]

In effect, while some of the findings on pricing objectives might be reconciled with some variant of the profits-maximization hypothesis, this does not mean that they are not also consistent with my position that profits maximization is not the *dominant* motive of the firm, particularly the large corporate oligopoly. Given empirical findings are likely to be consistent with, or at least partially reconcilable with, many hypotheses. The essential question is: which hypotheses are likely to yield the most useful and reliable short-run and long-run predictions for such issues as the effects of fiscal policy on the rate of change in prices and the effects of wage-rate increases on the general level of prices? The strength of the profits-maximization hypothesis, and perhaps the reason why economists have held on to it so tenaciously *for all market structures* is that, granted its heroic assumptions, it yields unique predictions with respect to many variables. The essential strength of the target-return thesis is that it has a much higher degree of plausibility and realism in a world where large firms have (a) such strong asset positions; (b) limited knowledge with respect to the possible cost and revenue alternatives created by (c) wide diversity of product lines, and the nature of multiprocess multiplant operations; (d) uncertainty regarding the shape and possible shifts in the short-period price-quantity function; plus (e) strongly entrenched market position, with all this entails by way of oligopolistic interdependence, antitrust pressures, and Congressional inquiry. Under these circumstances the short-run price will, I believe, be determined by some feasible *long-run* objective such as a predetermined target rate of return on invested capital and/or target market share. These firms do not appear to be *forced* by their objective environmental circumstances to attempt to maximize profits in the monetary

[9] Kahn is correct in stating that there is some investigation of demand for new products. I do not believe there is any real disagreement here; my position is that companies insist on the prescribed return *before* production is undertaken; Kahn is referring to pricing afterward. The attention given to elasticity of demand appears to vary primarily according to type and age of product and structure of the market. On established products, demand is considered mostly in terms of projections of total sales, or sales targets, with predominant emphasis on determining where the market is and forecasting secular growth. In the case of products subject to model changes, or new products, some effort is made to determine demand elasticity at different price levels. In general, however, total demand is taken for granted and not much consideration is given to alternative sets of small or large price changes. A similar picture exists on cost data. Actual costs appear to have little influence on the current structure of prices. Even in companies where the most detailed cost data are available to pricing officials, the typical practice is to base prices on "standard cost" at some long-run "normal" output rate. Of course, standard costs will reflect actual cost changes over time, but at any given time, and over the short period of price theory, prices will not bear any necessary or direct relationship to actual, full, direct, or incremental costs in any meaningful sense [10, pp. 446-47].

sense, even though the problems of internal control are perhaps such as to make this an alternative divisional goal (to the corporation target) for some product lines. In point of fact, however, the problems of internal control, the "banker mentality" (liquidity-solvency motives) of large firms and the fear of temporary financial embarrassment or inability to expand as rapidly as management feels necessary, outweigh the profits-maximization problem for the large corporation in the determination of price policy. In effect, if the large firm is to be a profits maximizer, it will be of its own free will, not the objective compulsions of the market.[10]

Finally, because of the associations Kahn has made with my statement that the target-return-minded companies tend to behave more and more like public utilities, I must admit misgivings about my use of the analogy. The reference was made *not* to stress that some of these firms are more "public spirited" than others in the group, or than other firms generally—at which point I am in agreement with Kahn. Rather, I wished to emphasize the similarity of approach to pricing in the sense of *planning* for some average rate of return to which some assurance of realization could be attached, not the *fairness* of the rate.

I also wish to make clear that I do not believe the findings presented on pricing objectives undermine the economist's traditional concern with market power, or that we can assume that market power will not be exercised in the interest of those who possess it; to the contrary, the findings should serve to intensify that concern [cf. 9, p. 940 and 16, p. 296 ff.]. It seems to me that one can have reservations about the realism and usefulness of the conventional price theory apparatus without lessening his concern about the problems of monopoly and competition.

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[10] The fiscal and monetary policies of an economy working under a "Keynesian guarantee"—whether these policies be geared specifically to attempting to maintain full employment, a stable price level, or both simultaneously—tend to reinforce the inclination and ability of the large corporation to price on the basis of a predetermined target [10, pp. 453-56]. These relationships need to be worked out in greater detail, but the limited available evidence lends some support to the hypothesis that the environmental constraints operating upon the corporate oligopoly—intracompany, intra-industry, and economy-wide—simply do not press upon these firms in a manner that forces them along profits-maximizing lines.

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## Industrial Growth in the Soviet Union: Comment

In a paper delivered at the 1957 American Economic Association meetings, G. Warren Nutter presented a number of different indexes of industrial production for the Soviet Union. These indexes are of three kinds, of different scope and construction: (1) industrial materials indexes, that is, materials at an intermediate level of fabrication, weighted by price; (2) finished goods indexes, weighted by price; (3) indexes of "All Industrial Products," both intermediate and final goods, weighted by value added or employment.[1] Out of these he selected the industrial materials indexes as the most reliable and compared them graphically with a single comprehensive industrial production index for the United States, consisting essentially of the Federal Reserve

[1] See [2]. Nutter's paper was a preliminary summary of a project on Soviet industrial production at the National Bureau of Economic Research.

Board index for 1929 to 1955, the Fabricant-Barger index for 1899 to 1929. Partly on the basis of this comparison Nutter concluded that: ". . . [Soviet industrial growth] has proceeded at about the same average pace as American industrial growth over the same period (i.e., 1913-55)" [2, p. 407].

This conclusion is surprising since most students of the Soviet economy have thought that Soviet industry has been growing considerably faster than U. S. industry. This paper will argue that Nutter's comparison of materials indexes for the Soviet Union with a comprehensive index for the United States is improper, and that a comparison of a U. S. materials index with Nutter's Soviet materials indexes does not support the conclusion that U. S. industry has kept pace with Soviet industry.

Nutter regarded the industrial materials indexes as the most satisfactory indicators of general growth trends "because they are not senstive to the essentially insurmountable problems of identifying and pricing relevant physical units in the swiftly and radically changing mix of producer durables" [2, p. 402]. It is true that industrial materials have the advantage of homogeneity and measurability, but the exclusion of mix changes and new products may change the trend of the index significantly. It is clear that some productive activity has taken place when iron is beaten into a ploughshare. When iron is made into a tractor it is possible that even more productive activity has taken place. The question is whether an industrial materials index is likely to have the same trend over the long run as a comprehensive industrial index including finished goods, such as the FRB or Fabricant-Barger indexes. Nutter presented no defense of the comparability of the Soviet and U. S. indexes which he employed. However, he cited as a precedent for his Soviet industrial materials indexes the industrial materials index for the United States developed by Geoffrey Moore [1]. If Nutter's indexes for the Soviet Union are similar to Moore's for the United States, it will be informative to update Moore's index and to compare it with the comprehensive U. S. indexes used by Nutter and with Nutter's Soviet materials indexes.

An examination of the indexes reveals that Nutter's are quite similar in scope and construction to Moore's for the United States. The former cover 37 intermediate industrial products (metals, fuels, construction materials and so on) and 17 nondurable consumer goods (flour, butter, fabrics, and so on). The commodities are weighted by prices, the prices being adjusted wherever necessary to exclude the cost of nonindustrial materials used in production. Moore's index includes 22 intermediate industrial products (metals, fuels, construction materials, etc.) and 25 products of agricultural origin (fibers, foods, etc.).[2] In Moore's index commodities are weighted by value except for those of agricultural or foreign origin, for which value-added weights are used. The most important difference in coverage seems to be the inclusion of hydroelectric power and the exclusion of aluminum (for lack of data) in Nutter's sample.

Table 1, below, compares the comprehensive index for the United States used by Nutter with Moore's industrial materials index. Moore's original index from 1913 to 1939 has been revised to include hydroelectric power [3,

[2] The industrial classification of items included in Moore's and Nutter's materials indexes is available upon request to the authors.

TABLE 1—U. S. INDEXES OF INDUSTRIAL PRODUCTION, 1913–1957

| | Nutter's Comprehensive Index[a] | | | Moore's Industrial Materials Index[b] | | |
|---|---|---|---|---|---|---|
| | 1913=100 | 1939=100 | 1947=100 | 1913=100 | 1939=100 | 1947=100 |
| 1913 | 100 | 53 | 31 | 100 | 60 | 40 |
| 1914 | 94 | 50 | 29 | 95 | 57 | 38 |
| 1915 | 110 | 59 | 34 | 104 | 63 | 42 |
| 1916 | 130 | 70 | 40 | 120 | 72 | 48 |
| 1917 | 130 | 70 | 40 | 124 | 75 | 50 |
| 1918 | 128 | 68 | 40 | 120 | 72 | 49 |
| 1919 | 113 | 60 | 35 | 113 | 68 | 46 |
| 1920 | 125 | 67 | 39 | | | |
| 1921 | 100 | 53 | 31 | 95 | 57 | 39 |
| 1922 | 127 | 68 | 40 | | | |
| 1923 | 144 | 77 | 45 | 136 | 82 | 55 |
| 1924 | 137 | 73 | 43 | | | |
| 1925 | 153 | 82 | 48 | 139 | 84 | 56 |
| 1926 | 164 | 88 | 51 | | | |
| 1927 | 165 | 88 | 51 | 148 | 89 | 60 |
| 1928 | 172 | 92 | 54 | | | |
| 1929 | 188 | 101 | 59 | 165 | 99 | 67 |
| 1930 | 155 | 83 | 48 | | | |
| 1931 | 129 | 69 | 40 | | | |
| 1932 | 100 | 53 | 31 | 99 | 60 | 40 |
| 1933 | 119 | 64 | 37 | | | |
| 1934 | 129 | 69 | 40 | | | |
| 1935 | 150 | 80 | 47 | | | |
| 1936 | 180 | 96 | 56 | | | |
| 1937 | 194 | 104 | 60 | 170 | 103 | 69 |
| 1938 | 155 | 83 | 48 | 142 | 85 | 57 |
| 1939 | 187 | 100 | 58 | 166 | 100 | 67 |
| 1940 | 213 | 114 | 66 | 188 | 113 | 76 |
| 1941 | 279 | 149 | 87 | | | |
| 1942 | 336 | 180 | 105 | | | |
| 1943 | 402 | 215 | 125 | | | |
| 1944 | 398 | 213 | 124 | | | |
| 1945 | 342 | 183 | 107 | | | |
| 1946 | 290 | 155 | 90 | | | |
| 1947 | 321 | 172 | 100 | 248 | 149 | 100 |

[a] Nutter's over-all industrial production index is a chained-link type constructed from com-ponent indexes for manufacturing, mining, and electric power production. From 1913 to 1929 the manufacturing indexes are from Solomon Fabricant, *The Output of Manufacturing Industries, 1899–1937*, New York 1940, and the mining indexes from H. Barger and S. Schurr, *The Mining Industries, 1899–1939*, New York 1944. After 1929 the Federal Reserve Board's manufacturing and mining indexes have been used. Electric power has been added to the manufacturing and mining indexes through 1955.

[b] Moore's index is based on 1947 weights for 1939–1957 linked to the materials index based on 1939 prices for 1919–1939 and the index based on 1914 weights for 1914–1919. Data for the years before the second world war are given by Moore with the exception of the census years 1921–1929 which were calculated by the authors using the procedures noted in Moore's appendix tables. Hydroelectric power has been added to the index for all years. Rubber consumption, including natural, synthetic and reclaimed, has been substituted for natural rubber imports in Moore's sample for 1939–1957. The authors have extended Moore's index from 1939–1957.

TABLE I—*Continued*

| | Nutter's Comprehensive Index | | | Moore's Industrial Materials Index | | |
|---|---|---|---|---|---|---|
| | 1913=100 | 1939=100 | 1947=100 | 1913=100 | 1939=100 | 1947=100 |
| 1948 | 337 | 180 | 105 | 252 | 152 | 102 |
| 1949 | 315 | 168 | 98 | 227 | 137 | 92 |
| 1950 | 365 | 195 | 114 | 262 | 158 | 106 |
| 1951 | 388 | 207 | 121 | 276 | 166 | 111 |
| 1952 | 401 | 214 | 125 | 264 | 159 | 107 |
| 1953 | 434 | 232 | 135 | 282 | 170 | 114 |
| 1954 | 408 | 218 | 127 | 262 | 158 | 106 |
| 1955 | 454 | 243 | 141 | 292 | 176 | 118 |
| 1956 | 459 | 246 | 143[c] | 300 | 181 | 121 |
| 1957 | 459 | 246 | 143[c] | 297 | 179 | 120 |

[c] Nutter's comprehensive index was not computed after 1955. The FRB index which differs from the over-all index by the exclusion of electric power production has been substituted.

Vol. 2, p. 107], since it is included in Nutter's Soviet materials index and all electric power is included in his comprehensive U. S. index. The extension of Moore's index plus hydroelectric power from 1939 to 1957 was calculated by the authors using 1947 weights. Moore's index has a significantly slower trend over the whole period except for 1929 to 1939. For 1913 to 1929 Moore's index gains 65 per cent against 88 per cent for the comprehensive index (Fabricant-Barger in this period). From 1929 to 1939 both indexes are almost unchanged. Moore, who was perhaps more interested in short- than in long-run changes, evaluated his index as follows: ". . . although the assumption of identical changes in industrial materials and 'total' production is a fair first approximation, it can certainly be improved" [1, p. 39].

For the period 1939-1957 the industrial materials index grows considerably more slowly than the comprehensive (FRB) index. The increases for 1939-1947 are 49 and 72 per cent respectively; for 1947 to 1957, 20 and 43 per cent. Writing while the war was still going on Moore said:

> As for a comparison that spans the war, it seems that unless there is a revolutionary change in the composition of peacetime output, involving a greater proportion of goods requiring much labor relative to materials, our hypothetical peacetime index of industrial production should correspond rather closely to an index based on materials output. Such a revolution may well be in process; but if not, the implication is that after the war the Federal Reserve index will resume its pre-war position relative to materials output, and that total man-hours of industrial employment will do likewise [1, p. 51].

It appears that a revolution has occurred, although it may be rather a continuation of a secular change in the structure of manufacturing which was obscured or retarded by the depression of the 'thirties.

The industrial products chiefly responsible for the more rapid growth of the FRB index are the same as those which Moore discovered were the chief cause of the divergence of his index and the FRB index in wartime—namely,

machinery and equipment of all kinds. Metal fabricating, including machinery and equipment, whose weight is 28.5 per cent of the FRB index and which grows to 377 per cent of 1939 by 1957 is, of course, not in Moore's materials sample. In Table 2 Moore's index extended is compared to the Census index of manufacturing for 1939 to 1947 and to the FRB index for 1947 to 1957 by industrial categories.

Metal fabricating is the most glaring difference but not the only one. There have been drastic changes in product mix including a bewildering array of new products since 1939 in many industrial categories. These are at least partially reflected in the FRB and Census indexes. In a fixed materials sample they are not included. The results indicate that these new products have involved an increasing degree of fabrication of raw materials. Thus, for the United States at least, an industrial materials index has a significant downward bias relative to more comprehensive indexes.

The foregoing discussion has not raised the question as to whether the FRB index is "right" or the best possible index. The inclusion of many series based on man-hours rather than on product data has been viewed with much suspicion. Moore showed that a substantial part of the rise in the FRB index from 1941 to 1943 was accounted for by the man-hours series. Only two comments need be made here. First, the Census index of manufacturing in Table 2 did not use man-hours data and the use of man-hour series in the FRB index was greatly reduced in the 1947 revision. Secondly, we are not concerned with correctness of indexes here but with comparability. For whatever reason the FRB index used by Nutter rises faster than Moore's industrial materials index.

If, as shown above, a materials index has a substantial downward bias in the United States then such an index may have a similar bias in the Soviet Union. Nutter's materials index for the Soviet Union should properly be compared with Moore's index for the United States, rather than with the Fabricant-Barger and FRB indexes. As a matter of interest, an index for the United States was calculated using Nutter's sample and procedure. This resulted in indexes of 147 for 1947 and 181 for 1956 as a per cent of 1939 in 1947 weights.[3] Thus it appears that Nutter's sample gives approximately the same growth as Moore's.

Nutter's chart comparing industrial production in the two countries is reproduced below, with Moore's index superimposed from 1913.[4] A glance at the chart confirms the view that Moore's industrial materials index presents an entirely different picture of growth from the comprehensive index used by Nutter. Comparing Moore's industrial materials index for the United States

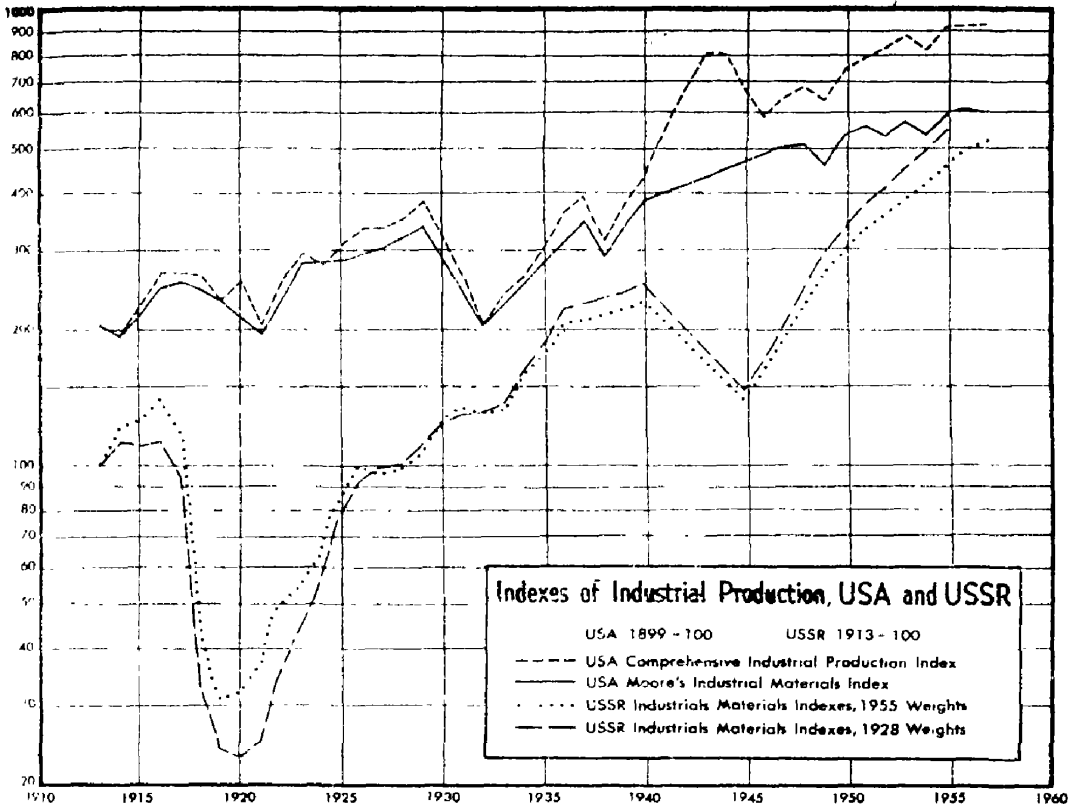
[3] Nutter's sample could be duplicated for the United States except for the following items: oil shale, peat, firewood, sand lime and slag bricks, asbestos shingles, and red lead.

[4] The vertical distance between the U. S. and USSR indexes has no significance as to relative levels of production in the two countries. Hence the imminent crossing of Nutter's and Moore's indexes is only an accidental result of Nutter's choice of base years and does not mean that Soviet industry is actually approaching U. S. industry in absolute magnitude. A comparison of physical quantities of materials in the countries indicates that Soviet industry is less than one-half the size of U. S. industry.

TABLE 2—MOORE'S INDEX[a] AND CORRESPONDING CENSUS AND FRB INDEXES BY INDUSTRIAL CATEGORY; 1947 WEIGHTS

| | | 1947/1939 | | 1957/1947 | | Moore Weights | FRB Weights |
|---|---|---|---|---|---|---|---|
| | | Moore | Census[b] | Moore | FRB | | |
| Manufactures | | 147 | 169 | 113 | 145 | 72.78 | 90.02 |
| SIC 20 | Food and beverage manufactures | 128 | 151 | 103 | 111 | 15.48 | 10.73 |
| SIC 21 | Tobacco manufactures | 153 | 146 | 131 | 112 | 2.16 | .78 |
| SIC 22 | Textile mill products | 147 | 127 | 82 | 98 | 12.24 | 6.32 |
| SIC 23 | Apparel and allied products | — | 121 | — | 114 | — | 5.55 |
| SIC 24 | Lumber and products | 142 | 127 | 94 | 113 | 6.56 | 3.09 |
| SIC 25 | Furniture | — | —[c] | — | 120 | — | 1.64 |
| SIC 26 | Paper and allied products | 162 | 146 | 186 | 160 | 1.50 | 3.46 |
| SIC 27 | Printing and publishing | — | 145 | — | 152 | — | 5.47 |
| SIC 28 | Chemicals and allied products | 134 | 200 | 138 | 190 | 0.65 | 6.84 |
| SIC 29 | Petroleum and coal products | — | 153 | — | 145 | — | 2.50 |
| SIC 30 | Rubber products | 185[d] | 179 | 123[d] | 127 | 4.38 | 1.47 |
| SIC 31 | Leather and products | 112 | 115 | 82 | 98 | 0.84 | 1.73 |
| SIC 32 | Stone, clay, and glass products | 154 | 167 | 159 | 158 | 1.25 | 2.82 |
| SIC 33 | Primary metals | 157 | 191 | 127 | 127 | 27.72 | 6.70 |
| SIC 34–38 | Metal fabricating[c] | — | 222 | — | 170 | — | 28.52 |
| Minerals | | 152 | 147[e] | 132 | 128 | 27.22 | 9.98 |
| SIC 10 | Metal mining | — | —[e] | — | 115 | — | .82 |
| SIC 11 | Anthracite mining | 111 | —[e] | 44 | 45 | 1.39 | .36 |
| SIC 12 | Bituminous mining | 160 | —[e] | 77 | 78 | 8.84 | 2.32 |
| SIC 13 | Crude oil and natural gas | 154 | —[e] | 163 | 158 | 15.63 | 5.67 |
| SIC 14 | Non-metallic minerals | 136 | —[e] | 212 | 149 | 1.36 | .81 |
| Industrial Production | | 148 | 166 | 118 | 143 | 100.0 | 100.0 |

[a] Moore's indexes extended by authors. Hydroelectric power is not included.
[b] See Census of Manufactures: 1947, *Indexes of Production.*
[c] Furniture and miscellaneous manufacturing included in metal fabricating.
[d] Rubber consumption, including natural, synthetic, and reclaimed, has been substituted for natural rubber imports in Moore's sample for 1939–1957.
[e] Federal Reserve Board's minerals index. The pre-1947 minerals index was adjusted to a benchmark measure for 1939 and 1947 by the Federal Reserve Board, but details for the minerals subgroups were not published. See *Federal Reserve Monthly Index of Industrial Production*, 1953 Revision, p. 12.

CHART 1

with Nutter's industrial materials indexes for the USSR average annual growths for various periods are:[5]

| | United States (per cent) | Soviet Union (per cent) |
|---|---|---|
| 1913-57 | 2.5 | 3.8-4.3 |
| 1928-57 | 2.2 | 5.9-6.5 |
| 1939-57 | 3.3 | 4.8-5.4 |
| 1950-57 | 1.8 | 8.3-9.2 |

These comparisons do not support Nutter's conclusion that American industrial growth has kept pace with Soviet industrial growth. We would state our conclusion as follows: Soviet industrial materials growth over the entire

[5] Only the 1955 weighted indexes for 1956 and 1957 were calculated in the worksheets made available by the National Bureau of Economic Research. The 1928 weighted indexes (the upper limits) were estimated by the authors from the ratio of the 1928 weighted index to the 1955 weighted index in 1955. The sample of materials for 1956 and 1957 is smaller than the sample for earlier years. Of the 54 commodities 13 are missing in 1956 and 18 in 1957. The index for the two years was calculated on the assumption that production of the missing items stayed constant from 1955 to 1957. This is a most conservative assumption.

Soviet period (1913-1957) has proceeded at a pace 50 per cent or more faster than U. S. industrial materials growth over the same period. For the period since an intensive industrialization drive was initiated under the five-year plans (1928-1957) Soviet industrial materials growth has proceeded at least twice as fast as U. S. industrial materials growth. While the rapid Soviet growth from 1950-1957 can be characterized as a short spurt the magnitude of this growth should not be overlooked. Industrial materials in the Soviet Union, by Nutter's index, have grown by 75 per cent in the seven years since 1950, starting from a level already above prewar. This is a larger percentage increase than that in the United States as shown by Moore's index since 1940 (58 per cent), and almost as large as the increase since 1929 (80 per cent).

The foregoing does not answer the basic question: What are comparative trends in over-all industrial production in the United States and the Soviet Union? If a materials trend can differ substantially from an over-all industry index over the long run, an international comparison of materials indexes cannot inspire much confidence. It is certainly true that at identical points in time the United States and USSR have been at different "stages" of industrial development and this may affect the relative trends between materials and finished goods of various kinds. However, the quantitative difference of the relative trends can not be assumed or estimated by analogy between one country at one period and another country at another period.

It is beyond the scope of this paper to examine all the indexes of Soviet industry which Nutter calculated or to present a definitive conclusion about the rate of growth of Soviet industry. However, in using any of Nutter's indexes in a comparison with indexes for the United States the same procedure should be used as in this paper, namely comparison with a U. S. index of comparable coverage and construction.

Can we not then compare Nutter's "All Industrial Products" indexes with comprehensive U. S. indexes? The answer is no. The available Soviet sample of finished products is substantially different from the U. S. sample. Nutter, like any other student of the Soviet economy, is limited to the sample of products reported by the Soviets. This sample is selected, we suspect, to include the more rapidly growing items and exclude more slowly growing or declining items. How important this is in earlier years it is difficult to tell. In recent years a factor of much greater importance is the exclusion of industries of direct military significance. In the FRB index some part of the rise since 1947 (and probably since 1939) is accounted for by new industries such as aircraft and electronics. In the Soviet Union also, aircraft, electronics, and other new industries have almost certainly been rapidly growing industries since 1937, the year before conversion to armaments production began. Inclusion of these industries would probably increase the rate of growth of the "All Industrial Products" indexes. It would increase the "Finished Industrial Products" indexes even more, since the relative weights of the missing industries would be larger.

A comparison of Nutter's "All Industrial Products" with the FRB index since 1937 suffers the same deficiency as his comparison of materials indexes

with comprehensive U. S. indexes. It includes for the United States and excludes for the Soviet Union some important and rapidly growing industries. The proper index to compare with his "All Industrial Products" would be recalculated U. S. index of the same general scope. This U. S. index should specifically exclude not only military equipment but all important new products not in the Soviet sample, production of which can reasonably be presumed to have begun in the USSR as well as in the United States. It is to be hoped that the final report by the National Bureau of Economic Research on industrial production in the USSR will include such carefully selected comparisons.

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## Industrial Growth in the Soviet Union: Reply

The paper by Dr. Greenslade and Miss Wallace presents some challenging evidence deserving much more careful study than I have been able to undertake in the short time available for a reply. My discussion here will therefore be brief and tentative, perhaps no more than suggesting the directions in which study might go. I hope to atone for present negligence by going into the issues more thoroughly in the monograph on Soviet industrial production I am now struggling to complete.

It must be said first of all that my comparisons of Soviet and American industrial growth were based on three different indexes of Soviet industrial growth, each with two weight bases [2, p. 408]. The indexes of industrial materials, which show a growth more or less midway between extremes, were chosen to represent the Soviet Union on a graph comparing Soviet and American industrial growth. But they were used only "for this broad look" [2, p. 407, n. 11], which was immediately supplemented by a table that presented the ranges of Soviet growth rates derived from all the indexes presented in the paper, and compared those ranges with growth rates derived from my index of industrial production in the United States.

My general attitude about the indexes for Soviet industry was briefly, and perhaps elliptically, put as follows [2, pp. 398 and 402]:

> . . . in my judgment, the best indexes [of Soviet industrial growth] that can be constructed fall far short of the reliability we have come to expect from Western indexes; and, on balance, they tend to exaggerate Soviet industrial growth.

> The wide divergence of behavior, when coupled with knowledge of the concrete shortcomings of each index, makes it reasonably clear that there is *no single best way* to construct a Soviet index. It should also be emphasized that none of these indexes is based on anything approaching the amount of verifiable data encompassed in standard indexes of industrial production used in most Western countries. Hence none can be considered an accurate measure, by Western standards, of what it purports to measure. In my own judgment, the two industrial materials indexes are the most satisfactory indicators of general growth trends, *primarily* because they are not sensitive to the essentially insurmountable problems of identifying and pricing relevant physical units in the swiftly and radically changing mix of producer durables. [Italics supplied.]

Indexes of industrial materials were therefore chosen as "the most satisfactory indicators of general growth trends" for two reasons: first, they showed an intermediate rate of growth among indexes that were all presumed to exaggerate growth of the productive areas they were supposed to represent; and second, they avoided the difficulties involved in measuring growth of rapidly altering finished products.

This brief restatement of my earlier views serves my purpose here, which is merely to make clear the approach followed in my article. I will not press the arguments in favor of using industrial materials to measure Soviet industrial growth, because this is ultimately a matter of judgment on which there can be legitimate and unresolvable differences of opinion, and because use of such an index is not crucial for my comparisons of Soviet and American industrial growth. It is more important to turn to the fundamental issues raised by Greenslade and Wallace.

They wield a two-edged sword. With the one edge, they attack comparisons of an index of industrial materials for the Soviet Union with a comprehensive index for the United States, arguing that an identical industrial materials index should be used for the United States and that it will show a slower growth than the comprehensive index. With the other edge, they attack comparisons between comprehensive indexes for both countries, on the ground that the one for the Soviet Union is bound to understate growth.

Let us consider first whether an index of industrial materials adequately reflects total industrial growth. We suppose for the moment that a representative sample of industrial materials is included within the index, that there is no doubt about the accuracy of the basic data, and that a satisfactory index-number formula is used. That is, we do not dispute that the index satisfactorily measures growth in output of industrial materials at some more or less definable stage of fabrication. Does the index also measure growth in total industrial production?

In principle, growth in the two areas of production could diverge for two broad reasons. First, productive activity may be growing at different rates above and below the stage of fabrication marking the boundary between materials and other industrial products. To use familiar terms, there may be either increasing or decreasing "intensity of fabrication." Second, productive

activity may be growing at different rates for industrial and nonindustrial consumers of materials. As these terms are usually defined and measured, industrial materials are used in significant quantities by such sectors as construction and transportation as well as by industry.

Having stated these conditions, we are not very far along the way to answering the basic question, and we cannot proceed much farther by a priori speculation on the nature and relative importance of the two factors. They could reinforce or offset each other. The issues are empirical, and they can be resolved only empirically, if at all. For all practical purposes, the only way to find out whether total industrial production grows faster or slower than industrial materials is to measure both and see.

This is what Greenslade and Wallace have done for the United States, and they find a markedly faster growth for industrial production than for industrial materials. Most of this divergence occurs since 1939, the period over which Greenslade and Wallace have extended Moore's index. For 1913-1939 the average annual rate of divergence is .5 per cent, while for 1939-1947 and 1947-1957 it is 1.8 per cent. These are striking findings and, if valid, suggest a fundamental change in recent years in the nature of industrial development in the United States. Are the findings confirmed by other evidence?

For the period 1947-1957, the standard Federal Reserve Board production index covering manufactures and minerals, as revised in 1953, is available in a breakdown between final products and industrial materials. The index for industrial materials shows an increase of 45 per cent, while the index for total industrial production shows an increase of 43 per cent, 2 percentage points smaller (see Table 1, old indexes). Along with a recent revision adjusting its index to census benchmark data for 1954, the FRB has also expanded the index to cover electric and gas utilities. Preliminary figures for the new indexes based on 1947 weights[1] show an increase of 54 per cent for both industrial materials and total production (see Table 1, new indexes). This evidence would not seem to confirm the findings of Greenslade and Wallace.

The FRB and extended Moore indexes of industrial materials seem to differ in four respects that might explain some of the divergent behavior. First, the FRB index includes some highly fabricated parts and components. However, this factor apparently does not influence the index appreciably, since an FRB index of "major materials," covering 18 basic materials and calculated for the special purpose of comparing production and capacity, is identical with a more comprehensive one (see Table 1, old indexes). Second, the product breakdown is finer and some materials are weighted at a slightly higher stage of fabrication in the FRB index than in the extended Moore index. For example, the iron and steel complex is represented by 6 products in the older version of the FRB index, and the weights for steel consumed in rolled products reflect value added in rolling mills as well as steel furnaces. In the Moore index, the iron and steel complex is represented by only one product, steel ingots and castings, weighted by its price. It is impossible to

[1] In the final revision the FRB may use a later weight base, which would probably reduce somewhat the figures given here.

determine how this difference in approach might affect the index without experimentation. Third, the FRB index does not cover materials used in the food and tobacco industry. If these items are eliminated from the extended Moore index, it rises by 20 instead of 18 per cent over 1947-1957; hence this factor seems to be of minor importance.

The fourth respect in which the two indexes differ is in the sample of materials covered. The FRB covers new products, such as synthetic fibers, not included in the Moore index. The latter was designed for the special pur-

TABLE 1—FEDERAL RESERVE BOARD INDEXES OF INDUSTRIAL PRODUCTION AND INDUSTRIAL MATERIALS
(per cent)

| | 1957 as Percentage of 1947 |
|---|---|
| Manufactures and minerals | |
| Old indexes[a] | |
| Total production | 143 |
| Industrial materials | 145 |
| Major industrial materials[b] | 145 |
| New indexes[c] | |
| Total production | 150 |
| Total except ordnance[d] | 146 |
| Manufactures, minerals, and electric and gas utilities | |
| New indexes[e] | |
| Total production | 154 |
| Total except ordnance[d] | 150 |
| Industrial materials | 154 |

*Source:* [1] and other materials compiled by FRB.

[a] Indexes based on 1947–49 weights, revised as of 1953.

[b] Index covering 18 materials calculated for the special purpose of comparing production and capacity.

[c] Preliminary indexes based on 1947 weights, revised as of November 1958. Derived from indexes for manufactures (152) and minerals (130) and their respective percentages weights (86% and 9.5%) in total index including utilities.

[d] Derived from index for ordnance (537) and its percentage weight (1%) in total index including utilities. Ordnance does *not* include business-type equipment (as trucks and instruments) purchased for military uses.

[e] Preliminary indexes based on 1947 weights, revised as of November 1958.

pose of measuring industrial production in wartime, and the materials included in it were presumably chosen as representative of conditions in both world wars. They are undoubtedly less representative of the later period than of the earlier one, and even less representative of conditions today.[3] The revolution that Greenslade and Wallace postulate for American industry may be occurring in the structure of materials used in industry rather than in the relation between materials and intensity of fabrication.

[3] The FRB has been reviewing the production record during the years between 1939 and 1947, and improved indexes of industrial materials and total production will probably be available at a later date, providing a firmer basis for assessing presently existing indexes.

On this matter, the fact that the Moore index of industrial materials for the United States and mine for the Soviet Union have similar product coverage is not an obvious blessing. The pertinent question is whether the products in each case are a satisfactory sample of industrial materials produced during the period of growth and in the country under review. The materials included in my Soviet index account for almost all Soviet materials on which there have been published output data for as late as 1955. The same cannot be said for coverage of U.S. materials in the Moore index. This is to say nothing of the relative exaggeration of growth by basic Soviet data. Greenslade and Wallace affirm such an exaggeration in the later part of their paper [see above, p. 694], but they do not go on to point out the inescapable implication that the Soviet index of industrial materials is biased upward on this score.

It would seem that the findings of Greenslade and Wallace should be treated with caution, pending further study. A full explanation of the differences between their and the FRB indexes would require a careful comparison of output data, weights, product coverage, and so on—a task I cannot undertake here.

I turn now to consider the argument that a comprehensive index of industrial production for the Soviet Union must have a downward bias relative to a similar index for the United States. Such a bias might result from any of four major factors that may reinforce or offset each other: (1) differences in index-number formulas, (2) differences in courses of growth, (3) differences in reliability of underlying data, and (4) differences in product coverage. The first is not in dispute at the moment, and the second is too complex to treat here, though it is important (on it, see [3]). Hence I limit my comments here to the last two factors, while emphasizing that conclusions about net bias can be reached only by considering the probable effects of all factors, and that all conclusions are ultimately matters of judgment, not easily refuted or supported by concrete factual evidence.

As to the reliability of underlying data, Greenslade and Wallace suspect that the Soviet sample of published data tends to underrepresent slowly growing sectors relative to the U.S. sample [see above, p. 694]. I agree, and am inclined to go further and say that Soviet data tend to exaggerate growth in the represented sectors, again relatively and at least with respect to an early base year like 1913 or 1928. In any event, Greenslade and Wallace believe that the upward bias of Soviet data, however strong it may be, is more than offset for years since 1937 by a downward bias traceable to exclusion of ordnance and some other important new machinery and equipment from the comprehensive index for the Soviet Union. That the one bias overweighs the other is only a presumption, unsupported by evidence. One can equally well presume the opposite and not be refuted by facts at our disposal. It would, of course, be desirable to find some way to represent ordnance and the newest types of machinery in the Soviet index, if only to see how much difference they make. But what to do when there are no data?

One thing that can be done, as suggested by Greenslade and Wallace, is to remove such items from the U.S. index and see what happens there. It needs to be stressed that they are already absent from the U.S. index that I

have used through 1939: the only producer durables covered are various items of transportation equipment.[3] Coverage of machinery and ordnance is much larger for later years, although many items are represented by adjusted data on man-hours input.[4]

We may next turn back to Table 1 and observe the effect of eliminating ordnance from the preliminary new FRB index covering 1947-1957. Ordnance includes military aircraft and other major defense equipment, but not the various items of business-type equipment purchased for military uses. Removing ordnance makes growth fall from 54 per cent to 50 per cent in the

TABLE 2—INDEXES OF INDUSTRIAL MATERIALS AND ALL CIVILIAN PRODUCTS: SOVIET UNION, 1937–1955

| | Index, 1937=100 | | Ratio (1)/(2) |
|---|---|---|---|
| | Industrial Materials (1) | All Civilian Products (2) | (3) |
| 1937 | 100 | 100 | 1.00 |
| 1938 | 103 | 103 | 1.00 |
| 1939 | 106 | 105 | 1.01 |
| 1940 | 110 | 103 | 1.07 |
| 1945 | 67 | 46 | 1.20 |
| 1946 | 76 | 60 | 1.27 |
| 1947 | 91 | 78 | 1.17 |
| 1948 | 108 | 102 | 1.06 |
| 1949 | 129 | 127 | 1.02 |
| 1950 | 142 | 149 | .96 |
| 1951 | 160 | 160 | 1.00 |
| 1952 | 170 | 165 | 1.03 |
| 1953 | 184 | 178 | 1.04 |
| 1954 | 200 | 198 | 1.01 |
| 1955 | 219 | 216 | 1.01 |

*Source:* Unpublished manuscript; for benchmark years, see [2, 402], slightly revised. Index for all products excludes "other machinery." For 1937–1940, geometric average of indexes with 1928 and 1955 weights; for 1940–1955, index with 1955 weights.

index including gas and electric utilities, and from 50 per cent to 46 per cent in the index excluding them. The reductions are modest despite the fact that ordnance production grew by 437 per cent over this period. It should also be

[3] My comprehensive index through 1939 is based on Fabricant's index for manufacturing. The FRB index, revised as of 1940, covers more items of machinery over the period 1923-1939. But the FRB and Fabricant indexes for total manufacturing move in general conformity over that period, the former showing a somewhat slower growth over 1923-1925 and 1937-1939, and hence over the entire period 1923-1939.

[4] For years after 1939, my index is based on the FRB index of industrial production, as revised in 1953. In the combined machinery, equipment, and ordnance sector, series represented partially or fully by man-hours data account for about half the 1947-49 weight given to that sector in the FRB index [4, pp. 1281-84].

observed that even the new FRB index excluding ordnance rises faster than the one used in my article, as extended by Greenslade and Wallace; the former rises by 50 per cent and the latter by 43 per cent.

Finally, it may be pointed out that the FRB index of industrial materials would seem to exclude all the items Greenslade and Wallace wish to exclude. To repeat, that index rises by the same percentage over 1947-1957 as total production including ordnance.

So much for evidence for the United States, the only evidence we have. Can we say anything at all about the adequacy of indexes of Soviet production, as far as reflecting growth in ordnance is concerned? The comparison in Table 2 may be of some interest. We note that industrial materials grew slightly faster over 1937-1955 than "all civilian products"; much faster during the mobilization and second world war periods (1937-1946); much slower over the succeeding demobilization (1946-1950); faster again during the Korean war (1950-1953); and finally slower in the post-Stalin period (1953-1955). These movements suggest that ordnance production is at least partly reflected in the divergences between the two indexes. It is impossible to determine how adequately it is reflected.

At the beginning of this reply, I promised to leave many questions unanswered. This promise has been fulfilled. But the mere posing of such questions helps us in our search for the best possible analysis of problems at hand. In the course of grappling with their questions, I have perhaps raised others that may be equally helpful to Greenslade and Wallace in their work.

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# BOOK REVIEWS

## General Economics; Methodology

*Fact and Theory in Economics: The Testament of an Institutionalist.* By MORRIS A. COPELAND. Ithaca: Cornell University Press, 1958. Pp.xviii, 347. $6.00.

It is a tribute to Professor Copeland that a collection of his papers going back as far as 1924 testifies both to his consistency in the thoroughly professional practice of as scientific an economics as possible and to his capacities for innovation in substance and specific methods. If strength and growth over four decades can generally be so convincingly displayed in the profession as in this one economist's testament, the intellectual history of twentieth-century economics will be indeed impressive.

It may some day come to be widely agreed that in the decades covered by these papers a combination of Keynesian conceptual innovations on the one hand, and rapid developments in social accounting on the other, accomplished a break-through in social science important even beyond economics. For in the interplay between concept formation in aggregative theory and the empirical measurement of observable variables related to national income, there developed for the first time in social science a relatively elaborate, elegant and relevant theoretical system whose hypotheses were so stated as to become capable to test by reference to observable phenomena. To have been instrumental in this break-through, as Copeland has been, not only through a sustained insistency that theory possess empirical import but also through particular contributions to social accounting is to have practiced social science at its best.

The collection consists of papers on large questions of method, others on testing and measurement, still others on social accounting, and finally several on particular points in economic theory. What is it that runs through the papers that leads Copeland to identify himself as an institutionalist? He does not quite fit any one of the six different characterizations of the institutionalist proffered by as many speakers on institutionalism at the 1956 meetings of the American Economic Association, and he is less the disassociated critic than the borer from within.

To Copeland institutional economics is, I think, economics that aspires to meet the canons of scientific inquiry and is not, as was the economics of the 1920's and to a lesser degree some of the economics of the 1950's, prescientific. Speaking very roughly, in institutional economics practitioners are self-conscious and explicit about their methods, they do not naïvely mix fact and value, their concepts are operationally defined, their theory has empirical import, and they are uneasy with a theoretical system, however elegant, whose theorems are not routinely tested through empirical observation.

Although institutionalists are usually seen as dissenters, Copeland's insti-

tutionalism can be seen as a protest against the profession's dissent, through much of the 19th and part of the 20th century, from the emerging principles governing scientific inquiry in all fields. It is, I think, because the profession's dissent has been crumbling in recent years, that Copeland finds that "institutionalists have been very silent" since about 1935. In the large, Copeland's institutionalism has become everyone's economics; we are all institutionalists now.

Some of the specific contributions of these essays are by now so widely accepted as to have dropped out of current discussion—water over the dam. But what a dam! The build-up of sustained runover was possible only through the confluence of a number of streams of thought, institutionalist and other, each itself fed by tributaries and watersheds too numerous and obscure to mention, together with a few great storms, including the Keynesian deluge. What has gone over the dam is probably even more interesting to the intellectual historian than what has not.

Whether I make much or little of Copeland's institutionalist label, I am impressed by his sophisticated and consistent practice of economics as an aspiring science. An institutionalist if you wish; a wise and skilled economist in any case. A real pro.

CHARLES E. LINDBLOM

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*Ordo, Jahrbuch für die Ordnung von Wirtschaft und Gesellschaft,* Vol. 10. Edited by FRANZ BÖHM, FRIEDRICH A. LUTZ, and FRITZ W. MEYER. Düsseldorf and Munich: Helmut Küpper, vormals Georg Bondi, 1958. Pp. xxiv, 537. DM 41, 80.

This is Volume 10 in a series of yearbooks founded by Walter Eucken and Franz Böhm in Germany after the second world war. The word *Jahrbuch* has frequently been used simply to denote a periodical, the separate issues of which taken together form the yearbook (like Schmoller's or Conrad's). *Ordo,* however, is a publication which appears as a full volume once a year; it displays greater unity of purpose and approach than one would ordinarily expect in a periodical. As the subtitle states, the main concern of the publication is with the economic and social order; actually discussion of the economic order is predominant.

In Germany, several groups (academic as well as nonacademic) have been concerned with the exposition and defense of neoliberal views on economic organization; since the defeat of national socialism, these ideas have made a surprisingly strong appeal. The descriptive term now commonly used for this economic order is *Soziale Marktwirtschaft* (social market economy). The academic foundations for these views in Germany have been laid by Eucken and Böhm. The influence of other neoliberal thought is also quite obvious; W. Röpke, Alexander Rüstow and F. A. Hayek are members of the editorial board of *Ordo* and frequent contributors to the volumes. The group around *Ordo* thus overlaps with that neoliberal group which has its focus in the meetings of the Mont Pélèrin Society, to which many of the contributors to *Ordo* belong.

A considerable degree of similarity of *Weltanschauung* is characteristic of the yearbook. This impression will be strenghthened by reading through the reprinted tables of content for the whole series (pp. ix-xxv) and by looking at the Foreword to the series (Vol. 1, 1948, p. vii). There it is stated that the problem which after the collapse of national socialism had to be faced in all countries but in particular in Germany is the development of an economic order that preserves human dignity and is economically efficient. This order is a competitive order which is claimed to be equally far away from central planning as it is from *laissez faire*. The state ought to set up and conserve the competitive economic order but the state must keep free from interfering with the day-to-day activities of business. This is called a *Wettbewerbswirtschaft* (Vol. 1, p. ix). The term social market economy has now become quite widely used to describe these views which to a large extent coincide with the political views held by Erhard. Emphasis on competition as the central motivating force of the order is pronounced, though this emphasis is by no means uncritical. Brief descriptions which present the expressions of these general ideas in the different papers will be given here.

The present yearbook in the first place focuses on the acute problems of freedom and competition in the process of integration of the (Western) European Economy. Five of the fourteen major papers concern themselves with this timely subject. Röpke compares the Common Market with the Free Trade Area. He stresses that the close grouping of the six nations that form the Common Market (the European Economic Community, which has in the meantime become operative) may easily lead to bloc building. The effect of such partial integration may be an increase rather than a decrease of the disturbances in the flow of trade not only in the world at large, but also within Europe. There is too little experience available at the present to judge whether this criticism has been borne out by the facts.

Willgerodt exemplifies some difficulties of integration by showing the obstacles posed by the different tax systems to the free flow of trade. He discusses especially the German turnover taxes and the measures permitted under the EEC agreements in order to equalize tax burdens. Muthesius is concerned with Euratom and its implied policy of government activity in the atomic field. He stresses the possibilities of private development of the civilian uses of atomic energy. Lutz makes a plea for freer exchange of currencies by proposing use of the device of flexible exchange rates. Meyer treats the present problem of the German balance of payments. He believes that to require all other countries to change their internal and external economic policies is unrealistic. According to his reasoning, the German mark should have been appreciated. As this has not been done, largely for political reasons, some degree of inflation in Germany becomes unavoidable even though foreign currency holdings pile up. In fact, from the middle of 1954 to April 1958 the cost-of-living index in Germany rose from about 108 to 120 (p. 149) under seemingly very conservative economic policies.

The bulk of the remaining papers are concerned with other aspects of the economic framework. Highly specialized topics appear in Möttelis' history

of attempts toward cartel laws in Switzerland, Rasch's discussion of German corporate and tax laws in regard to self-financing of enterprise out of retained profits, Leffson's criticism of the judicial attitude toward existing German statutes on overpricing (*Preistreiberei*).

The papers of a more general nature will be of greater interest to the American reader. Ethical and religious issues here come to the fore. F. A. Hayek's essay, which leads off the volume, is entitled: "*Gleichheit, Wert und Verdienst*," roughly, "Equality, Value and Merit." As an oblique reference makes clear, the fundamental question is: To what extent, in economic matters, can distributive, and probably contributive, justice (i.e. merit) be applied to correct, so to speak, commutative justice (i.e. value) without destroying ethically desired "equality" properly interpreted. Hayek opposes as arbitrary and undesirable all but the rules of commutative justice. Hayek's expected new book on the constitution of liberty will make these ideas available to the English-reading economist. Hayek has expounded other aspects of his social philosophy in previous volumes. One may refer to his other papers in *Ordo*, e.g., on "Fundamentals of Progress" (Vol. 9), "Economic History and Policies" (Vol. 7), "Market Economy and Economic Policy" (Vol. 6).

Böhm provides a historical sketch on freedom to compete and freedom not to compete. He relates the freedom to agree not to compete to vestiges of feudalism and contrasts the two "freedoms" as fundamentally different, historically as well as philosophically.

There is also an interesting paper by Solterer in which he attempts to find a rationale behind the apparently empirical generalization of income distribution known as Pareto's Law. The point of departure is the revision of the statement of Pareto's Law by Gibrat. A hypothesis is offered by Solterer to provide a theoretically valid rational explanation of the structure of income distribution and its stability. It is an attempt to demonstrate that the so-called stable wage share and the increasing equalization of income—both observable under widely differing political and social conditions—can be derived from the Gibrat distribution of personal income. Solterer emphasizes that these aspects of social economic life constitute aspects of the basic order of the social economy.

This volume of *Ordo* (as well as preceding ones) also contains discussions which would be considered by many in this country to be theological in nature, yet which are of paramount practical importance in Western Europe. A major problem is the compatibility of modern neoliberalism with the social teachings of the Catholic Church. There have been important papers on these topics in the previous volumes, such as Villey's excellent "The Market Economy in Catholic Thinking" (Vol. 7; also reprinted in *Revue d'Economie Politique*, 1954, pp. 936), and Hensel's and von Nell Breuning's papers on the vocational order of the Papal Encyclical in its relation to competition (Vol. 2 and 3). The review by Bless (Vol. 9) of the proceedings of an important symposium on the Christian and the social market economy (ed. P. Boarman, introduction by Ludwig Erhard, Stuttgart 1955) must also be mentioned.

In this present volume the paper pertinent to this topic written by an Austrian Catholic novelist and essayist, Kühnelt-Leddihn, is entitled "Catholic

Faith and Liberal Attitude." It does not discuss economics, but rather the question to what extent a Catholic is obligated to follow authority. Kühnelt-Leddihn claims that the Catholic must make his final decisions on his own responsibility (even if in invincible error according to Catholic doctrine). Therefore, what we call the liberal attitude of personal responsibility is to him wholly compatible with fundamental Catholic theology. This problem of liberalism is discussed in the American context in B. W. Dempsey, *Functional Economy*, Ch. 7, (New York 1958).

More than 100 pages of extended reviews of selected books follow the main body of the text. A 40-page review article by Ammon on a new edition of Eucken's well-known studies in capital theory is noteworthy.

This volume of *Ordo*, as well as the preceding ones, contains articles which are not all of equal importance or of equal quality. But many papers treat of fundamental problems which seem to me to be often unduly neglected in the more technically sophisticated discussions one finds in the Anglo-American literature. There is merit in posing questions of a fundamental nature to which there are no easy answers instead of working out in ever greater detail the answers to questions which may not always be important. Beyond that, *Ordo* gives a good introduction to the thinking of a noteworthy circle of economists whose work is of analytical importance as well as of weight in policy making in certain parts of the world.

WALTER FROEHLICH

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*Wstep do ekonometrii.* (Introduction to Econometrics.) By OSKAR LANGE. Warsaw: Panstwowe Wydawnictwo Naukowe, 1958. Pp. 370. Zlotys 28.

The book under review is the first postwar text on econometrics to appear in Eastern Europe. Until recently, as stated by Lange himself, the study of econometrics in the USSR was considered to be "unnecessary and even harmful" and the subject was "politically discredited" (p. 18). Other East European countries followed suit. Publication of Professor Lange's book, after two recent Polish translations of the Western texts by Jan Tinbergen and by W. Winkler, marks a change in this negative attitude. The change seems to be due not so much to a thirst for pure knowledge as to a desire to streamline methods of economic planning and accounting. In Lange's view, "econometric methods are applicable in a socialist economy and represent an indispensable tool for efficient planning and administration" (p. 5). The author even believes that at least some programming methods can find practical application only in a socialist economy (p. 208).

"Introduction to Econometrics" is divided into three parts, each of which purports to introduce the reader to one of the three main fields of econometric research which, according to the author, are: business cycle analysis, demand analysis and programming.

The first part suffers from one major weakness: a thirty years' time-lag. The discussion is centered around the defunct "Indices of Business Condi-

tions" of W. M. Persons and then branches off into computational formulas for trends and seasonals. One is left wondering just how representative all these topics are of econometric research of the business cycle.

A more balanced treatment is found in the second part devoted to market analysis, although the presentation remains on a fairly elementary level. Starting with the law of supply and demand, the author discusses at length formulas for elasticities, statistical demand curves and methods of market forecasting. The reader is then introduced to that moribund whipping-boy, Pareto's law, which is held to be applicable to a capitalist but not to a socialist economy of the Soviet type where, it is argued, a normal or log-normal distribution fits the existing income pattern. Empirical evidence presented in support of this interesting hypothesis is not conclusive. Samples of income recipients covered by the author are not representative of the populations to which his argument refers. In the case of the USSR, only payments made to wage-earners on the piece-rate basis are considered (pp. 198-99). The data presented for Poland are more comprehensive but still exclude some major categories of income-recipients, such as farmers.

The concluding part deals with input-output models and with linear programming. Its first draft appeared in English in 1957[1] but the Polish text contains some interesting additions and elaborations. In this section, the author's skilful exposition and familiarity with the realities of economic planning add up to a stimulating and thought-provoking whole. Particularly illuminating is the discussion of the applications of input-output models in economic planning, including such problems as the relationship between the rate of accumulation, the rate of economic growth and the objective of full employment. Less convincing is the attempt to look upon input-output techniques as originating in Marx's $2 \times 2$ matrix. Nor can one agree with Lange's contention that "Leontief's analysis came into existence under the influence of Marx's theory of reproduction and of the practice of material balances in the Soviet Union *since* Leontief is well familiar with both Marx's works and Soviet economic literature" (p. 223; italics mine). This inference opens fascinating vistas for a historian of economic thought: "Who read whom?" would become an all-important question. Still, some doubts persist in the reviewer's mind. To quote only one: Leontief is familiar with Quesnay's writings too. And so was Marx.

Despite the rather uneven level of presentation one can only welcome the publication of this volume. Its essential message being the Leibnizian *"Calculemos!"*, it may strengthen the tendency which has been gaining ground among the East European economists: to think in terms of numbers rather than of slogans and to use, in case of doubt, their slide-rules rather than the local counterparts of the dictionary of familiar quotations.

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[1] "Some Observations on Input-Output Analysis," *Sankhya, The Indian Jour. Stat.*, Feb. 1957, *17*, 305-36.

*Die logischen Grundlagen der mathematischen Wirtschaftstheorie als Methodenproblem der theoretischen Ökonomik.* By GERHARD KADE. Berlin: Duncker & Humblot, 1958. Pp. 185. DM 16,60.

The present volume is a methodological dissertation. The author sets himself the task of finding out if economic theory should make use of mathematics, and if it should, why. Some may feel that the issue has been settled long ago. Not so Dr. Kade, whose approach is thoroughly philosophical. He is, of course, aware of what he calls the victory parade (*Siegeszug*) of mathematics in economic theory in recent years, but he precedes his book with a quotation from Kant saying that human reason is so fond of construction that it frequently creates the tower before the foundations. In this spirit, Kade refuses to cite the uncovering of new economic truths by mathematical inquiry as proof of the usefulness of mathematics. He does have the beginnings of a history of mathematical economic doctrine, but it is confined to the Pysiocrats, von Thünen, Cournot, Dupuit, Gossen, Jevons, Walras, and Pareto, and what interests him is not the theoretical achievements of these writers but their attempts to justify their use of mathematics. Nor does Kade try to uncover new truths himself by using mathematics. Indeed there is not a single equation or inequality in the entire book. There is one and only one place in the book in which symbolic logic is used, i.e., on page 155, but unfortunately the use is erroneous: When the symbols $yQv$ are translated back into German they read: "the supplier of butter will *supply* money," which is senseless, for he must surely supply butter and *demand* money!

If the construction of the tower is uninteresting, how might one go about digging the foundations? Why may mathematics be used in economic theory? It is tempting to say that anything which is measurable lends itself to mathematical treatment, hence economics which deals with income and outlay, production and consumption, prices and profits, lends itself to mathematical treatment. Indeed this idea is found in the history of our discipline from Petty's preface to his *Political Arithmetick* through Jevons to the Cowles Commission motto: "Science is measurement." But Kade thinks that this simple idea misses the point: Measurement is not a prerequisite for the use of mathematics; mathematical economic theory began to flourish with the marginal utility school whose members could not measure the very concept they made the key to economic theory. Kade sees mathematics not merely and not primarily as a tool for the ultimate computation of numerical solutions but rather as a tool for thinking about problems involving large numbers of variables. And many variables there are in a multi-output, multi-input, multifirm, multihousehold general equilibrium. Kade's highly philosophical book ends with the rather practical but trivial conclusion that ordinary humans cannot fully comprehend such an equilibrium in any way other than a mathematical one. In his lack of interest in the ultimate computation of numerical solutions and in his satisfaction with the equality of the number of equations with the number of variables Kade is Walrasian, or pre-Leontiefian.

Several times Kade observes that in the English-speaking world and in Scandinavia mathematical economic theory, or indeed just theory, never met

the hostility it met in Germany. Hence the Anglo-Saxons and the Scandinavians were never forced to discuss methodology; they just used mathematics because it worked, without asking why. As a Scandinavian *and* an Anglo-Saxon, the reviewer is hopelessly prejudiced in this tradition, and the present book has failed to convince him that the proof of the pudding is *not* in the eating.

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*Wirtschaftsmechanik*. By W. G. WAFFENSCHMIDT. Stuttgart: W. Kohlhammer, 1957. Pp. vii, 301. DM 23,00.

*Wirtschaftsmechanik* is an important contribution to the increasing literature dealing with the mechanics of economics. The book represents an attempt to apply the concepts and techniques of general theoretical mechanics to the relationship among "economic masses," such as commodities, labor, money, credit, and to the principles of social organization and "mechanization." The book is divided into three parts (General Mechanics, General Mechanics and Economics, and New Mechanics). The somewhat disconnected topics are richly illustrated with examples and over a hundred graphs, taken mostly from physics, biology, and medicine, as well as from higher mathematics and geometry.

As the title indicates, the book is mostly about mechanics and in general contains a higher proportion of substantive mechanics than economics. It is methodological only in the sense that it shows how mechanical thinking and models can be applied to economics, for the author is more interested in following the intellectual requirements of mechanics than those of economics.

Quite early, prominence and maximum attention are given to the models of economic flows. In what perhaps could better be labeled mathematical economics than mechanics, Waffenschmidt introduces logarithmic scales to show growth, and simple difference equations to show fluctuation. Leontief's input-output analysis is explained with the help of simple examples, and is expressed as far as possible in terms of flows.

Using devices similar to those employed by Dahlberg, the author shows how the rate of the flow is regulated by the schedules of supply and demand, isoproduct curves, marginal utility, and so on. Waffenschmidt pays considerable attention to technicalities throughout; e.g., how the values work in the Phillips model (London School of Economics).

Waffenschmidt's preoccupation with flows, although interesting and instructive, causes his economics to suffer somewhat. Fascinated by the rates of flow and by the instantaneous measure of stocks of money or real assets (as these appear in basins collecting the liquid flowing through the system), Waffenschmidt shows a tendency to forget period analysis. He does not make allowances for all the consequences of a change in the rate of flow. This omission enables him to call the Keynesian $I = S$ equation a superficial statement, because it simply tells us that the same flow measured on our instruments gives the same reading.

Waffenschmidt's misunderstanding of period analysis, even the Keynesian timeless period, is also reflected in his interpretation of the *ex ante* and *ex post* concepts. For the author these concepts are not concerned with planned and realized values of variables, but with actual flows at the beginning of the period: the initial sum borrowed by the entrepreneur to employ factors of production—*ex ante;* and flows at the end of the period of production when calculating realized profits, savings, or losses—*ex post.*

It is also somewhat difficult to determine when Waffenschmidt deals with money and when with real assets. Thus he thinks it is possible for savings to accumulate over and above the outflow for investment, calling this phenomenon an effective accumulation for later investments. But it is not clear whether in this case only money and claims are put aside, after being withdrawn from current use in economic activity. In this case, of course, there would be an accumulation of claims only; i.e., paper claims not backed by any production and build-up of assets, accumulation of ownership claims. Perhaps he has in mind the accumulation of investment goods, a process which would mean that every dollar saved would be matched by a corresponding dollar's worth of investment in inventories, thereby assuring the balance of saving and investment flow.

After describing many models of flows, reminding this reviewer of Gruning's book, Waffenschmidt presents his own model, which he calls *Blockschaltbild* (switch-tap-closed-loop diagram) of economics. This model shows in some detail the monetary and real flows in the economy. We have for instance separate markets for raw materials, finished products, labor, and capital goods. On this macroeconomic model are imposed microeconomic relations; e.g., a firm. This is an excellent teaching device, but its analytical and operational significance is small.

Obviously Waffenschmidt's firm is a representative one; therefore its level of production and costs depend on many varying data. But this fact does not lead us much farther, because the firm operates with one representative type of labor, one representative raw material, etc. If several firms were to be introduced, Waffenschmidt's diagram would become unmanageable, creating problems which could not be answered with the help of the apparatus presented by the author. To illustrate, Waffenschmidt shows that the amount of raw material used by the representative firm depends on the supply and demand for it; but there is no provision to show how any amount of raw material, determined by the market supply and demand curves, could be divided among new firms.

This listing of weaknesses should not obscure the virtues of Waffenschmidt's thought-provoking book, for *Wirtschaftsmechanik* familiarizes the reader with many important noninstitutional and mathematical techniques in economics. Furthermore the author very convincingly presents the views of those who stress the importance of the use of models in economics.

GEZA GROSSCHMID

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## Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

*Economic Forecasts and Policy.* By H. Theil. Contributions to Economic Analysis no. 15. Amsterdam: North-Holland Publishing Co., 1958. Pp. xxxi, 562. $13.35.

This is not an orderly book. However, it is an extremely important one and should confirm Theil's status as one of the outstanding econometricians of this decade.

The combination of topics treated and the sequence in which they are treated are at first glance major puzzles which the reader must solve before he can settle down comfortably with the book. Theil's starting point was a 1953 directive from the Director-General of Statistics and the Director of the Central Planning Bureau of the Netherlands to investigate in a systematic manner the possibilities of a new econometric model of the Netherlands. Though a model existed already and had been used for the formulation of the annual central economic plans, it was felt that major improvements were possible.

In carrying out this directive, Theil and his assistants apparently asked themselves a series of questions:

1. How good are macroeconomic forecasts from the existing Dutch model compared with those from similar models for other countries? How accurate are the Dutch forecasts in absolute terms and relative to the actual changes in the economy? These questions are answered in Chapter 3.

2. How does the accuracy of forecasts made from macroeconomic models compare with that of other types of forecasts for which data are available—specifically, survey data on the plans and expectations of West German businessmen with respect to prices, sales, and inventories at various levels in the leather and shoe industry? Can the econometrician learn something from the microforecasts of the entrepreneur that will improve his own macroforecasts? Pursuit of these questions accounts for Chapter 4.

3. Why do both econometricians and entrepreneurs tend to underestimate the extent of future changes? This phenomenon is illustrated and analyzed in Chapter 5.

4. What factors determine entrepreneurial predictions and how can they be demonstrated empirically? This compound question is treated in Chapter 6, "Analysis of Interrelationships among Expected, Planned, and Actual Prices." Some sections of this chapter deal explicitly with the "business test data" of Chapter 4 while others deal with basic problems of statistical estimation of economic relationships from time series.

5. What are some of the methodological problems involved in the process of forecasting and in the verification and appraisal of the accuracy of forecasts? These problems are treated in Chapter 2, "Elementary Aspects of the Methodology of Forecasting."

6. What are the consequences of imperfect macroeconomic forecasts in

terms of loss of welfare? How can one quantify the gains that might be derived from specific types of improvements in the accuracy of forecasts? These questions lead to a tightly woven analysis of the interrelationships between economic forecasts and policy which constitutes the core contribution of the book. These interrelationships are treated in terms of the theory of "decision-making under uncertainty," and comprise Chapters 7 and 8.

The introductory chapter tries to prepare the reader for the heterogeneity of the contents; and the concluding chapter emphasizes certain unifying themes that run through the book as a whole. As a further aid to the reader, the author has included a detailed table of contents occupying 9 pages; lists of tables and figures take up 9 pages more.

In retrospect, the major organizational difficulty seems to arise from the inclusion of the leather and shoe industry analyses *in addition to* those involving macroeconomic models. Chapters 7 and 8 are framed in terms of national economic policies rather than entrepreneurial decisions. These chapters, together with Chapters 2, 3, and parts of 6, would have made an outstanding book by themselves, leaving the "business test data" and their analysis for a separate and highly interesting monograph. As it is, the reviewer is embarrassed by abundance when he tries to give a fair impression of the book as a whole.

The professional quality of the author stands out impressively in almost every section. Theil appears to be a "complete econometrician" in the same sense that Henry Schultz was a "complete demand analyst." Theil shows power in mathematics, statistics, and economic theory; patience in the examination of data; judgment in the choice of appropriate analytical tools; resourcefulness in the development of special coefficients to summarize new types of data; statistical realism in appraising the relative magnitudes of different sources of error; and political realism concerning the ability of policy-makers to use economic information and the differing roles of policy-makers and economic advisers. He avoids the dichotomy which characterizes much econometric work—the tendency for theory to be "too theoretical" and for practice to be "too empirical." Theil's theory and practice seem to be equally sophisticated and mutually consistent. His work should, therefore, command the respect and attention of both theoretical and applied economists.

From my standpoint, the most important contributions in the book are contained in Chapters 7 and 8. Theil describes man in his role of "decision-making animal" as follows (pp. 380-81):

> (i) First, he should have *knowledge,* in terms of values assumed by relevant variables, *of his present state. . . .*
>
> (ii) He should divide these variables into two groups: those which he controls, and those which he cannot control, but which he can at most influence indirectly and perhaps stochastically. . . . For purposes of analysis . . . we shall assume that certain variables, to be called *instruments* or *controlled variables* in contradistinction to the other, *noncontrolled variables,* exist which are fully controlled by the policy-maker. Changes in controlled variables will be called *measures* taken by the policy-maker.

> (iii) He should make a *prediction* of the future course of the noncontrolled variables under alternative assumptions concerning his own (present and future) behaviour; i.e., he should formulate conditional expectations about the time pattern of noncontrolled variables, the conditions being alternative measures to be taken by himself in the present and in the future. . . .
>
> (iv) He should *evaluate* the various outcomes of the predictions sub (iii); i.e., he should make an appraisal of the relative merits of the results of alternative measures, this result for each set of measures consisting of (a) the changes in the controlled variables and (b) the accompanying changes in the noncontrolled variables. The second part (b) implies in general an element of uncertainty. . . . This appraisal is usually based on the ordering of all conceivable individual outcomes according to a preference scale. . . .
>
> (v) Finally, he should make a *choice* among all alternatives available to him (i.e., he should choose a certain set of measures) such that the result according to the appraisals sub (iv) is "good" or even "best." This choice is a *decision.*
>
> The formulation of a decision by means of the acquisition of knowledge of the present state in terms of instrument values and those of noncontrolled variables, of prediction and of evaluation—this formulation will be called the policy-maker's 'policy.'

In Section 7.1.8 Theil distinguishes his approach from that of Jan Tinbergen and from certain ideas of H. A. Simon and others. Tinbergen, he says, fixes a priori certain desirable "target values" for the noncontrolled variables (which he calls "target variables") and then tries to find those values of the "instrument variables" which are necessary (as evidenced by an econometric model) in order to reach the targets. Tinbergen does not explicitly concern himself with the policy-maker's rationale for choosing particular target values nor with what, if anything, would be maximized by their attainment.

H. A. Simon puts forward the concept of an "aspiration level" with which the policy-maker is satisfied even though it may fall considerably short of the highest utility level that is attainable. Theil comments that this idea is sometimes attractive for a description of actual behavior and might even be appropriate for "rational" behavior under certain circumstances. However, "It is important to make a distinction between the reduction of attainable welfare due to imperfect forecasts and the reduction of actual welfare caused by the fact that the policy-maker is satisfied with less than the attainable maximum." Theil's loss-of-welfare concept is the same as "loss" in statistical decision theory, and Theil notes his indebtedness to Von Neumann and Morganstern's *Theory of Games,* to A. Wald's *Statistical Decision Functions,* and to L. J. Savage's *The Foundation of Statistics.*

Theil's *novum organum* for the analysis of economic policy is based upon a very close formal relationship with the classical theory of consumer's demand. He assumes that the policy-maker's preferences are given. The policy-maker's criterion for the evaluation of alternative policies corresponds to the utility function of consumer's theory, and the econometric model which is

used for prediction purposes corresponds to the budget constraint in the theory of consumer demand. A complication arises from the fact that, while the consumer is assumed to know his budget constraint perfectly, the policy-maker's knowledge of the accuracy of his prediction model is always imperfect, and this, says Theil, "is the cornerstone in the relation between prediction and decision-making." On the special assumption that the policy-maker's evaluation criterion is a quadratic function, Theil shows that the celebrated distinction between income and substitution effects is equally well applicable in the policy-maker's case. The titles of major sections in Chapters 7 and 8 suggest the general drift of Theil's argument: 7.1, Predictions and Preferences; 7.2, Choice Subject to a Linear Constraint: A Re-Statement of Consumer's Demand Theory; 7.3, Choice in Risk-Taking Situations; 8.1, Assumptions on Constraints and Preferences; 8.2, Certainty Equivalence and Certainty Bias; 8.3, Optimal Decisions and Changing Constraints; 8.4 and 8.5, The Loss of Welfare Due to Imperfect Predictions; and 8.6, A Dynamic Generalization.

Theil gives us, then, the foundations for a rational appraisal of the welfare consequences of alternative economic policies. This is an outstanding contribution. At the same time, he leaves important gaps which others may attempt to fill—most important, he does not consider the relation between the "given preferences" of the policy-maker and those of the electorate, and/or of the consumers and entrepreneurs who are affected by the policy-maker's decisions. He comments also that "the empirical determination of the behavior patterns of Government policy-makers is an almost untouched field."

I believe that every economist who professes an interest in economic policy should read at least Chapters 1 and 9 of Theil's book, and its table of contents, to get some idea of what the book is all about and the variety of theoretical and empirical resources upon which it draws. Teachers of courses in economic policy should by all means read Chapter 7 and those who hope to make significant contributions to the quantitative analysis of economic policies should study Chapter 8. (Chapter 8 makes liberal, though not wanton, use of matrices and calculus). After Chapter 8, Chapter 2 could be recommended as relatively light reading on the methodology of economic forecasting. The empirical chapters 3 through 6 will be of value to economists interested respectively in the gross success of macroeconomic forecasts and the analysis of businessmen's expectations.

In the short space of a review, many important points must be passed over. Section 6.2 contains a highly condensed treatment of the relative merits of least squares and maximum likelihood methods of estimating economic relationships from time series. Those who have followed the arguments over "simultaneous equation" versus "single equation" or least-squares estimation will be interested in Theil's conclusions:

> When reviewing all arguments, we should conclude that the method of least-squares can no longer claim to have the brilliant properties which earlier econometricians thought it had. But, as far as experimental estimation in small samples is concerned, the method may be regarded

> as one of the few one-eyed men who is eligible for king in the country of the blind. Since the validity and the sharpness criteria do not give a clear answer to the question which method is to be chosen, the simplicity criterion should convince us to use least-squares. . . . (p. 240.) The main conclusion from this analysis is not that least-squares estimation is perfect; but rather that it is difficult to defeat this method. (p. 499.)

A number of implications may be drawn from Theil's book (though the reviewer, rather than the author, must accept full responsibility for drawing them):

1. It is now possible to analyze sets of economic policies on a sophisticated level in terms of economic theory, statistical decision theory, and the theory of statistical estimation of economic relationships. The data network available for use in macroeconomic models is constantly being improved and extended.

2. Full comprehension of the economic and statistical bases for the appraisal of macroeconomic policies requires advanced training in economic theory and statistics, along with calculus and matrix algebra. These tools are needed for the *derivation* of principles and methods of policy analysis; however, it should be possible to convey to policy-makers and other intelligent laymen a practical understanding of the concepts involved and some quantitative awareness of the relationship between forecasting errors and welfare losses in real situations.

3. Forecasts from the Dutch and the Scandinavian econometric models suggest that knowledge of the structures of these economies is reasonably sound in the sense that the coefficients of the models do not lead to seriously biased forecasts of dependent or "endogenous" variables once the actual values of independent or "predetermined" variables are known. It should be possible to achieve similar or better results for the United States and perhaps other economies. I have noted in another place my belief that much more detailed, more flexible, and more useful models can be devised for the United States economy than have thus far been put forward.

4. Economists can no longer afford to be satisfied with forecasting on the basis of mere description of the time paths of multitudes of individual series. While economists who work for many years in the "reference cycle" framework may become good catch-as-catch-can forecasters, there is no sound reason for ignoring the existence of structural relationships among industries and of behavior equations reflecting the decisions of particular classes of economic agents. We can now aspire not only to be reasonably accurate in our forecasts but to be accurate for economically intelligible reasons.

5. In particular, there is need (in the United States) for one or more macroeconomic models developed from the perspectives of the Council of Economic Advisers, the Bureau of the Budget, and the Federal Reserve System—a model, or set of models, that would ultimately provide a means for anticipating the effects on the economy of almost any set of policy measures, given particular initial values and rates of change of the relevant "noncontrolled" variables.

6. There is need for clarification of the economic goals of policy-makers of cabinet rank and in the larger bureaus and independent agencies. Theil (p. 415) shows hypothetical diagrams of the indifference curves for profits and wages of (1) conservative and (2) "left-wing" policy-makers. Though the diagrams are only illustrative, they suggest that the compatibility, if not the ultimate rationality, of the preference structures of different policy officials might be ascertained. Quantitative statements of the economic consequences of their different preference structures might form a basis for constructive modification of conflicting policies.

7. Much greater sophistication is possible, and is needed, in appraising the real effectiveness of policy-makers as distinct from their good or ill fortune in taking office at a particular stage of the business cycle. For example, says Theil (p. 555), "There is a widespread tendency among policy-makers 'to do something' (i.e., to revise instrument values) when things become worse, but 'to do nothing' (i.e., to keep instrument values constant) when things become better. Naturally, when a policy-maker's constraints change such that the attainable utility level increases or decreases, then, in both cases, the optimal instrument values will change in general."

A reduction of the utility level *attainable* owing to circumstances beyond the policy-maker's control should not, of course, be charged against him; however, a reduction of welfare below the maximum actually attainable under the given "outside" circumstances could be ascribed to the policy-maker's imperfections. This distinction is not often adequately made in practice; the best players may sometimes draw the worst hands.

8. One might even consider measuring the relative welfare content of the economic planks of two political platforms and the natures of the two sets of indifference functions for which the opposing platforms represented optimizing behavior.

It may be naïve to suggest that the major economic decisions of the 1960's will (in the United States) be greatly modified by econometric analyses of the consequences of alternative economic policies. But we should learn a lot of economics through such attempts and perhaps raise the level of economic debate in national and state legislatures, in the public press—and in the professional journals. Thiel's book will provide much ammunition and encouragement for those who believe that econometric analysis is essential to the rational formulation of economic policy.

KARL A. FOX

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*Allocation in Space.* By LOUIS LEFEBER. Amsterdam: North-Holland Publishing Co., 1958. Pp. xv, 151. $4.50.

It has long been recognized that much of economic theory needs restating and perhaps reworking if it is to allow explicitly for the spatial distribution of resources, production, and consumption. Recent years have seen large steps in this direction, notably the books of E. M. Hoover and Walter Isard,

and the appearance in translation (and consequent rediscovery) of the remarkable work of August Lösch.

Lefeber's study is a further step, a necessary and major one. His goal is a "general equilibrium analysis of production and choice of industrial location." Completion of the general-equilibrium framework (the generalization to many dimensions, the addition of final-demand equations, and the counting of equations and unknowns) is wisely left to an appendix. The main task of the text itself is the derivation of a transformation function or efficiency frontier with respect to final goods in the simplest interesting case. Isard and Lösch dealt with a continuous plane where transport is feasible between any two points. Lefeber's context is the more realistic and analytically fruitful one of discrete sites, finite in number, at which production can take place and between which transportation can be provided at a (variable) cost in final goods foregone.

Part I considers two sites, each producing a different output but using the same two inputs, of which each site has a fixed endowment. The inputs may flow between the sites. Production of each output is constrained by a smooth production function whose isoquants are convex, as is production of the commodity "transportation," which requires the same two inputs. A final smooth function relates total transportation needed to the amounts of each input shipped in each direction. (Lefeber forgets to specify that the production functions must permit disposal, i.e., if the point $[\varphi(x_1, x_2), x_1, x_2]$ is feasible, where $x_1$, $x_2$ are inputs and $\varphi$ is the output, then $[\varphi(x_1 + \Delta, x_2), x_1 + \Delta, x_2]$ is also feasible for any positive $\Delta$.) The transformation function for the two outputs, given the input endowments at each site, is first derived in a geometric fashion. Cross-hauling is excluded and four basic patterns of input flow are distinguished, each giving rise to a segment on a first approximation to the transformation curve. Joining the segments, a curve with humps and cusps is obtained. They are bridged over, and the final smooth curve obtained, by considering the cases "in between" the basic four. The geometric derivation (which is numerically illustrated) is ingenious and has intuitive value but would be hopelessly unwieldy in more dimensions.

Accordingly, Lefeber proceeds to a "neoclassical approach" in which the no-cross-hauling condition is still imposed. The amount of the second output is to be maximized subject to production of a given amount of the first, subject to the constraints—equalities—imposed by the production functions and input endowments (which are exactly "used up," a constraint that makes sense only under the disposability condition noted above), and subject to the condition that transportation produced equals transportation required. Differentiating a Lagrangean expression for each of the four basic cases of the geometric analysis yields maximum conditions which are given a rather complex "common-sense interpretation" in terms of the consequences of a marginal shift of one of the inputs into production of the first output.

The derivation is still cumbersome and Lefeber arrives finally at a linear-programming formulation of the problem. Fixed coefficients characterize production, and the required transformation curve is to be traced out by maxi-

mizing a linear function of the outputs subject to *inequality* constraints and by varying the weights. Inefficient cross-hauling is automatically excluded by the maximizing process. The superiority of this approach over the previous two is amply demonstrated. Aside from its practicability in many dimensions its interest lies in the interpretation of the solution to the minimization problem which is dual to each maximization. Necessary and sufficient for a maximum is that there exist for each input at each location shadow prices or rents (the solution of the dual problem) satisfying certain intuitively meaningful conditions (e.g., rents of the same input in the two locations must differ by the input's transport cost, an input not used up in a location has zero rent there). A nonlinear programming formulation (where a smooth production function with decreasing returns to scale replaces fixed coefficients) yields the same rent conditions.

Part II adds consumption locations, each consuming the two outputs of Part I. There is a "welfare maximization" programming approach (with linear welfare function) and additional shadow-price conditions are derived. Part III further generalizes the programming problem so as to allow for production of each output at each location and discusses the extensions needed to treat immobile factors (which may be mobile in the long run).

While Lefeber skillfully compares alternative analytic frameworks and erects the preferred programming framework, he obtains from it no new theorems or major "results" (there is the tantalizing remark that "agglomeration effects" can be demonstrated even in the absence of increasing returns, but this is nowhere done). The programming formulation suggests fruitful theoretical questions (e.g., the sensitivity of technology—the production processes chosen—to location). These questions, and the rigorous investigation of the existence and stability of locational equilibrium, should beckon much more clearly as the result of Lefeber's work.

THOMAS A. MARSCHAK

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*L'hégémonie du consommateur: vers une rénovation de la science économique. Les théories actuelles de la production et de la répartition sont fausses.* By BERNARD LAVERGNE. Paris: Presses Universitaires de France, 1958. Pp. 362. 900 fr.

In some ways this is Professor Lavergne's most ambitious book. His attempt at *rénovation* has two main purposes. One is to change economics for the better, primarily by integration of his consumer philosophy on almost every level of theory. The other is to put economics back on the French track and away from the Ricardo and Keynes tradition. While many of his criticisms of current theory are telling, the premises upon which he bases his reconstruction are open to dispute among reasonable men, and his conclusions are so colored by his social views, especially about cooperatives, that the work turns out to be more provocative and stimulating than successful. But perhaps this should not interest us so much as the attempt itself. Only

a wise and skillful economist could make an essay on such a scale. Certainly no similar attempt has yet been made in English. And I suspect that those best grounded in consumer and welfare economics would feel that our knowledge of these matters is not of a kind that might provide the basis for major theoretical reconstructions. In this connection, it is striking that the book does not contain a single reference to important writings or writers who have recently been working in this field. There is no mention of George Katona, James Duesenberry, Richard Stone, M. G. Reid, or K. J. Arrow, to say nothing of the text-writers like L. J. Gordon or W. W. Cochrane and C. S. Bell. In fact, Lavergne seems to be repaying the relative neglect English-speaking authors have visited upon continental writers. It is strange to find economists in the same field virtually ignoring each other. Their loss is mutual.

The first and third parts of the book center on important political and social questions and how they might be resolved by a formal recognition of the role of the consumer. The middle and longest part is more specifically economic. Thus the book has two main foci. The one is rather institutional, pleading and reformist. The other is more theoretical, more critical, and more narrowly economic.

The 17-page preface is a statement of the intellectual development which has led Lavergne to advocate *le socialisme coopératif*. For the rest of the first part, 66 pages, he discusses such problems as universal suffrage, personal freedom, and state power. His approach to these problems is guided by the idea that some sort of explicit political representation should be given to the interests of people as consumers. The second chapter, after a denunciation of political evils, contains a plan to do this for France.

In the third chapter, he builds his discussion of freedom on the idea that science, almost by definition, denies liberty, demanding, as it does, discipline and being primarily concerned with material production. Modern states also are concerned with material production, use science, and thus restrain freedom. But the consumer, in his consumption choices, follows not scientific, but moral, spiritual, and other values—and is thus almost automatically opposed to the state. Cooperative socialism can ameliorate this conflict about freedom.

Just how this is to be done is set forth in the last part, 35 pages, which deals with the problems of justice in the distributive process, efficiency in production, and respect for individual choice. He proposes a twofold development of cooperatives. One kind should be of the consumer type, a modern development of the Rochdale ideas. But because there are certain things that seem to be done best, or only, by public or semipublic organizations, he proposes the development of *régies coopératives*. Organization of these will mainly take the form of legal persons of a public nature. They could only be undertaken by public decision, but would be completely separated from the usual political power and budget structures of the state. They would be owned by the users of the service and would aim at low prices and a low profit. He cites examples of this kind of organization now in use, in Belgium, England, France, Australia, and Canada. These two parts alone make the

book required reading for those deeply interested in the cooperative movement.

The longest part of the book, 263 pages, is the second part. Its aim is to show how wrong most of our value, price and allocation theory is, and then to indicate how a proper understanding of the consumer will allow us both to select correctly from the present theory and to build a new one, more modest and more realistic. Essentially, the critical parts are based on the fact that most of the currently received doctrine does not reflect economic reality. His suggestions for rebuilding follow from the psychological bases of the theory of value set forth by Jevons and Menger, as modified by the French "economic reality" school, exemplified by such men as Aftalion. If we begin like this, we can give to the consumer the central role he deserves in our theory. The criticism is better than the reconstruction. The value of the criticism stems from his viewpoint. The weakness of the reconstruction stems from the relatively restricted range of data and concepts about the consumer that Lavergne employs.

Professor, now *honoraire*, Lavergne is the author of more than a dozen books. As early as 1910 he made the important suggestion that profit may be the reward for *l'idée créatrice* of the entrepreneur. He has developed a theory of the psychological causes of the business cycle. He recognized the importance of the consumer before the present revival of interest. Even while teaching at the University of Paris, in addition to his work as an economist he was a strong advocate of the cooperative movement. He is now editor of *L'année Politique et Économique* with which the journal he formerly edited, *Revue des Études Coopératives*, merged.

This ardent work attempts to draw together his more theoretical economic contributions with a practical plan of cooperation leading to a solution of some of our economic problems.

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*Reddito nazionale e politica economica.* (National Income and Economic Policy.) By GIORGIO FUA'. Turin: Edizioni Scientifiche Einaudi, 1957. Pp. xi, 271.L.2,500.

Italian economists have long been aware of the significance of welfare economics and of national product analysis, but it was only after the war that, in line with recent developments in English-speaking countries, those twin areas of study were brought together under a common focus. Since that time there has been a steady output of work, some of it of very high caliber, dealing with the measurement of national product and applications to welfare economics.

Giorgio Fua's lucidly written and attractively printed book, prefaced with an introduction by Gustavo Del Vecchio, makes a significant contribution to that literature. It is divided into two roughly equal parts. The first, based on classroom lectures, deals with the theoretical foundations of national product analysis. The second includes a number of essays on applied problems of

national accounting and welfare economics, some of which had already appeared in Italian scientific periodicals.

There is no pretence on the part of the author to break new ground on fundamentals. The theoretical presentation, in its essence, is an able restatement, with revisions and refinements, of the Pigovian approach, which, in Fua's judgment, still excels, for depth and realism, more recent and fashionable treatments of the subject. Consequently it covers, in orderly steps, the concept of national product and its meaning, pricing also in relation to public services, amortization and capital gains and losses, interspatial and temporal comparisons (a topic on which the author has significant views of his own) and index numbers. There are also extensive references, followed by a critique, to J. R. Hicks' recent book on the evaluation of social income. Great stress is laid upon the consideration that different methods of measurement of the national product imply alternative assumptions as to social optima and related policy objectives.

The selection, organization and treatment of the main topics bear the mark of a high degree of competence. Fua's preoccupation—almost to the exclusion of anything else—with the "meaning" of the aggregates measured by national accounting procedures may perhaps strike some readers as excessive. Few would quarrel however with his caveat to the effect that national product analysis may become misleading, or meaningless, as a guide to policy, outside of the frame of reference set by its explicit and implied assumptions.

The essays on applied welfare economics are lively and eminently readable. Some deal with the role of fiscal stabilizers in the business cycle and the finance of rearmament in the light of European experiences during the Korean war. There are a few pages on Keynes, and a thoughtful little essay on the social philosophy of Luigi Einaudi. Other essays discuss the so-called Vanoni program for the development of the Italian economy, and social security plans, inflation and full employment.

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*La répartition du revenu national. Part I, Les participants. Vol. 1, Les salariés. Vol. 2, Les non-salariés.* By JEAN MARCHAL and J. LECAILLON. Paris: Ed. Génin, 1958. Pp. 667; 338. 3600 fr.; 2400 fr.

These two volumes are the first part of a longer work on the distribution of income. They deal with the question of how many types of income recipients should be distinguished in a study of the subject, and analyze their characteristics. The second part will set forth how the interaction of these groups determines the distribution of income. It is impossible to give an adequate evaluation of the thought of the authors on the basis of this first installment of their work: A drama cannot be judged from a description of the *dramatis personae.*

The authors maintain that it is a major flaw of existing theories of income distribution that they are not based upon a sufficiently realistic description of

the groups that share the economic pie in an advanced capitalist economy. This point is well taken. It is supported by a lively but not too profound summary of existing theories of income distribution.

In their own theory, the authors propose to distinguish five major groups: employees, profit recipients, agricultural producers, lenders, and recipients of transfer incomes. The following table, which appears in the concluding section of the second volume, shows these groups as well as the subgroups which the authors wish to keep in mind for the study of the distribution of incomes:

| Groupes de participants determinés par les procédés d'insertion dans l'économie | Catégories de participants a la répartition dotés d'un comportement homogène |
|---|---|
| I. Salariés | 1. Ouvriers (travailleurs manuels) |
| | 2. Salariés non manuels (employés et fonctionnaires) |
| | 3. Cadres des secteurs privé et nationalisé |
| II. Titulaires de profits | 4. Entrepreneurs individuels de l'industrie et du commerce |
| | 5. Sociétés privées et nationalisées |
| | 6. Administrateurs de sociétés |
| | 7. Membres des professions libérales |
| III. Exploitants agricoles | 8. Exploitants agricoles |
| IV. Prêteurs | 9. Prêteurs |
| V. Bénéficiares de transferts | 10. Bénéficiares de transferts |

They amass a wealth of demographic, economic, sociological and other data characterizing these groups in various advanced capitalist countries. Much of the information is quite interesting, and lends an air of realism to the discussion. However, it remains to be seen whether in a more fundamental sense the basic classification scheme adopted by the authors is sufficient to provide the realistic description of the structure of modern capitalist economies which is necessary as a setting for a viable theory of income distribution.

For instance, it is not apparent that an adequate foundation has been laid for a theoretical and quantitative study of the role of the modern corporation in the distributive process. Similarly, no preparation seems to have been made for assessing the role of government in influencing the distribution of incomes through taxes.

However, as noted at the outset, these two volumes cannot really be evaluated in the absence of the sequel that is promised; the points of criticism that have just been put forth are tentative, and will perhaps appear invalid in the light of the analysis of the process of income distribution which the authors will provide later.

The book is well organized and written in a clear and fluent style.

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***Robert Torrens and the Evolution of Classical Economics.*** By LIONEL ROBBINS. New York: St. Martin's Press; London: Macmillan & Co. Ltd., 1958. Pp. xiii, 367. $7.50.

*Letters on Commercial Policy*. By R. TORRENS. Introduction by Lionel Robbins. Series of Reprints of Scarce Works on Political Economy No. 14. London: London School of Economics and Political Science, 1958. Pp. xii, 80. 18s.

The appearance of a new work on the history of economic thought by Professor Lionel Robbins is always an occasion for rejoicing among economists, particularly when the subject is a neglected though relatively important writer as was Colonel Torrens (1780-1864). Robbins concludes, (p. 258), on the basis of Torrens' writings—little first-hand evidence (including a few letters) has been discovered; not even an authentic photograph has been turned up—that "as an economist he was not in the first rank" with Ricardo, Malthus, or J. S. Mill. He ranks above McCulloch and James Mill, roughly on a level with Senior; "he played his part; . . . he is not one of the figures in the foreground, . . . he deserves a position in the middle distance." Robbins observes also, in his introduction to Torrens' *Letters on Commercial Policy* (originally published in the *Bolton Chronicle* and first reprinted as a booklet in 1833), that the latter's work reflects, more significantly than does the work of any of his contemporaries, "the main movement of economic thought" between 1800 and 1850. These letters not only furnish a readily accessible sample of Torrens' style; they also constitute the "first systematic exposition" of Torrens' view, adumbrated in 1827 and more fully developed in the 1840's, that a policy of reciprocity and a colonial *Zollverein* were preferable (in real national-income terms) to a policy of unilateral free trade.

Robbins devotes six of the seven major chapters of his work to the contributions made by Torrens: two to his contributions to central classical theory in the fields of production, trade, distribution, and value; two to his writings on money and banking; and one each to his theory of colonization and his theory of commercial policy. The last chapter recapitulates Torrens' contributions and assigns him his place in the history of economic thought. In an extremely helpful bibliographical appendix of 89 pages, Robbins lists and briefly summarizes "the contents of Torrens' books, pamphlets, and articles, which are identifiable"; 84 items fall within this category. There is included also, besides a subject index, a quite detailed select index of names. In sum, Robbins' mode of presentation might well serve as exemplar to economists doing books on a single economics author. Because Torrens' work is not well known I shall confine myself to presenting Robbins' findings.

In his exposition and analysis of Torrens' contributions Robbins is careful to point out that the classical system of thought, analysis and prescription was an evolving system, a system that had its origins in the work of Hume and Smith but thereafter was continually subject to correction, revision, explanation, and extension, particularly when new problems and policy-issues arose. Torrens played a part in the evolution of this system. In his introduction to the *Letters* Robbins groups Torrens' chief contributions under two heads: (1) those he made as a contemporary of Malthus and Ricardo, namely, his clear formulation of the theory of comparative costs in international trade, together with the improvements he introduced into the theories of value and

distribution; (2) those he made "as a senior contemporary of J. S. Mill, McCulloch, and Senior," namely, his exposition of the theories of the Currency School, his analysis of colonization and defense of self-supporting colonization, and his abandonment of the cause of unilateral free trade.

In his discussion of division of labor and international trade Torrens not only distinguished territorial from nonterritorial division of labor, but went on to formulate the law of comparative costs earlier than did Ricardo and to anticipate the notion of riciprocal demand later developed by J. S. Mill. He differed with Ricardo in respect of profit and of value and its measurement, and he emphasized the importance of factors of production other than labor. In his treatment of money, credit, and accumulation—i.e., in his theory of aggregate demand—Torrens clearly expounded the role of cash reserves in banking, the nature of the response of investment to interest-rate changes, etc. Furthermore, in a manner anticipatory of Keynes and unlike that of either Ricardo or Malthus, Torrens reasoned that saving might continue even after profitable outlets for investment no longer were available, with the result that the economic system would become slack.

Torrens' economic-policy prescriptions were largely those of the Classical School; they had as their principal objective elevation of the long-run equilibrium level of wages. His chief contribution to classical policy, Robbins states, was his theory of systematic, self-sustaining colonization, a theory inspired by Wakefield and later adopted by Mill—colonization which would relieve population pressure until prudential habits had been established and which would also provide outlets for investment and markets secure against "the hostile tariffs of foreign rivals." His chief departure from classical policy was his advocacy, whilst insisting on the advantage of international specialization, of reciprocity and a colonial *Zollverein* in place of unilateral free trade. In the realm of monetary policy Torrens, in the end an exponent of the currency principle and a critic of the Bullionists and the advocates of the banking theory, favored regulating issue in such manner that the supply of money would behave as metallic money and disequilibrium in the balance of payments would be averted.

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*Progress and Profits in British Economic Thought 1650-1850*. By G. S. L. TUCKER. New York and Cambridge: Cambridge University Press, 1958. Pp. vii, 201. $5.00.

British economists have been interested in the influence of progress on the rate of profit from the time of Sir Josiah Child (1630-1699) until that of John Maynard Keynes. Most of the writers have agreed that increasing wealth is linked up with a fall in the rate of profit. Explanations of this downtrend vary with increasing insight into the market mechanism and the change of the business situation.

G. S. L. Tucker has described the most interesting part of this development: the discussion of the falling profit rate from 1650-1850. With great industry and thoroughness he reports on the early British mercantilists (Sir

Josiah Child, Sir Dudley North, and others), on the forerunners of Adam Smith (Cantillon, Hume, Massie, and Turgot), and on Adam Smith. The presentation of Ricardo's opinion, the discussion between Ricardo and Malthus, the discovery of Lalor round up this survey. Since Marx wrote his *Theories of Surplus Value,* much of the material used by Tucker has been investigated time and again. Tucker found new source material in Sraffa's famous Ricardo edition, and in *Money and Morals: A Book for the Times* by John Lalor (London 1852).

This new evidence is worth a more detailed report. Before Ricardo, British economists had already discovered the more obvious reasons for the decline of the profit and the interest rate. Higher money income will reduce both rates when savings, not consumption, increase (p. 38). Ricardo adds new aspects to this discussion. The downward trend of the rates in the Ricardian system is a short-run tendency, Tucker claims, due to diminishing returns, the increase of capital, and of population.

An increase of capital may lead to a better bargaining position for the workers, the consequence of which is a rise of money wages. Higher money wages, given the labor theory of value, can not lead to a higher price level because no more labor is embodied in the goods produced. Profits will be reduced (p. 101).

After revising his ideas several times, Ricardo, if we accept Tucker's interpretation, came to the conclusion that the decline of the profit rate happens only in the *short* run. It cannot happen in the long run. If profits fall because of higher wages, the demand for labor will be reduced because capitalists will save by employing machinery instead of workmen. The supply of labor will be increased at the same time, because higher money wages lead to more marriages. Wages will fall and profits will rise again. Eventually a long-run trend of the profit rate will be established in the following way: capital increases at a constant rate, the supply of labor will grow at the same rate as capital accumulation, and profits are unlikely to be affected (pp. 114, 117).

Although Tucker backs up his very interesting interpretation with many quotations I still prefer John Stuart Mill's interpretation. Mill claimed that Ricardo envisages a fall of the profit rate in the *long* run (p. 110). Tucker underrates the role of falling productivity. In my opinion, even a proportional increase of capital and population must enlarge the demand for food (wheat), less productive lands will be used, money wages will rise to keep commodity wages on the same level, finally the rate of profit will fall. Malthus' summary of the Ricardian theory is meaningful only if Ricardo dealt with a long-term trend (p. 128).

In his critical interpretation of the Ricardian position, Tucker writes, Malthus makes a very subtle distinction between limitation and regulation of the rate of profit (pp. 128, 130). Ricardo, Malthus believes, shows that the productivity of industry limits the size of profit. Profit is regulated by the discrepancy between the value of input and the value of output. Capitalists do not consume their share of the output; therefore produced consumer goods become abundant, prices fall and the rate of profit shrinks.

At the same time another causal nexus works in the same direction. If the

capitalists do not consume enough, they save too much, and the rate of capital accumulation increases too rapidly. This oversupply of capital depresses profit and finally "causes distress" to the workers (p. 149). It is not low profits and high money wages that are linked together, as Ricardo assumed, but low profits, falling wages, and unemployment.

This Malthusian deduction contains a small part of the Keynesian pattern but no more. Tucker remarks: "Those who compare the ideas of Malthus and Keynes should remember that in the work of the former, the argument that accumulation might be excessive referred not only to saving but also to investment—the other half of the act of saving" (p. 135).

Not Malthus but John Lalor, who wrote after Malthus, distinguishes between saving and investment in the Keynes sense (pp. 138, 176). Lalor believed that money savings had tended "to outgrow the opportunities for profitable and safe investment." The overflowing savings will be wasted in sudden bursts of speculation. A following crisis of speculation will be aggravated by new money savings. The destruction of savings by unproductive investments leads to a decrease of the profit rate. Lalor more than Malthus foreshadows Keynesian ideas. But it seems that Lalor was not known to the author of the *General Theory*.

The discovery of Lalor and the interesting Ricardo interpretation are the highlights of Tucker's book. Tucker does not make it easy to pick out these main points. The book lacks a clear organization, and the author does not always distinguish between essentials and nonessentials. Besides the author promises more than he carries out. He intends to show "how have these theories been connected with the economic conditions and practical problems of the countries to which they refer . . ." (p. 3). Yet in the text I could find only sketchy allusions to the connection between the history of facts and the history of thought. In spite of these shortcomings this is a thought-provoking book which should be read by the historians of economic thought.

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*Thorstein Veblen: A Critical Reappraisal.* Edited by DOUGLAS F. DOWD. Ithaca: Cornell University Press, 1958. Pp. xii 328. $5.00.

The centenary of Veblen's birth fell in 1957. To mark the occasion the Department of Economics at Cornell sponsored a series of eight lectures on various aspects of Veblen's thought. These together with nine other essays commissioned for the same purpose make up the present volume. One has come to expect little from books designed to stimulate interest in some thinkers said to be neglected, but this one falls short of all decent expectations. Its total effect is soporific. It is much too long: there is hardly a piece in it that would not have benefited from being cut in half. The quality of the prose is, generally speaking, indifferently bad. The level of argument varies from pedestrian to esoteric. The focus of the symposium is uncertain: it is sometimes difficult to see what audience it is intended for.

A few informal essays by Joseph Dorfman, Walton Hamilton, Myron Wat-

kins and G. W. Zinke introduce the beginner to Veblen. Papers by C. E. Ayres, Norman Kaplan, Leslie Fishman, Philip Morrison, Carter Goodrich and Douglas Dowd suggest, with varying degrees of success, Veblen's contributions to sociology, labor economics and comparative economic history. Morris Copeland, Lawrence Nabers, Melvin Brockie and Forest Hill discuss Veblen's critique of orthodox economics and compare Veblen to Keynes and to Marx. These are grounds often trod before. Allan Gruchy and Paul Sweezy in two stimulating essays convey the sweep of Veblen's thinking on the development of capitalism. Finally, there is an excellent analysis by Joel Dirlam of Veblen's idiosyncratic views on corporation finance; this is perhaps the only essay in the book to add something new to Veblen scholarship.

It was Veblen's habit not to express himself succinctly or unambiguously, not to simplify or systematize. This makes it difficult to expound his ideas, still more difficult to evaluate them. Most of the authors in this volume concentrate on the former at the expense of the latter. Veblen's fundamental dichotomy between making goods and making money—it is really a distinction between material and subjective criteria of economic welfare—is frequently mentioned but nowhere adequately analyzed (see pp. 96-99, 170-71). Several authors applaud Veblen's call for an evolutionary science of economics: the economic system should be studied as "a cumulatively unfolding process," not as "a self-balancing mechanism." It is easier to say this than to show how it should be done. They defend him against the charge of being antitheoretical but do not pause to consider reasons for the failure of others to follow his lead. Much of what Veblen himself did was of course theoretical, in the invidious sense of consisting of too many imaginative conclusions chasing too few accurately known facts. But theory, as he understood it, seems to consist solely of realistic descriptions of observable trends in terms of the clash of institutions and ideas. He rejected neoclassical economics because he found its philosophical premises outmoded; more fundamentally, because he deplored the sacrifice of social insights to analytical rigor. There is an air of irrelevancy in all his objections; it was simply that he did not like what had passed for economics. Economics is what economists do. Veblen at any rate practiced his methodological precepts. But is it altogether accidental that his influence has come to be felt more in sociology than in economics? The least impressive of Veblen's books are precisely those in which he deals with the traditional subject-matter of economics; after *The Theory of the Leisure Class, The Theory of Business Enterprise* is a great disappointment.

The editor of this book decries the fact that Veblen's works are now read by a small and shrinking number of students. Certainly this is deplorable. Veblen's irony, not to mention his flamboyant prose, is a tonic few can afford to do without. But will this series of lectures and essays send anyone back to Veblen? I doubt it. Veblen can still provoke and inspire. Let us have more Veblen and Veblenism in the spirit of the master. But no more introductions, appreciations and endless, endless expositions of what he really meant. Veblen is too enjoyable to bury beneath a mound of annotations.

MARK BLAUG

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*Price Theory*. By W. J. L. RYAN. New York: St. Martin's Press; London: Macmillan, 1958. Pp. ix, 396. $7.50.

*Price Theory* by W. J. L. Ryan is a competent, well-organized and lucid exposition of neoclassical price theory. It relies mainly on the traditional tools of analysis, and admittedly (p. vi) strives for no special originality of content. Essentially, the study is a replay, though with marked virtuosity, of familiar themes. The first part (9 chapters) sets up the purely competitive economic model as a means of explaining the formation of relative prices. These result from the interplay between households and firms acting in analogous manners. Both take prices and adjust quantities. Both maximize: the firm seeks the greatest net revenue, while the household seeks "the fullest satisfaction of its desires" (p. 258). Whereas households possess indifference curves (or, at least, act "as if" they do) and budget lines, firms have isoproduct curves and isocost lines. As the purchase plans of households and sales plans of firms are implemented, there emerges the well-known assemblage of both short-run and long-run supply and demand curves, and of both total and marginal cost and revenue curves. Conversely, the sales plans of households and purchase plans of firms yield the relative prices of productive services.

The second part (2 chapters), by modifying some of the assumptions of the original model, works into models of "impure and imperfect" competition. These the author regards as useful principally because they afford better market structure classification ("morphology") rather than more useful hypotheses. The works of Robinson and Chamberlin are regarded as "attempts to verify the assumptions of traditional analysis" and as yielding "no more useful hypotheses than those that were already available" (p. 260), that is to say, in Marshall.

The third part (1 chapter) deals summarily with mathematical programming and theory of games, which are characterized as "alternative techniques of analysis" (p. 370).

Ryan has succeeded admirably in accomplishing what he set out to do. His study certainly takes its place with the standard texts in the field of price theory. The criticisms of this reviewer, in fact, spring from Ryan's self-imposed limitations. In the first place, observing that the traditional tools of analysis are "suffering a rapid obsolescence" (p. vi), he accepts the techniques of programming and game theory in the analysis of price theory. The question then arises: if the mathematics (say, convex sets, linear inequalities, and so forth) used in these areas is thus admitted in whatever form, when does centuries-old calculus make the grade? Ryan states that he has "relied mainly on the neo-classical tools of analysis—namely, revenue and cost functions and their derivatives" (p. 370). Yet he observes the customary taboo against differential calculus, and this despite the fullest involvement with continuous variables, very small changes in variables, and rates of change of variables. Are we to presume that students of advanced economic analysis are still incapable of learning some simple derivation, though quite capable of digesting contour mapping, indifference configurations, and heavy prose explanations of changing rates of change? It is indeed good to see that in an increasing num-

ber of theory texts in this country the elementary calculus has at long last escaped from the appendix into the main body of discussion.

In the second place, while I would be very much disposed to adopt Ryan's text in my course in advanced economic analysis, my use of it would be qualified by a repeated note of caution. Price theory, as set forth by Ryan, suggests a tight, fortified island to which, it seems, only new techniques can gain access. The economist who operates on this island of theory accepts the underlying assumptions as to household and firm behavior that Ryan defends ingeniously and stoutly. He will also display ruling conceptions of human behavior that, by the very definition of his conventional role, are not to be subjected to interdisciplinary evaluation and revision. The danger for the student is that, lulled by the formidable apparatus and synthetic polish of the theory, he may fall into unthinking acceptance of its basic premises and, so to speak, become an island economist himself before he realizes what it is all about. For this reason, I emphasize to my students that our formal classroom preoccupation with the logical consequences of the assumptions of received price theory in no way commits us to them. This does not mean that I mind if they fail to join me in disliking the assumption that households maximize satisfactions, or know their indifference curves. It means that the student should form his attitude consciously, in the light not only of Ryan's views but also of some of the critical literature on the subject. Ryan's text, therefore, requires supplementation. It is a Euclidean text, and for non-Euclidian inspiration the student will have to go elsewhere.

HARRY MALISOFF

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## Economic History; Economic Development; National Economies

*The Economic Development of Western Civilization.* By SHEPARD B. CLOUGH. New York: McGraw-Hill, 1959. Pp. xv, 540. $7.50.

The subject of Professor Clough's latest book is the evolution of the basic economic institutions and patterns of western civilization since the decline of Rome to the present day and their impact upon other facets of this civilization. I have great respect for any scholar with the temerity to attempt such a Herculean task. What is more, I think Clough has done an admirable job. I would be proud if I had authored this book. His style is fluent, simple, and delightfully refreshing; his 51 illustrations and 13 maps are well chosen and provide valuable teaching aids; his more than 50 tables and charts present considerable quantitative data in a manner which most students will readily comprehend; and his over-all organization, presentation, and interpretation of economic change are sound.

An introductory chapter briefly explains the nature and importance of economic growth, and the second chapter summarizes man's technical advances before the genesis of western civilization. Four chapters then trace the evolution of the medieval economy; six concern themselves with economic expansion from 1500 to 1800; the following six deal with the period of rapid growth preceding the first world war; and the final four chapters—containing many

tables and charts—present a cursory analysis of the main problems and trends of western culture during its recent "time of troubles." Clough, therefore, resists the temptation to concentrate upon economic developments after 1800 at the expense of slighting the earlier period. As a matter of fact, I personally find his treatment of the centuries preceding the nineteenth more satisfying than his summary of subsequent trends. He skillfully combines economic developments in the New World with those of the Old and especially attempts to explain the various shifts in economic leadership among nations. He is aware of most of the "new viewpoints" in European economic history, and he stresses most of the factors and events which I think should be stressed.

If I had written this volume, I doubtless would have committed a great many more errors of fact and on the whole would have expresed myself less felicitously than Clough; yet I might have avoided a few of his minor slips. For example, according to the definition which most economists follow, "forced saving" does not include investments which individuals make with voluntary savings and which later prove to have been unwise (see his footnote on p. 13). The diversion of vast amounts of productive resources to the construction of opera houses, pyramids, and country clubs should not be condemned (if one prefers to condemn them) on the ground that these were *noneconomic* [sic] endeavors but rather on the ground that the resources could have been better employed if they had turned out capital goods rather than such durable consumer goods (p. 15). Instead of stating that "economic theory invariably reflects the environment from which it emanates and hence varies from place to place . . ." (p. 207), I would have written that the problems which interest economists and the public policies they espouse vary with time and place, whereas the analytical tools they have gradually developed during the past are abstractions and hence independent of any particular environment. I would have switched the definitions given for carding and combing textile fibers (p. 261). I would not have written that Kay's flying shuttle was so widely and rapidly used as to have created a bottleneck in spinning and hence forced improvements in spinning techniques (p. 264). Wadsworth and Mann, more than two decades ago, presented convincing evidence that this was not the case. The success of the clipper ships in pulling oceanic freight rates down was more pronounced prior to the American Civil War than after 1873 (see p. 308). How could the value of French assignats—or of anything else, for that matter—have decreased by 2000 per cent? A more serious confusion exists over the limited liability of shareholders in joint-stock companies. In one place (p. 152) Clough leaves the impression that the first joint-stock companies of the early seventeenth century offered investors limited liability; in two other places (pp. 315, 345) he more accurately describes limited liability as an innovation of the nineteenth century.

All of the foregoing are minor or even petty criticisms. In my opinion they do not lessen the value of this book. Without reservation I recommend it as a useful text for college courses in the development of the basic economic institutions of western civilization.

WARREN C. SCOVILLE

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*The Struggle for a Higher Standard of Living: The Problem of the Underdeveloped Countries.* By WILLEM BRAND. Glencoe, Ill.: The Free Press; The Hague: W. Van Hoeve, 1958. Pp. ix, 438. $7.50.

Professor Brand divides his analysis of the development process into three main parts: Part I examines the difficulties of the low-income countries in terms of the present status of land, capital, labor and "leadership plus organization." Part II directs attention to domestic financial resources which are analyzed by examining the behavior and the determinants of the behavior of consumption, investment, foreign trade, and the government. A final chapter on inflation completes this part of the book. Part III is concerned with the quantity and source of international financial resources available to countries pursuing a development program.

The author places heaviest emphasis on leadership and organization, and asserts that dynamic leadership may overcome the problem created by capital, land, and labor obstacles. The need then arises to explain what kind of society generates individuals with initiative and organizing ability. Although Brand does not present a theory of the creation of an entrepreneurial class, he does have some things to say on this subject that, while not new, bear repetition. He attaches great importance to immigrants in the early history of the United States, Canada and Australia. But large-scale immigration to present-day underdeveloped countries is no longer possible, and the author is not completely convinced of the effectiveness of foreign technical experts brought in on a short-term basis. This leaves the local government to meet the entrepreneurial function. There are problems here too. Giving a man a government job does not thereby make him an expert at organizing and carrying through projects, and in the type of country considered in this book, the government bureaucracy is as underdeveloped as any aspect of the economy. So the question remains: How to create an indigenous entrepreneurial class.

The emphasis on leadership and organizational ability has considerable appeal. There is much evidence that available resources in the underdeveloped countries are not being used in a manner that contributes most to the development of the economy. Presumably with better leadership and organization a more effective contribution could be made. Such emphasis, however, does not lead to very obvious policy conclusions. Until we are able to answer the question as to how an indigenous entrepreneurial class is created, placing it in the strategic role is to provide a rather pessimistic outlook for those countries seeking to develop.

But it may be that this is not the best question to ask. It is easy to convince oneself that entrepreneurs are created in the course of the developmental process, and that men become entrepreneurs only by engaging in entrepreneurial activities. It is therefore necessary to find ways of initiating development without a pre-existing highly gifted leadership group. That this is not impossible is evidenced by the growth and change that is now taking place in countries where there is very little effective entrepreneurial talent available. This means that development will take place in an irregular, haphazard sort of way; and a policy of aiming for balanced or smooth or optimum growth

rests on a misunderstanding of social evolution. These remarks are not meant to be a criticism of Brand's (or others') emphasis on the entrepreneur, but only that such emphasis has significant limitations as a guide to policy and as a device for shedding light on the process of development.

Two remarks on the method employed in the book should be made. To a very large extent the procedure used is first to present a problem, then to provide a summarization of some of the literature having to do with this problem, and finally to offer a commentary on the arguments of the literature cited. Consequently there is a great deal of exposition of other people's ideas (footnotes are placed in the back of the book and require some 68 pages), and in many cases it is difficult to ascertain just where the author stands on a given issue or what kind of analysis he feels is most rewarding. The author has read widely and absorbed his material well, but he has not chosen to use it as a springboard to work out new ways of thinking or to make a positive contribution toward revisions of existing ideas and arguments. The reader conversant with the literature in this area will find little that is new in the way of empirical hypotheses and/or theoretical formulations.

The author has also contented himself with very loose, imprecise statements, and does not provide a careful and complete tracing out of the arguments he discusses. This is especially evident in the discussion of the problem of the criteria of allocation of capital, and in the frequent references to external economies and "social overhead facilities." For example, on page 258 he writes, "For sound and harmonious economic growth, a certain balance has to be maintained between the increases in the various forms of capital goods." The policy-maker seeking guidance or the student seeking to understand the developmental process will hardly find this to be a revealing observation. It is one of the paradoxes of the economics of development that it is a relatively simple matter to make appropriate highly abstract generalizations and, at the same time, exceedingly difficult to give exact formulations that show the mechanics of the developmental process. Concentration of effort on the latter exercise seems definitely called for at this stage of our study of development.

The question of bilateral technical assistance contrasted with a program sponsored by the United Nations or other international organizations is discussed briefly in several places in the book. This is a particularly important issue at the moment, and given the author's long association with the UN, I would have preferred that he had spent much more time on this subject than in examining topics conventionally covered in the literature. Especially helpful would have been the author's appraisal of the effectiveness of the UN technical assistance program plus some discussion of the problems in establishing and directing such a program.

The book was originally published in Dutch in 1954, and according to the preface only minor additions and revisions have been made in the English version. The author was with the Secretariat of the UN from 1946 to 1957 and is now at the University of Leyden.

Henry J. Bruton

*Tehran, Iran*

*Desarrollo y equilibrio en la economía actual.* By ALFREDO LAGUNILLA INARRITU. Madrid: Aguilar, 1958. Pp. xv, 312.

Alfredo Lagunilla is a professor at the University of Mexico and Director of Research at the National Bank of Mexico. His *Desarrollo y equilibrio en la economía actual* (Development and Equilibrium in the Present Economy) is a work on business cycles and growth. His purpose is to find a way which will lead to economic growth in our modern economy. He asks the question: Is it enough to emulate the policies which have been used so successfully by the industrialized countries, or should new ideas be tried out?

Lagunilla is by all means for trying new ideas. In his opinion, the approach of the classical and also of what he calls the "postclassical" economists (Keynes and Hansen are "bad imitators of postclassicism") only served the purposes of the industrialized nations. Monetary and fiscal policy are only helpful in countries with hard currency where such measures are used by the government to help increase the amount of savings and investment; in the soft-currency areas of the world, these same measures are ineffective because the lack of savings makes other than inflationary investment impossible.

While the author is Keynesian in his basic macroeconomic approach and agrees that government stimulus to aggregate demand may be needed to avoid stagnation, he rejects some other elements of the Keynesian theory. His discussion of these problems is not always clear; thus he attributes to Keynes the idea that economic development programs should be financed with funds created in government banking systems, while in reality Keynes was mainly concerned with devising a method by which savings (real and not only nominal savings) would re-enter the income stream.

According to Lagunilla, the nonindustrialized areas of the world will only begin to grow if economic life is put under complete control. In a "functional economy" (as he calls it) production is to be entirely planned in order to keep economic life in balance. Prices and taxes are to be used to influence production and consumption: to raise production when demand is high and reduce consumption when the limits of productive possibilities have been attained. According to the author, when production and consumption are totally balanced (and this is the final purpose of his functional economy), taxes will not be needed any more. "Functional economy" thus is to "socialize value" and thereafter money will serve only to measure production and consumption quantitatively.

Lagunilla is widely familiar with the literature in the whole field of economic theory. Sometimes he makes almost too much use of his knowledge and his argument gets lost among the number of historical examples. The main interest of his book, especially because it was written by a Latin American, would have been in what he had to say about the development of the underdeveloped areas of the world. Unfortunately, Lagunilla is not helpful in this respect. Like many other authors, he too neglects to define what he really means by an underdeveloped area; what he does is to divide the world into haves and have-nots. While it is undoubtedly true that the national governments of Latin America had to start the economic-development process them-

selves, and that the application of marginal and comparative-cost analysis as developed by the classical economists is not always the pertinent answer in the new Latin American countries, this does not mean that the answer to the Latin American problem lies in a "functional economy." Maybe it would have better served his cause if the author had followed the path opened up by Raul Prebisch in his *Theory of the Periphery*, or even that of the late Ragnar Nurkse whose basic results, although he was a "classicist," are not so far from those of Prebisch.

ARTHUR KARASZ

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*Ekonomicheskii zakon preimushchestvennogo rosta proizvodstva sredstv proizvodstva.* (*Economic Law Concerning the Faster Growth of Output of Producers' Goods*) By A. I. PASHKOV. Moscow: Gosplanizdat, 1958. Pp. 232.

The title of this book (whose author is a corresponding member of the Soviet Academy of Sciences) describes its contents somewhat lengthily but correctly. According to Mr. Pashkov, there is an economic law in obedience to which, in a growing economy, output of producers' goods necessarily grows at a faster rate than does output of consumers' goods. The law is said to have been discovered by Marx and to have received its final formulation at the hands of Lenin. It is slightly disconcerting that in the numerical growth models which Marx produced in Volume 2 of *Das Kapital*, output of producers' goods and consumers' goods was assumed to develop at the same rate. Pashkov explains, however, that in those examples Marx abstracted from technological progress. When this assumption is relaxed and technological progress introduced, "variable" capital, the author says, is steadily replaced by "constant" capital which necessarily implies that output of producers' goods outstrips that of consumers' goods.

Historically, the author explains, the validity of the law dates from the introduction of machinery into the process of production. Pashkov even criticizes Stalin, who is said to have erred in believing that the law governed even the premachinery stage of economic growth (p. 19). Yet once that stage is overcome, the law becomes both ubiquitous and eternal. It characterized capitalist development in the past and still continues to do so because "in the epoch of imperialism and the general crisis of capitalism, technology in capitalist countries continues to develop despite the tendency toward putrefaction" (p. 49). [One must not infer from the use of a term such as putrefaction—*zagnivaniye*—any special predilection of Soviet economists for strong language; such terms are coined at the very top of the Soviet hierarchy and are then frozen into a general usage which seems both obligatory and colorless.] As far as Soviet Russia is concerned, the law has been in force during the "period of transformation from capitalism to socialism" which period extended from 1917 to 1936. It has continued to prevail since 1936, i.e., since the "establishment of a socialist economy in Russia" [another verbal icicle] and it will remain valid even in the communist society of the future, as the

author informs us with a prescience that is quite unmarred by doubt or reservation (p. 85). On the other hand, if the law were not valid and if output of producers' goods and consumers' goods were to develop at the same rate, constant productivity of labor and "complete absence of any technological progress" would be the inevitable result (p. 140).

All this does not make too much sense. Most of the time, it seems, the author has only a vague notion of what he is talking about. The statement just quoted does not, for instance, prevent him from stressing elsewhere in the book (p. 59) that much technological progress can be achieved through replacement of worn-out equipment, that is to say, in principle at a zero rate of growth of producers' goods output. The inference—*a minori ad maius*—that technological progress must be possible at *any* positive rate of output of producers' goods—whatever its relation to output of consumers' goods—eludes him entirely. There is, incidentally, no sign of comprehension on the part of the author that the "law" which he expounds with so much vigor implies a steadily *growing* rate of investment, leading eventually to a situation in which —*ceteris paribus*—investment would come to constitute the bulk of national income and the rate of growth of output would tend to approach the output-capital ratio asymptotically.

In discussing the effects of changes in the ratio of constant to variable capital, Pashkov does not allow himself to be perturbed in the least by problems such as the degree of unemployment, consumption of profit receivers, or the index number problem. Accordingly, it never occurs to him that a labor-saving innovation in a relatively labor-intensive branch of the economy may well result in shifting factors of production to that branch and away from other more capital-intensive branches, making the economy as a whole *less* rather than more capital-intensive. That capital-output ratios may fall not only because of faster growth of branches in which such ratios are low but also because labor-saving innovations may be more than offset by capital-saving innovations; and that accordingly even a declining rate of investment may under favorable circumstances be associated with a high rate of technological progress and growth of output is something Pashkov would like to ignore but cannot. For both Polish and East German economists seem to have made the point with much vigor and resonance.

In addition, there has been some criticism of the "law" from indigenous sources, but these could be handled more effectively. Pashkov devotes some time to criticizing articles of Soviet economists that have been submitted to *Voprosy Ekonomiki*, a leading Soviet economic periodical, or to the Institute of the Academy of Sciences, but *not* published by them. This remarkable trouble-saving innovation in the area of academic polemics is likely to blur and to distort the dissenters' views. Still, it seems clear that the suppressed Soviet economists tried to argue that growth and technological progress were quite possible at a constant rate of investment and also felt that constant rates of investment and consumption were much more consonant with the basic principles of socialism.

Abuse apart, Pashkov has little to reply to those strictures. He has spent

some time trying to verify his law. In the case of Soviet Russia, Soviet policy would have provided full support for his contentions, even if unsupported by faulty index numbers. For the rest, his statistical compilations, which are confined to manufacturing, are astonishingly primitive and reveal ignorance even of those historical materials which he could use most effectively (Walther Hoffmann's well-known study of stages and types of industrialization being an obvious example). Nevertheless, Pashkov was quite willing to consider his statistical ragout as "incontrovertible" evidence in support of his law (p.46); and yet when faced by dissenting views and, in particular, by empirical data for the more recent economic history of Western countries which are not consistent with his thesis, he is eager to escape at once into generalities and to accuse the critics of confusing a simplified abstract model with description of complex reality (pp. 180-81). The truth is that he never makes up his mind as to whether his law denotes historical "inevitability," an empirical regularity, or simply a condition of rapid growth.

Pashkov's erudition is less than overwhelming.[1] He does an exceedingly poor job in defending his thesis. But this does not mean that his book should be brushed aside as unimportant. The contrary is true. For what he defends and for what he is trying to find some theoretical basis and vindication is nothing less than the official line of Soviet economic policy; in fact, its guiding principle. In his *Theses* (Control Figures for the Development of the Soviet Economy, 1959-1965), N. S. Khrushchev referred to "Lenin's doctrine" concerning "the faster growth of heavy industry" and to the need for setting the plan targets for the current seven-year plan accordingly.[2]

Since the inception of the five-year plans Soviet economic policy has been essentially directed to investment for investment's sake. It is largely by *increasing* the rate of investment that the Soviets have been able, in recent years, to avoid considerable declines in the rate of industrial growth. There is very little doubt that, in any foreseeable future, there is no *economic* reason to prevent the Soviets from continuing that policy, i.e., from acting as though there were in fact an economic law of the sort suggested by Pashkov. And there may be very weighty political reasons relating to the technique of power exercise by a dictatorship to make it appear advisable to the Soviet government to continue such a policy more or less indefinitely.

Thus the present book is an excellent example of the traditional Soviet policy of using Marxian theory and ideology in order to justify policies which were pursued for eminently practical reasons that had very little to do with Marxian theories and beliefs. But Pashkov's book reveals more than that. For

[1] He even presents an apologia for the Soviet statistical use of the concept of "gross value of output" which is based on aggregations of values of product of individual factories, involves numerous double-countings throughout the process of production, and varies with changes in the structure of industry and the concept of "factory." The reason given in defense of such a gross concept ("gross" in every sense of the word) is that value-added data do not include the "most important part of the reproduction process," i.e., "producers' goods designed to produce producers' goods" (p. 81).

[2] *Sovetskaya Rossiya,* November 14, 1958.

it is at the same time a very good example of what happens in this process to Marxian theory and ideology. One of the basic elements of Marxian doctrine used to be the emphasis upon the historical nature of economic laws. The Marxian critique of the Austrian school, for instance, was largely directed against the concept of general economic laws, common to all stages of economic development and to all economic systems. In particular, Marxian scholars at all times stressed the specificity of laws of capitalist development. It is indeed most instructive to see how something that is presented as a fundamental law of economic growth is now declared to be common to both capitalism and socialism. Pashkov himself seems at times astonished at his own views and keeps looking for some *differentiae specificae* without being able to find anything more convincing than the law of increasing misery which, he says, is conjoined with the operation of his law of growth under capitalism (pp. 21, 59, 64). To repeat, the function of ideologies changes with the times. But the content of ideologies changes with their function.

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*Aspetti e problemi dello sviluppo economico.* By MARIO TALAMONA. Milan: Società Editoriale Mondo Economico, 1958. Pp. 194. L. 1.200.

This book contains fifteen essays on various aspects of the problem of economic development. Several of the papers were first published between 1956 and 1958 as articles in the review *Mondo Economico*; they were revised, enlarged and provided with detailed bibliographical notes before their inclusion in the present volume.

In the opening paper on "Economic Development, Economic Theory and Value Premises" the author discusses the importance of a well-defined conceptual framework for any fruitful investigations in the field of economic development and calls attention to per capita income as a basic analytical criterion; he then briefly reviews the contributions made to development theory by classical, Marxian, and neoclassical writers, and calls for new work on basic concepts and for the development of more adequate analytical tools.

This thoughtful introductory paper is followed by two essays on the role of the West in economic development: the first emphasizes the challenge of socialist ideologies in the field of development and the need for appropriate Western responses; the second contains an eloquent plea for more intensive and fruitful integration of the West's international economic development efforts. There is then a general essay on economic development and balance-of-payments equilibrium in Latin American, Near Eastern and Asian countries, and a special study on the recent experiences of India in this field.

These chapters are followed by five papers on recent economic development in Soviet bloc countries: the first three deal mainly with the Soviet Union, the remaining two with the differing "roads to socialism" followed by Poland and Yugoslavia. There are then two studies on certain development problems of the *Mezzogiorno*, Italy's long underdeveloped South: the first

on capital investment in Southern Italian industry, the second on the development of certain types of Southern consumption. The book ends with three general studies, on foreign capital investment in Italy and recent legislation pertaining thereto, and on the role of residential construction in economic development.

Although a wide range of topics is dealt with in this moderate-sized volume, the essays which it contains make instructive and stimulating reading. They endeavor to throw light on some special aspects of the general problem of economic development which have attracted the author's attention or have appeared to him to constitute fruitful avenues for further research. In a number of instances the comments offered are accompanied by citations of interesting facts. In the essay on India's development effort for example, attention is called to the circumstance that, although there are many socialist overtones in India's development program, the Indian government's share in the country's gross national product was, a few years ago, still only about 10 per cent of the total, or considerably less than that of a number of other South Asian countries. Again, figures given in the essay on capital investment in southern Italy indicate that during the past several years the equivalent of well over $1 billion has been invested in that region, mostly through the *Cassa per il Mezzogiorno*, the Italian government agency for development financing in Italy's southern provinces.

Although a number of the papers in this volume are rather brief and are concerned mostly with certain special aspects of the general problem with which they deal, their publication in book form represents, nevertheless, a welcome contribution to the literature of economic development by an informed and perceptive writer.

William G. Welk

*Washington, D.C.*

*Economic Development of Communist China: An Appraisal of the First Five Years of Industrialization.* By Choh-Ming Li. Berkeley and Los Angeles: University of California Press, 1959. Pp. xvi, 284. $7.50.

This book is a quantitative account, largely in aggregative terms, of the economy of Communist China during the first five-year plan (1952-57). As such, it is the best available compilation of the data, assembled with great diligence and care mainly from official Chinese-language sources inaccessible to most Western economists. It is also notable for careful checking of the data, and for efforts to construct over-all and analytic measurements which have been sorely lacking for that country. The scope of this study, however, is more restricted than the title implies, for it is chiefly an estimate of overt trends rather than an "appraisal" of the Chinese development as a process.

The volume begins with an introductory chapter on the first five-year plan, the extent of the socialization of the economy, and the character of markets and prices under the Communist regime. The next three chapters are concerned with the growth of output in industry, agriculture, and the whole national product. Another three chapters deal wih the rate of capital formation,

the sources of internal finance, and the magnitude of external transactions. A final chapter discusses future prospects in terms of population growth and agricultural requirements, and is followed by a valuable statistical appendix on the structure and output of the economy.

On the crucial question of the growing strength of that economy, Li estimates that during the five years of the plan, according to the official data, industrial output advanced by 16 per cent a year, agricultural output by 4.8 per cent a year, and net national product by nearly 9 per cent a year. But he throws serious doubt on these figures. While he reports (p. 3) that he found "no evidence of deliberate fabrication" of data, he considers the trends to be greatly exaggerated by the pricing methods used, the pyramiding of gross values, and improvements in the methods of reporting.

On the other hand, Li's estimates omit the "mass mobilization projects," which he notes at several points but dismisses as "not appreciable" (p. 109). These projects, constituting perhaps the most remarkable contribution of the Chinese Communists to developmental possibilities in very poor countries, are open to much debate. Recent reports, which have accumulated since Li finished his book, indicate that the regime has found ways of utilizing the vast reservoir of underemployed labor-time, for adding to capital construction and current production without fully proportionate outlays on wages and equipment. It is also reported to have found ways to utilize innumerable small pockets of resources without incurring the social costs of constructing large "modern" facilities in each locale. If one gave greater weight to those operations, the alleged increases of production would be more credible. Similarly, these operations, which amount to "investment in kind," may also explain the monetary estimates of the capital-intensity ratios which otherwise seem so puzzling (see pp. 137-38), since the marginal capital-output ratio for China was apparently below that of India whereas one would expect a higher ratio in view of the much greater Chinese emphasis on heavy industry. Perhaps a similar explanation can be provided for the Chinese ability to advance with so little aid from Soviet Russian grants or loans, which Li computes at less than 4 per cent of China's net fixed capital formation in monetary terms during this period.

These considerations point toward a still deeper level of significance, namely the drastic social transformations which the Chinese Communist leadership has ruthlessly enforced upon their people. To what extent do these transformations account for the great outpouring of national energy, as well as the major dislocations, recently described by Western observers and by official Communist agencies? To what extent can the means of releasing such energies be adapted to democratic auspices in other retarded countries without an accompanying loss of personal and economic liberty? These matters occupy little space in Li's book, which is restricted mainly to aggregative, monetary, and *ex post* description. There is a vital subject here for a further study of the dynamics of economic evolution in Asia.

EDWIN P. REUBENS

*The City College of New York*

*The French Economy and the State.* By WARREN C. BAUM. Princeton: Princeton University Press for RAND Corporation, 1958. Pp. xvi, 391. $7.50.

By concentrating on the role of the state in the French economy, Warren Baum, a former ECA official in Paris whose book is part of The RAND Corporation's continuing work on France, aims to do two things. He uses the array of governmental policies as a framework on which to mount a description and analysis of the way the French economy works. At the same time he is interested in analyzing the extent, character, and results of the government's intervention in the economy. As to the bearing of the French experience on the perennial general discussion of the "proper" role of the state, Baum largely lets the record speak for itself, and it does not speak clearly. His own analysis of intervention is strongest when he is dealing with specific problems or fields of action, where he can get fairly precise answers to the repeated basic questions: Why did the government act? What did it do? What were the results?

When these questions are asked about the whole range of government actions, however, the answers are too general to be very revealing. Like most governments, those of France have intervened in the economy in many ways and for a number of purposes, some of them conflicting. ("To promote economic . . . growth. . . . To preserve particular groups or classes against the forces of economic change. . . .") The convention of distinguishing between "economic" and "political" books curtails Baum's discussion of why some measures of state intervention were undertaken, why contradictions were maintained, and why many measures were never effectively enforced. Even so, the book makes clear the salient features of French interventionism, notably its extent, variety, and intricacy. State action is apt to turn up at every stage of an economic process (and often on every side of an issue—"*Chacun a son compte;* everybody has his deal").

The anatomizing of this pervasive intervention is one of the main contributions of the book. The other is the use of this analysis as a skeleton for a well-proportioned, clearly written exposition of the operation of the French economy from 1945 to early 1955, which occupies the largest part of the book. We have not had this kind of account in English before. It provides an excellent general picture and a whole series of separate treatises that are first-rate. Major sections are devoted, for instance, to the Monnet and Hirsch plans, balance-of-payments problems, the budget, cartels and competition, prices, agricultural policy, wages and labor relations. Here at last we can find good, brief expositions of such matters as the French tax system, the process and extent of nationalization, the policies followed by nationalized enterprises, the cyclical and inflationary movements of the French economy traced year by year, and other matters that in the past have had to be quarried out of reports and articles.

There are no great surprises in this book except for those who have been misled by travelers' tales and too journalistic an approach to the French economy. Baum clarifies and develops a picture already discernible in some articles and reports. One of the strengths of his study is the way it digests,

analyzes, and sets forth in a useful and significant fashion a substantial body of material drawn largely from French sources. It is one of the responsibilities of the profession to provide from time to time authoritative and comprehensive works like this. We have not had such a book on France in English and Baum has done well to provide us with one. Since he wrote we have seen a new surge of the French economy, another round of crises, and the beginnings of a new set of policies and reforms that may conceivably change the familiar pattern. It is the fate of books like this to run behind events, but this is not really a serious limitation. When well done, as this one is, they become the standard works on one phase and provide the footing for the next stage of analysis.

WILLIAM DIEBOLD, JR.

*The Council on Foreign Relations*

*Industrial Change in India.* By GEORGE ROSEN. Center for International Studies, Massachusetts Institute of Technology Series. Glencoe, Ill.: The Free Press, 1958 Pp. xxv, 243. $5.00.

The literature on underdeveloped countries is usually divided between theoretical contributions on the one hand and case studies on the other. Seldom is there an attempt at linkage. Dr. Rosen's study makes a contribution in this respect by singling out certain strategic functional relationships for empirical research into India's industry. The basic purpose of the work, according to the author, was to utilize existing statistical materials to compute the historical trend (1937-1953) of capital-output and capital-labor ratios in certain important factory industries in India; namely, cement, paper, iron and steel, cotton textiles and sugar.

Most of the theoretical distinctions made between marginal and average ratios or between the marginal productivity of capital and the capital-output ratio will be familiar to economists, but the discussion of the pitfalls involved in calculating and utilizing these ratios is stimulating. For one, the margin of error in the statistical computation of these ratios is very high even in the United States (probably above 20 per cent) and is probably much higher in India. The author points out that a capital-output ratio of 3:1 with a probable margin of error of one-third would mean a range within which the ratio actually falls between 2:1 and 4:1; and with the reciprocal rate of return thus varying between 50 and 25 per cent (a 100 per cent difference), it would seem that use of the ratio would be hazardous. Why then calculate these ratios? Their main use is as a tool in planning, for which they are presumably more significant than the broad sectoral estimates implicit in India's second plan. In any case, Rosen's calculations come out broadly consistent with Mahalanobis' model ratios of 5 for heavy industry and 2.8 for large-scale consumer industries.

There are many possible ways of defining capital-output ratios. Rosen chose a gross ratio of gross fixed capital, including land and vehicles (taken from balance sheets and adjusted in various ways), against capacity operation. The

latter was chosen rather than actual output to exclude the influence of the business cycle, war and unrest. A value-added concept of output rather than sales value was used. The ratios thus computed provide a measure of gross fixed investment per value unit of GNP derived from each of the five industries. Totals for capital and output value-added were computed for each industry annually and deflated to 1939 prices. The end result is a time series of average fixed capital-output ratios for each of the five industries from 1937 to 1953.

The author is fully aware of the statistical daring involved in his procedures and in the assumptions underlying them. For example, it was assumed that the firms computed accounts in a comparable fashion, which he knew was not true for certain items. Also it was assumed that public records were accurate accounts of true financial transactions in spite of the Report of the National Income Committee to the contrary. There are other leaps such as aggregating data from firms at different degrees of integration or the use of one price index from the United Kingdom and the construction of another on the basis of impressions of the movement of machinery prices gained from interviews with textile manufacturers. Then after all the daring procedures, the average ratios are not useful directly for planning, though the implicit marginal ratios contained in the series are given. The author felt it necessary to compute marginal ratios from actual costs of plant construction or engineering data for plant expansion. There was, however, considerable consistency with the implicit marginal ratios computed from the financial data. The engineering marginal ratios are considered superior to those derived from the average ratios; but it is clear that everything is very tentative (the author's language is indeed very cautious) and the question arises whether these calculations surrounded with such dubiety are useful in planning.

On one level, speaking as one who first saw a well-worn copy of the book belonging to his office in the Planning Commission at New Delhi, this reviewer can testify that it is already in use. But whether over the longer pull this type of research will make planning in India's democratic framework any more successful than it has been up to this point only the future (and more studies like Rosen's) can answer. At least it is fair to say that this piece of empirical research is in the front line in the effort to get more realistic data for democratic planning. In some sense the kind of techniques used here have an affinity to those in Soviet planning. It is the engineering marginal ratios which have the greatest validity and this concept gets pretty close to the "norm" of Soviet planning. India's planners who are trying to work out their country's destiny in their own way are finding methods used in this study that can be set alongside the Soviet planning influences in New Delhi. It is quite possible that Rosen and his colleagues at M.I.T.'s International Center may be setting in motion profound counterinfluences in India's planning.

In addition to capital-output ratios, capital-labor coefficients for the five industries are derived and also show a rising trend. This trend toward both higher capital-output ratios and capital-labor ratios is startling when one recalls that factor proportions in India theoretically favor labor-intensive opera-

tions. Rosen has chapters discussing the institutional conditions in India which seem to negate this theoretical prescription. Lastly, there is a study of subcontracting in Indian industry which while providing very illuminating insights seems remote to the main concern of the book with incremental capital estimation in organized industry. One wonders why this type of interview research couldn't have been directed at providing such material on the firms involved in the five industries chosen. However, this book is an imaginative attempt to calculate critical planning tools from sparse and uncertain data. It is a real contribution to the literature on planning for industry in India's mixed economy pointing the way to further studies of its kind.

DANIEL L. SPENCER

*Southern Illinois University*

*Problems of Indian Economic Development.* By B. R. SHENOY. Madras: University of Madras, 1958. Pp. x, 236. Rs 10.00.

In 1955 a panel of distinguished Indian economists, at the request of their government, prepared a memorandum of comments on the draft of the proposed second five-year plan. Alone among the economists, Professor Shenoy of Gujarat University dissented on the scope of the plan, arguing that it was far too ambitious for the resources available or impending and that it would produce both domestic inflation and balance-of-payments problems. The dire consequences he predicted have occurred, and since then Shenoy has emerged as the major academic spokesman for a conservative, essentially private-enterprise economy in India.

This volume is a restatement and elaboration of Shenoy's dissenting position of 1955. Given as lectures early in 1956, revised for publication early in 1957, it was not published until mid-1958. Consequently, it suffers from the fate of all essentially polemic documents. The issues with which it is directly concerned have vanished into the past. New problems have appeared, associated with the shaping of the third five-year plan. However, Shenoy's volume is worthy of attention because of the general philosophical position he espouses and the economic assumptions with which he operates.

Putting his case briefly and generally, Shenoy argues that as a poor economy India has a very low rate of savings. In such a democratic society, where decisions to consume and to save are the results of individual choice, the rate at which savings will grow is likely to be extremely slow. In these terms, Indian planners vastly overestimated the rate of growth of resources available for investment in the second five-year plan. The effort to resolve this difficulty by tampering with money flows would not, he argues, be effective, and may in fact be self-defeating. The inflationary effects of vast credit creation would lead to excessive consumption, diversion of resources into nonessential investment and intensified gold hoarding. This, he claims, would culminate in the export of capital and the consequent reduction in the pace of economic development. For Shenoy, the ultimate touchstone of effective governmental policy is credit creation sufficient only to maintain a stable price level. Any attempt to exceed such a limit would lead only to inflation and disruption. Shenoy

indicates that he is perfectly willing to accept the consequences of his analysis when he comments that "The comparatively slow rate of progress of underdeveloped economies . . . is universal and inevitable" (p. 19).

There are a number of disturbing theoretical flaws in his arguments, but within the framework with which he operates Shenoy has some shrewd comments to make. He points out a number of critical inconsistencies in the plan and the weakness of many of its assumptions. One chapter considers the problem of agricultural price stabilization in agriculture and he raises some tartly useful considerations.

I do not propose to treat his analysis in detail, but it is important to suggest that Shenoy's "strong case" reveals the crucial weakness of attempting uncritically to apply a Marshallian short-run analysis to problems of economic development. Having warned the planners that they must not assume that credit creation in India can serve the same function as in developed economies facing substantial underemployment, he falls into a comparable fallacy in virtually every other respect. In effect, he treats the Indian economy as if it is already a developed, competitive economy in which there is full employment of all resources except labor. He has an extremely naïve view of resource rigidities and ignores the role of taxation as a means of transforming patterns of resource utilization.

Shenoy fails to recognize that in a stagnant economy of the traditional sort, economic growth means economic change, structural change. He is anxious to preserve "the democratic way of life," but for him this means in effect allowing the market to do all things. He fails to recognize that the market is not necessarily the best arbiter of social policy in the long run, economic theory being restricted by some rather substantial static assumptions. At no point does Shenoy face up to the problems associated with a high rate of population growth and the simultaneous mounting popular demands for rapid economic development. His Marshallian short-run model, based on the assumption that *natura non facit saltum*, is not likely to preserve the thin fiber of democratic institutions that has been woven in India, but is more likely to destroy it altogether. It is true that the planners in India have muddled badly, but they are on the right track while Shenoy is not.

MORRIS DAVID MORRIS

*University of Washington*

*Essays on Japanese Economy.* By SHIGETO TSURU. Economic Research Series 2, Institute of Economic Research, Hitotsubashi University. Tokyo: Kinokuniya Bookstore Co., Ltd., 1958. Pp. v, 241.

This valuable collection of previously published essays by a leading Japanese economist may be approached on three different levels. First, each chapter provides important information and analysis on a significant subject. The best-known essay is "Economic Fluctuations in Japan, 1868-1893," a summary of Professor Tsuru's doctoral dissertation (accepted at Harvard), published in the *Review of Economic Statistics* in November 1941. Here

Tsuru not only describes and interprets the economic ups and downs of his country during its first quarter-century of active modernization. He also tells much about the problems encountered and the course of development in that remarkable period. His information and insights—on capital formation, growth of a managerial class, inflation, and foreign trade—may well find a much more appreciative audience today than when this essay first appeared.

The volume's long final chapter, also written before the second world war deals with economic strain in Japan as a result of the "China incident." In his note at the head of this chapter, Tsuru acknowledges exaggerating Japan's weaknesses in 1941, but asserts that this essay "contains one of the first attempts, I believe, of the aggregative analysis which subsequently came to be refined in terms of gross national product and so on" (p. 154).

The remaining essays deal with the postwar period. Four chapters, written between 1949 and 1956, provide a valuable account of Japan's recovery and its main features. Two chapters, both written in 1957, deal in some detail with particular problems. He analyzes Japan's growing employment problem and shows how much its solution depends on foreign trade. His analysis of the 1955 economic plan tells much about the planning process and how events dramatically overtook the plan.

On a second level, this book may be examined for the views it reflects upon Japan's economic circumstances, prospects and policies. As a prominent left-of-center writer, Tsuru speaks for a substantial minority on some issues and for the majority on many others. When he says, on pages 93-94, "Exports in sufficient volume will be both a necessary and a sufficient condition for full employment prosperity," he is expressing a conclusion of profound importance that not only is accepted by Japanese generally but also agrees with the considered conclusions of the Japanese Economic Planning Agency and of most scholars who have worked on the subject. When in 1953 he urged (p. 55) that Japan "press for freer and larger trade with the continent of China," he stood alongside most of his countrymen, although changing conditions in the years since then have weakened this sentiment somewhat, so far as many Japanese are concerned. Part of Tsuru's reason for urging China trade is his expectation that other foreign markets cannot be found for enough Japanese exports and that consequently Japan will need to undertake all possible steps to alleviate the serious underemployment of the rapidly growing labor force. He includes among these measures not only an emergency public works program, but also TVA-type river basin projects, despite their very high capital costs and limited scope in the restricted Japanese land.

On a third level this book carries implications for U. S. policy. Tsuru is critical of the United States on important issues and regrets Japan's great economic and political dependence on this country. This dependence, he thought in 1952, would interfere with "free economic intercourse with the continent of Asia" (p. 36). He opposes Japan's alliance with the United States and Japanese rearmament. He regards Japan's dependence on military programs as a major defect in the character of the economy, today as in the fifty years before 1945. Although he speaks with restraint and tact, Tsuru is

clearly not very optimistic. Many other Japanese are much more sharply critical of the United States than he.

The problem for U. S. policy, then, is to conduct our trade and other foreign relations so as to dispel as far as possible the gloom in such Japanese views. It may be that U. S. trade policy will prove decisive as to whether critical attitudes in Japan, and elsewhere as well, harden into dangerous opposition or gradually soften with growing prosperity.

WARREN S. HUNSBERGER

*Council on Foreign Relations*

*Socialism in One Country, 1924-1926*. Vol. 1. By EDWARD H. CARR. New York: Macmillan, 1958. Pp. viii, 557. $7.50.

This fifth volume of Professor Carr's exhaustive and penetrating "History of Soviet Russia" is the first of three volumes which will examine the developments of the period 1924-1926, during which Lenin's disciples were espousing their conflicting theories of economic policy and Stalin was gradually emerging as the dominant power. Although it begins with some penetrating sociological observations on the changing outlook regarding family, church, literature, class and party, the book primarily analyzes economic developments.

The source material cited is almost entirely from original works in the Russian language. A fresh approach has been made to the events, no reliance being placed upon previous writings of journalists and other authors in English or other non-Russian languages. The result is a thorough, scholarly treatment of a critical transition period in Soviet ideological and economic development. Yet the style is limpid and the writing has sweep and verve.

The heart of the book is a portrayal of the struggle over agricultural policy following the failure of the harvest of 1924. Zinoviev exhorted the Party to "turn its face to the countryside." Opposing concessions to the peasants, Preobrazhensky called for "expropriation of the surplus product of the country for the broadening of socialist production" in industry. Bukharin linked this view with Trotskyism and contrasted it with Lenin's conviction that "progress toward socialism could be realized only in close alliance with the peasants" (quoting Carr). Trotsky was deposed from office in January 1925 and the question became, not whether to support agriculture, but which group of peasants to support, kulaks or small peasants.

Led by Bukharin, the notion grew that only a prosperous peasantry could provide both a market for industrial goods and capital accumulation out of agricultural surplus for the development of industry. Only well-to-do peasants could provide for future capital accumulation. "Cooperatives for the middle peasantry and kolkhozy for the poor peasant could go hand in hand with freedom of opportunity for the bourgeois peasant." But Stalin seems to have recognized that this turn of policy in favor of the kulak had "outraged a large section of party opinion." However, he held his fire until after the "new opposition" of Kamenev and Zinoviev had arisen to attack the prokulak policy. By the spring of 1926 "the incompatibility of the policy of intensive

industrialization with the policy of conciliating the well-to-do peasant was already apparent."

This is an oversimplified summary of the trend of the ideological struggle over policy. It is necessary to read the book to discover the complicated maneuverings, inconsistencies, and pragmatic adjustments to painful realities of the protagonists and shifting governmental decisions.

A chapter on "Industry" traces the rise of the doctrine of developing the "capital goods industries as the broad basis on which consumer goods industries could be ultimately expanded." Industrialization became "the economic corollary of socialism in one country," producing machines and equipment so that the USSR would not become "an economic adjunct of the capitalist world economy." What rate of industrialization was practicable became the subject of controversy, Stalin advocating an increased tempo of socialist accumulation in the development of heavy industry.

A chapter on "Labor" traces the shift of the trade unions from agents of genuine collective bargaining to agents of management against troublesome workers and in behalf of increased productivity. The ideological conflict between communistic egalitarianism in wage payments and the principle of employing wide wage differentials to stimulate productivity is well described. "The will of the party" ultimately became "the synthesis in which every conflict or contradiction between the trade unions and the political or economic organs of the state, between union officials and public administrators or managers, was ultimately resolved." Other chapters discuss Planning, Finance and Credit, and Internal and Foreign Trade.

The first "control figures of the national economy" were published by Gosplan in August 1925. Carr makes the interesting point that one strong factor in adding urgency to planning in the mid-'twenties was the persistence of mass unemployment, caused partly by the rapid population increase and the influx into the cities of underemployed peasant workers from the countryside.

There is a brief discussion of the familiar dispute between the "genetic" and "teleological" conceptions of planning, which the author rightly says was "not free from an element of unreality."

ROLAND GIBSON

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*Perestroika upravleniia promyshlennost'iu i stroitel'stvom v SSSR.* (Reorganization of the Administration of Industry and Construction in the USSR.) By A. N. EFIMOV. Moscow, 1957. Pp. 142 35c.; 2 rbl. 25 kop.

*Novoe v organizatsii material'no-tekhnicheskogo snabzheniia.* (New Developments in the Organization of Material-Technical Supply.) By N. S. GAL'PERIN. Moscow, 1957. Pp. 54. 10c.; 85 kop.

In the summer of 1957, the Soviet Union embarked on a massive reorganization of its industrial administration. From a highly centralized system based on ministries, primarily along product lines (usually numbering about 30), the change was made to at least a formally less centralized system based on

105 separate geographic regions. The abundant economic literature which has accompanied the reorganization has been of great interest and value, not only for the light it throws on the present and future situation under the reorganization, but also for the light it throws on the past. For most authors, in discussing the reform and the reasons for it, have pointed up the deficiencies of the past system with a clearness and directness very rarely seen when the ministerial system itself was in force. The two books reviewed here are good examples of this literature.

A. N. Efimov's book covers the reorganization in general, with an emphasis on planning, whereas N. S. Gal'perin's book is concerned specifically with problems of supply (a major factor in the reorganization). Both men are highly qualified for their respective tasks: Efimov is a professor, a doctor of economics and the director of the Economics Research Institute of the State Planning Committee (Gosplan), while Gal'perin is a special consultant to the Ministry of Finance, who has written a number of very good articles on the supply system.

Efimov begins his book with a chapter on why it was necessary at this time to change industrial administration to the territorial principle. He gives a short history of the organization of industry and states that the ministerial form has "played its positive role," but now it is a fetter on further industrial growth. In discussing its deficiencies, he emphasizes the absence of cross-ministerial coordination and the major consequences flowing from this: retardation of specialization and subcontracting for components; supply mainly within ministerial channels and thus often irrationally long transportation hauls; failure to develop diversified firms producing a range of by-products from the given inputs; and duplication of staff and organization.

The second chapter is an analysis of the "economic-administrative region," the territorial unit used in the reorganization. There has been much speculation in the West as to why such a large number of regions were set up. Efimov discusses the history of the attempts to work out usable regional patterns, indicating the conflict which developed between "economic" regions and "administrative" regions. He states that the principle in the reorganization was to have the economic-administrative region approximate the then existing administrative regions, but, he continues, this does not destroy the need for thinking in terms of broader economic regions. There is reason to believe, he adds, that in the future several economic-administrative regions will be enlarged.

The third chapter is concerned with the new "councils of the national economy" (*sovnarkhozy*) which were established to administer the economic-administrative regions. Efimov describes the organization, responsibilities and functions of the *sovnarkhozy*. In general, they are to perform for the firms within their regions the functions which were formerly performed by each firm's ministry. One significant exception is technical research which will be centralized under Gosplan. Efimov speaks of the specialization of economic regions and the importance of interregional relationships. He sternly warns against localist tendencies—a problem foreseen even before the reorganization came into being.

The fourth chapter is on planning under the new conditions and it is probably the most interesting part of the book. It begins with a detailed listing of the inadequacies of planning under the old system. Among these inadequacies, Efimov includes the hallowed method of balanced planning. Plans often contained errors and omissions, he claims, which led to shortages of materials, disruption of production schedules and idleness of plants. The Soviet method of balanced planning is based on the use of balance sheets called "material balances." These are constructed by product (varying in coverage over time, reaching a high in 1951 when there were 1,600 of them) and list the planned sources and planned demands for each product. Balanced planning is supposedly a process whereby the sources and demands on each and every material balance are brought into balance. Efimov indicates, however, that this mutual balancing is an exceedingly difficult problem. There are not only "1st order" relationships between products, i.e. good 2 an input into good 1, but also "2nd order" (2 into 1, 3 into 2), "3rd order" and so on. All of these direct and indirect relationships must be taken into account when a change in any planned output is made if a truly balanced plan is to be achieved. This, he informs us, is not done in present Soviet planning. Often only 1st order relationships are considered, thus giving rise to imbalances and strains in the plan. The solution, Efimov asserts, is to go over to a system of input-output planning. He describes what is essentially the Leontief system, enumerates its advantages for a planned economy and mentions some of the input-output work already done in the Soviet Union by Gosplan and the Academy of Sciences.

Also in this chapter, Efimov discusses the new role and organization of Gosplan, the new emphasis on the long-term plan with yearly breakdowns to give more continuity to the annual plans, and the decrease in the number of plan indicators to be confirmed by the central government. The final chapter is concerned with the problem of specialization of firms and subcontracting. The book ends with a six-page summary of the improvements hoped for from the reorganization.

The format of Gal'perin's book is similar to Efimov's, although, as was stated, Gal'perin is concerned only with the reorganization as it affects the system of supplying industry and construction with materials and raw materials.

The book begins with a short but useful chapter on the history of the organization of the supply system. The second chapter is a wonderfully rich discussion of the deficiencies of supply under the past ministerial system. In his review of the failure to meet delivery plans and the failure to fulfill plans for the reduction of inventories, Gal'perin provides some rare and interesting data. He is very critical of the work of the former "main administrations of sales" (*glavsbyty*), which were attached to each ministry. In the last stage of constructing the annual plan, the consuming ministry would send the producing ministry a specified list of products it required (within its aggregate allotment). The *glavsbyt* was then to allocate these specified orders among producing firms within its ministry. Gal'perin claims this whole process was handled very inefficiently, thus leading to imperfections in the supply system.

The importance he places on this is much greater than what appeared in the prereorganization literature and might help to explain the stress now being put on direct contracts between firms. The final chapter contains one of the best descriptions available of the new organization of supply as it was developed up to the end of 1957.

Though both these books are short and in brochure form, which usually signifies a popularization, they will reward the efforts of the interested reader and provide him with much valuable information, previously unavailable in published sources.

HERBERT S. LEVINE

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*Ekonomicheskoe sorevnovanie dvukh mirovykh sistem.* (Economic Competition of Two World Systems.) Edited by A. M. ALEKSEEV. Moscow: Gospolitizdat, 1957. Pp. 382. Rbl. 9.

This collection of nine papers, on various aspects of economic competition between the Sino-Soviet bloc and the West, by members of the Economic Research Institute of the State Planning Commission of the USSR, is of interest from several standpoints. First, it sets forth the Soviet view of the nature of economic competition and economic relations between East and West during a period of "peaceful co-existence." Second, it shows the criteria and indicators used by Soviet economists in their international comparisons of economic power and performance, and presents estimates of the present and future relative positions of East and West in regard to levels of national product and industrial and agricultural production. Finally, it reviews Western, principally U. S., literature on Soviet economic growth and economic comparisons of East and West.

The papers in the collection vary considerably in scholarly purpose and quality. Several are primarily propaganda efforts intended to assert the superiority of Soviet socialism over Western capitalism and to "prove" such Soviet propaganda themes as "the increasing exploitation of American workers by militaristic monopolists." On the other hand, a number of the papers are serious studies worthy of attention by Western students of Soviet economics. The remainder of this review is devoted to comments on the three papers likely to be of greatest interest to Western readers.

Probably the most interesting paper in the collection is the study by Alekseev and Bogomolov of comparative levels and growth trends of industrial and agricultural production in the Soviet Union and in the United States and the major Western European countries. Unlike most Soviet articles on this subject, which merely present the results of unexplained calculations, Alekseev and Bogomolov defend their estimates by providing a fairly extensive discussion of the choice of indicators for comparisons, the derivation of estimates of present comparative levels, and the assumptions underlying their forecasts of future relationships. Thus, for example, their conclusion that Soviet industrial production will equal that of the United States on an aggregate basis by

about 1970 and on a per capita basis by about 1975 rests on an estimate that in 1955 Soviet industrial production was 42-44 per cent of the U. S. level; on the assumption that recent high rates of growth of Soviet industry will continue while U. S. industrial production will grow at less than half the Soviet rate; and on population projections to 1975.

In his paper on Sino-Soviet-bloc economic relations with nonbloc countries, Alkhimov offers a rather frank explanation of the bases on which the bloc has promoted its trade with the underdeveloped countries in the past few years. He emphasizes the following points: (1) Bloc exports to the underdeveloped countries do not cause payments problems for the latter because the bloc accepts payment in their usual food and raw materials exports, ordinarily through bilateral agreements. (2) The bloc explicitly endorses the aspirations of these countries for industrialization and offers them machinery and complete factory installations for their industrialization programs. (3) The bloc provides a market for the exports of underdeveloped countries, which they are often unable to sell on the world market, and offers them price stability through long-term trade agreements.

The review of "bourgeois" economic literature on Soviet economic growth and East-West comparisons by Barabanov and Churakov is on a less scholarly and more polemic plane than the Alekseev and Alkhimov articles. Virtually all of the leading American experts on the Soviet economy are denounced for refusing to accept official Soviet statistics on the growth of Soviet national income and industrial production and for preparing alternative, lower estimates. On one level, they are labeled servants of American monopolies, because their research has often been supported by funds from the government, considered to be dominated by powerful monopolies, or by "foundations of the great billionaires," such as the Ford, Rockefeller, and Carnegie Foundations. On another, more scholarly level, they are criticized for applying capitalist concepts such as entrepreneurial profits, interest and rent to the Soviet economy and for misinterpreting the nature of the Soviet turnover tax, erroneously treating it as an indirect tax, rather than as a form of income to the state created in the production process. Finally, in attempting to explain Soviet economic growth, the "bourgeois" economists are charged with overlooking the most important factor of all: the manifest superiority of the socialist mode of production over capitalism.

MORRIS BORNSTEIN

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## Statistical Methods; Econometrics; Social Accounting

*Studies in Linear and Non-Linear Programming.* By KENNETH J. ARROW, LEONID HURWICZ AND HIROFUMI UZAWA, with contributions by others. Stanford: Stanford University Press, 1958. Pp. 229. $7.50.

The year 1958 was a memorable one in the development of the literature of the important and rapidly advancing subject of programming. First there was the publication of a new classic, *Linear Programming and Economic Analysis*

by Dorfman, Samuelson and Solow which consolidates, systematizes and expounds the many advances in this field in recent years. Then at the end of the year there appeared the volume under review which offers a series of technical papers in which new theoretical results and applications are reported upon.

The emphasis in this volume of studies is on mathematical problems rather than on applications and it is directed to the specialist who is well prepared in the mathematical aspects of programming theory. The introduction by Arrow and Uzawa is a masterpiece of succinct exposition; it places the contributions of the essays in perspective and summarizes the results. Even the nonspecialist who has some appreciation of the mathematics of the subject will be able to gain an impression of the advances made in the book from this introduction and summary. Of the main body of the book about 45 per cent is devoted to existence theorems, another 30 per cent to the gradient method for solving constrained maximum problems by successive approximation and the remaining 25 per cent to solutions of particular problems of linear and nonlinear programming.

The problem of linear programming is to maximize a linear function $f(x)$ where the vector $x$ has $n$ elements, subject to $m$ linear inequalities $g_h(x) \geqq 0$ (where $h = 1, \ldots, m$) and subject to the requirement that all components of $x$ be nonnegative. It has long been known that the solution to this problem is the same as the solution to the problem of finding the saddle value of the function:

$$\phi(x, u) = f(x) + \sum_h u_h g_h(x),$$

where the $u_h$'s are nonnegative. Kuhn and Tucker in their well-known 1950 paper (published in the *Second Symposium on Mathematical Statistics and Probability*) proved, among other things, that the same equivalence holds if the functions $f(x)$ and $g_h(x)$ are concave and differentiable.

In Chapter 2 of this volume, Uzawa develops a formula by which the vectors that span a convex polyhedral cone when it is represented as the intersection of half-spaces may be expressed explicitly, and uses the formula in new proofs of several of the fundamental theorems on convex polyhedral cones that underlie basic theorems in linear programming theory, and (in Chapter 12) in a new method, on the whole less efficient than the simplex method, for solving linear programming problems. In Chapter 3 Uzawa proves the Kuhn-Tucker theorem under somewhat different restraints than were used originally. In Chapter 4 Hurwicz discusses programming in general linear spaces allowing the number of activities or the number of commodities ($n$ and $m$ respectively in the previous paragraph) to be infinite. He offers generalizations of the duality theorem of linear programming, of the Kuhn-Tucker theorem for concave programming without assuming differentiability of the functions (this work is extended further in Chapter 5 with Uzawa) and of previous results for convex programming( where the maximand is a convex function) that are akin to the Kuhn-Tucker theorem for concave programming.

Existence theorems concerning the reciprocal relation between constrained maximum problems and saddle value problems indicate the possibility of directing attention to saddle value problems in order to obtain solutions to programming problems. Part II of the book is concerned with the study of a particular method of finding extreme values of a function. The method is known as the gradient method. As applied to a very simple unconstrained maximum problem the method may be illustrated as follows. Consider the function $f(x) = b - (x - a)^2$. To find the value of $x$ for which the function assumes an extreme value, set

$$\frac{df}{dx} = \frac{dx}{dt} = 2(a - x).$$

After integrating and recognizing the initial conditions $x = 0$ when $t = 0$, we find

$$\log\left(1 - \frac{x}{a}\right) = -2t,$$

from which it may be deduced that

$$\lim_{t \to 0} x = a,$$

and hence that $a$ is an extreme point of $f(x)$. The gradient method for finding the extreme value of the function $f(x)$ depends on the convergence of the function $x(t)$. Much of the discussion of the gradient method in this book is a discussion of the conditions under which the solutions of the differential equations appropriate to the saddle value problems that arise in programming will converge. Many results are obtained, but perhaps the one of most general interest is that provided by Arrow and Hurwicz in Chapter 8 which, by introducing a suitable modification of the function $\phi\ (x,u)$ yields a method for solving all concave programming problems. In practice the method requires the use of an iterative technique based on difference equations that approximate the differential equations of the gradient theory. Thomas Marschak gives two examples of the application of this so-called modified gradient method to linear programming problems in Chapter 9, and iterative methods for concave programming are discussed further by Uzawa in Chapter 10. In Chapter 11, Arrow and Robert M. Solow prove that two versions of the gradient method applied to the finding of a constrained maximum with arbitrary functions $f(x)$ and $g_h(x)$ will converge to a local maximum.

In the final chapters of the book, three particular problems in linear and nonlinear programming are presented with the object of illustrating how specific characteristics of a programming problem may be used in simplifying the method of solution. A case of commodity speculation involving storage costs or brokerage fees is discussed under the assumption that future prices are known. A second example concerns the assignment of multipurpose machines to a specified series of tasks when machines can be ranked according to capability and tasks can be ranked according to difficulty. Finally Hollis B. Che-

nery and Uzawa discuss the solution to a problem of resource allocation formulated with the aid of a generalized input-output model. The problem and the solution have been presented earlier in this journal (*Am. Econ. Rev., Proceedings,* May 1955, *45,* 40-56) and in *Econometrica* (Oct. 1956, *24* 365-99).

This reviewer commends the introductory chapter to the general, mathematically inclined, reader as a concise summary of the treatment accorded in this volume to problems on the frontier of programming theory. Specialists will regard these pioneering contributions as advances in our understanding of the structure of programming problems and of the techniques for their solution.

WM. C. HOOD

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*The Income of Nations: Theory, Measurement, and Analysis: Past and Present.* By PAUL STUDENSKI. New York: New York University Press, 1958. Pp. xxii, 554. $25.00.

Many things besides wine are improved by being allowed to mature; and so it has turned out with this book. It must be 15 years ago that the first version of the manuscript was submitted to an advisory committee of the Income Conference. At that time it was intended to be a vast compendium of national income information, including concepts and methods as well as statistics, covering all countries and the history of the subject. But the time was not appropriate for such a work; ideas about national income were in some turmoil, national accounting was being developed, and there was much new research on foot. It was apparent that the existing manuscript would become dated rather quickly.

Professor Studenski wisely decided to defer publication. Although he lost the collaboration of Julius Wyler through the latter's ill health, he patiently absorbed the new work that was appearing in increasing volume, and rewrote and polished over the years to give his work more definitive form. As convenient handbooks of current national income statistics began to be provided by the United Nations and the Organization for European Economic Co-operation, he dropped the idea of making his volume a statistical source book and concentrated on the history, theory and methods of national income accounting. Connoisseurs say that 15 years is about right for a fine Bordeaux. After that span of years, Studenski's book has ripened, and the times have become ripe for it. The result is a rich and readable work on national income that is likely to be the standard reference book for many years.

The work is cast on a large scale: 550 double-column pages, relatively few of which are taken up with statistics. It aims to give a full survey of national income work from its beginnings, and indeed all aspcts of the field are treated, though with much difference in emphasis. The material of the book is divided into four main parts dealing with: History; Theory and Methodology; Estimates for Selected Countries; Developments in Sixty-Six Other Countries. Some sections are designed primarily for reference but a surprisingly large portion has real narrative interest.

This is particularly true of the first part dealing with history. It is a history which focuses on the estimators of national income, starting with Petty, but which also covers nonquantitive economists who contributed importantly to developing the concepts of national income and total production. The last 30 years or so, when investigations of national income multiplied enormously, are summarized in a few pages, as was almost necessary. But for the previous three centuries, the history is fully developed. Each writer is taken up individually, a sketch of his life and interests is given, his work is placed in its historical context, and his conceptual ideas, quantitative estimates and estimating methods are discussed in detail.

Some of these writers are names which appear in any standard manual on the history of economic thought, but many are little known and their work, surely, unfamiliar. Studenski has performed a real service in so thoroughly combing the literature and in bringing together the works of these contributors to political arithmetick. For they are a noteworthy group of men who in most cases were thinking imaginatively about the problems of their day. The pure producer of statistical estimates is a late-comer in the field. The earlier investigators had other objectives besides the estimates themselves: to show the monarch how the tax system could be made more equitable and even more revenue-producing; to show how much resources could be mobilized for war; or to develop the policies needed to promote economic growth—and it was just part of the day's work to estimate the national income to help prove the case.

Contemporary workers on national income have had limited interest in these earlier studies, feeling that they could not learn much about their art from estimators who had only scanty data to work with. As someone quipped at a recent conference, I think it was Robert Solow, they either do it the way Kuznets did it or they do it the way Kuznets didn't do it. Studenski's history bears out this hunch; the older writers are no help in solving today's problems of theory or method. But they are interesting in themselves and in their contribution to the development of quantitative economics. I certainly found Studenski's account absorbing.

Part II of the book, on theory and methodology, treats the substance of national income research and the problems encountered in it. It is also very readable, but not as recherché as the history, being pitched, I would say, at about the advanced undergraduate level. Hence, one does not expect the expertise to be found, say, in the report of the National Income Review Committee or in George Jaszi's paper on conceptual problems given to the Income Conference a few years ago. Also, as it is in essence a survey of the field, it puts less stress on tightness of theoretical ideas and the logic of the accounting framework than a book like the Ruggles'. I would have thought these matters important at the graduate level, where the student is firmly on the way to economics as a profession. But to familiarize a wider audience with the subject, Studenski's account is very good.

The first few chapters of Part II are devoted to discussing the basic concepts of national income, followed by a review of the difficulties of fixing the

scope of economic production, and leading on to an exposition of national accounts and input-output tables. The emphasis is more on national income than on national accounting, and all the main problems, from housewives' services to the treatment of government, are taken up. In such matters the author leans to a welfare approach, and his conclusions are to be interpreted in this light. I don't mind this, although I do think it would be well to explain that other aims are legitimate. If one is interested in cyclical changes in economic activity and in econometric relationships, conceptual problems may appear differently than if one is concerned with longer-run changes in economic welfare, particularly if the latter idea is not defined too precisely. And it is this difference in aims which explains the different solutions adopted on many controversial points.

What I do mind, however, is a certain looseness in the discussion of economic welfare. Studenski really agrees that only goods and services produced can be measured, but somehow he wants them to be considered in relation to other factors, such as the quantity and nature of available free goods, as elements in human happiness. He says "A sarong is sufficient in the South Sea Islands, but it would hardly do in the Arctic. A hut made of bamboo sticks and palm leaves provides sufficient shelter in the tropics, but sturdier constructions employing stone or timber are required for shelter in northern regions." But he doesn't say firmly that this bit of anthropology has nothing to do with economics or economic welfare. After all, the temples of Karnak were built in a warm climate, while eskimos live in igloos, and Karnak suggests a level of economic welfare above that of the igloo regardless of what the climate required. Living down the street from Dior for several years may have warped my judgment, but I find it hard to see that the economic aspects of clothing have much connection with climate. My hunch is that when sarongs come to be worn in Paris, Dior will design them; and they won't be cheap; and my wife will consider her welfare raised by having a Dior model. As to happiness, its relation to economic welfare should be seen in the light of the economic law coined by some TV comedian, "happiness can't buy money"—even if the reverse proposition is also considered relevant.

The next section of Part II deals with intertemporal and international comparisons, in which are included a few pages on forecasts and projections. The construction of time series of real national income or output is treated too briefly to really give the reader the feel of index number problems; nor are the references to the literature adequate. The discussion of forecasts is rather better but also too brief and somewhat confusing. For example, the author is sceptical of longer range forecasts because the predictions of a postwar slump made towards the end of the war did not materialize. The point is, however, that these forecasts were short-term in nature—short-term from the time the war would end and military expeditures would be cut. The longer-term projections made at that time, designed to show the economic potential of the postwar period, proved to be on the right lines.

Most of this section deals with international comparisons which are treated very well. The work that has been done on absolute comparisons is first dis-

cussed and then an analysis of relative comparisons is presented. In the latter, Studenski groups the countries for which data are available by approximate income levels and analyzes the relative composition of their national products by industry of origin, by distributive shares and by categories of final output. Various marked tendencies emerge and the whole discussion is interesting and provocative.

I may use this section to reproach the author for taking the easy way on various controversial problems, by calling for more research instead of penetrating the controversy more deeply and baring the real complexity of the problem. In international comparisons, for example, he presents the work of Gilbert and Kravis quite adequately. Then he says we did not give due importance to the complicating factors in making international comparisons (without saying what they are), and cites Nicholson's disagreement with our work (without saying what he disagrees about). Then the author says that while our approach is promising, more research will be needed "before it can be accepted as offering a definitive solution to the methodological problem of international comparisons." Now the essence of Nicholson's critique is that identical tastes cannot be assumed as between countries and, therefore, that an index number solution to the comparison is inadmissible. And surely we were at pains to make clear the ambiguity inherent in a wide index number spread. Hence, while further research can improve the numbers in some sense, it would have helped the reader's understanding to bring out the fact that this is one of those problems not susceptible of "definitive solution." And there are others about which the same can be said.

The final chapters of Part II are given to a discussion of estimating methods. This has always seemed to me a rather boring subject for anyone not forced to be concerned with it, but Studenski manages to make it livelier than usual while at the same time giving the reader a good idea of what it's all about. Classification problems are also dealt with and in a few places I am afraid the reader could be left in some confusion as to whether the point being made is a matter of classification, of definition or of estimating technique—for example, in the section on interest.

In Part III the author has selected the estimates of 13 countries for detailed review with the aim of showing how the various concepts and methods are being used in practice. His sample includes both industrialized and underdeveloped countries, countries with different richness of statistical sources, some whose estimates cover a broad accounting system and some a very limited one, and two countries, Russia and Yugoslavia, that use the Marxian concept of total production. For each country he gives a brief history of the estimates, an outline of the basic concepts and the general methodological approach used, and a description of the sources and methods employed in constructing the various estimates. As an average of about 10 pages is devoted to each country, the descriptions are reasonably full; moreover, as a whole they fulfill the author's purpose of showing the variety of present practices and of making the preceding discussion of concepts and methods more concrete.

One must say, alas, that time moves on. Thus, while the procedures covered are accurately described, at least for the countries I know fairly well, some of them have been superseded in the time since Studenski had to finalize his manuscript. For Germany, for example, he describes the estimates that appeared until 1957, which were extrapolations from prewar base figures, while now there are base estimates constructed from postwar statistical sources. For France, the national accounts have been thoroughly recast and a new territorial basis of aggregating has been introduced. For the Netherlands, most of the space is devoted to the prewar estimates, and the brief section on more recent work hardly does justice to the present Dutch national accounts, either statistically or conceptually.

Part IV gives a brief account of the national income work in 66 other countries. It covers the history of the estimates and gives a picture of the scope of the work being done currently. The sketches should be quite useful for reference purposes, as should the bibliographies, particularly as they cover years prior to those available in the bibliographies of the International Association for Research in Income and Wealth.

All in all, I am happy to compliment Studenski on his success with a difficult and extensive job.

MILTON GILBERT

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*China's Gross National Product and Social Accounts, 1950-1957*. By WILLIAM W. HOLLISTER. The Center for International Studies, Massachusetts Institute of Technology. Glencoe, Illinois: The Free Press. Pp. xxv, 161. $4.00.

This study presents an estimate of mainland China's gross national product during the first eight years of Communist rule. It is the result of a painstaking effort and the first report of its kind that has been published in book form for general consumption. To those who have long awaited such a study, it is a very useful reference and in parts, such as the discussion of budgeted and completed construction expenditures, highly informative.

The nine chapters of the book are devoted principally to a step-by-step description of the methods used in arriving at the estimated final sales of the sectoral accounts. The economy is divided into four sectors, including the agricultural and nonagricultural business sectors, the government, and the household. From the sectoral estimates are then derived the gross national product divided into the usual Department of Commerce categories of personal consumption, gross domestic investment, net foreign investment, and government purchases. Also presented are estimates of distributive shares and of the sources of savings for financing investment. Finally, there is a comparison between U. S. and Chinese gross national products. While the author employs 1952, and, to a lesser extent, 1955, as the benchmark throughout, estimates are also made for the other years, both at 1952 and current prices.

According to Hollister, China's gross national product, at 1952 prices, in-

creased at an annual rate of 12 per cent from 55.02 billion yuan in 1950 to 77.06 billion yuan in 1953, and then at a slower yearly rate of 7 per cent to 102.42 billion yuan in 1957. While the rate of increase was lower during the first five-year plan than in the period of rehabilitation, the entire seven-year period nevertheless witnessed an increase of 86 per cent in the gross national product or of 62 per cent on a per capita basis. A major factor underlying the advance was undoubtedly the high rate of gross investment which rose from a mere 9 per cent of the gross national product in 1950 to the magical 20 per cent in 1957 that, according to some economists, though not necessarily also Hollister, is almost the certain sign of a successful "take-off" into sustained growth. Yet all this was essentially a "bootstrap" operation, for the net inflow of foreign capital was not only negligible for the entire period, but was actually negative during 1956 and 1957.

The concluding chapter deals with the problems of differences between the United States and China with respect to the "product mix" and pricing structure. Then, almost as an afterthought, Hollister concludes: "These valuations serve as a measure of the relatively small capability of the Chinese economy for producing and using a wide range of goods, for adding the complex equipment required to increase rapidly the productivity of the Chinese economy, and for adding complex weapons and equipment needed to modernize the military establishment of China" (p. 148).

These are highly relevant observations and few will take exception to them as statements concerning past performance. But they would be even more informative if Hollister had provided us with some basis of comparison with the pre-Communist past, say, the gross national product of 1933 of which estimates are available. Again, March 1958, as the data cut-off date, fell unfortunately just before the launching of the commune movement and the "great leap" forward of 1958 in both industrial and agricultural production. An arbitrary cut-off date is inevitable in such studies. But one wonders whether the impression one gets of the composition and size of China's gross national product from the present study may not have to be modified radically when developments in 1958 are taken into account. Not the least important is the possibility of a better assessment of the rate of investment and the prospects for sustained or even accelerated growth.

The area of disagreement between Hollister and others will probably be drawn most sharply around individual magnitudes. Considerations on this score should really be preceded by a detailed discussion of the sectoral accounts. Since this cannot be done here for obvious reasons, we shall confine ourselves to raising a few moot questions, not so much as criticisms of Hollister's contribution but as points of possible interest for further thought.

For instance, by far the largest share of the GNP is still accounted for by agriculture (39 billion yuan in 1952 out of a GNP of 67.86 billion). This is an area where estimates of physical production may be seriously inaccurate and the danger probably lies primarily in underestimation of earlier outputs. An adjustment of the 1952 production of food and industrial crops from 146 to 157 million tons of "fine grain" is made by the author. The interpretation

of the term "fine grain" itself seems to contain still an element of conjecture. But principally one wishes that one were told more about the reasoning and the procedure followed. The same point may be raised concerning the construction of the various volume index numbers for the subsectors under "agriculture."

The author points to the possibility of "overpricing" of industrial products in China in the beginning of the study (p. xvii), but makes little mention of it thereafter. One wonders how this has been taken into account and whether, for instance, the official adjustment of many industrial prices in 1956 has affected the estimates. Since estimates of retail sales are a basic prop in the nonagricultural business sector, what about the rise of sales on the "free markets" following the emergence of many unauthorized traders and "manufacturers" in 1957? The absence of a fuller discussion of foreign transactions may be unavoidable, but is nevertheless to be deplored. There is no estimate, for instance, of unrecorded trade, including the compulsory export of labor service. While one may agree with the small size of Soviet credit, that the net inflow of foreign capital was negative in 1956-57 is slightly questionable. If the period studied were carried further, in view of the added number of development projects undertaken with Soviet assistance, the picture portrayed may become different. At any rate, the reader would do well to realize that the importance of imports from abroad far exceeds their quantitative measurement in national income accounting. Finally, we may mention the relatively unsatisfactory state of estimation concerning personal savings and one suspects that there may be some underestimate of rural savings.

Some of the preceding remarks probably stem from the fact that the book is essentially a summary report. While omission of working tables and explanatory appendices is unavoidable, the curious reader is sometimes at a loss to discover whether certain statements represent entirely arbitrary assumptions or are based on facts and reasoning not disclosed. All this is no adverse criticism of Hollister's efforts. It only means that much more work remains to be done in some of the sectoral accounts now that we have Hollister's work as a very good start.

YUAN-LI WU

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## Economic Systems; Planning and Reform; Cooperation

*Nationalisation in Britain: The End of a Dogma.* By R. KELF-COHEN. New York: St. Martin's Press, 1959. Pp. x, 310. $5.50.

As Mr. Kelf-Cohen's subtitle suggests, we have come to "the end of a dogma" on nationalization. The evidence of the past twelve years in Britain does not support the claims of earlier socialist writers and politicians, nor does it suggest that an extension of nationalization by the Labour Party should be taken lightly.

It is unfortunate, however, that a book attacking dogma should itself lapse into dogmatism. The chapter on coal is a case in point; it is a steady review

of labor disputes, absenteeism, shortages of output, unsatisfactory productivity, deteriorating quality, and massive financial outlays, unmarked by any rays of hope. When he discusses transport, electricity and gas, the author is more restrained. It is not at all clear that the National Coal Board has done a poorer job in facing its immense problems than, let us say, the Electricity Authority has done in facing its more routine ones. The failure of the Coal Board has been a failure in solving a human and geological problem that may have been insoluble.

By using as his main criteria of performance the unrealistic socialist claims he is attacking, Kelf-Cohen avoids the questions of serious interest to social scientists. Public ownership may or may not be a means to the improvement of an industry's performance; it should be weighed carefully against the alternatives available. Such an evaluation requires a temper different from that involved in writing a political tract. It requires an ability to apply realistic judgment to the problems at hand. Kelf-Cohen's views on human relations are hardly superior to those of the Fabians he attacks. His main prescription for the coal industry seems to be "more responsibility" on the part of the miners (p. 52) and a greater effort on the part of the National Union of Mineworkers to educate its membership to such responsibility (p. 211). A closer study might indicate that the Union has gone about as far as it can without becoming a part of the "them" against which the miners intuitively rebel.

The book, written by a former government official concerned with the nationalized industries (the reader might appreciate fuller biographical information, including an account of how the author turned from support of nationalization to its condemnation), contains interesting details not available elsewhere. The best chapters are those dealing with the relation of the nationalized industries to Parliament and the Ministries. But even here, Kelf-Cohen fails to make a convincing case for his advocacy of greater centralized control by the government and less autonomy for the Boards. He seems to believe that it would be appropriate for the government to issue a directive that the Coal Board stop producing smaller coal (p. 282), without asking whether the cost of such a move would justify the investment involved. He also suggests that the Minister of Power might issue a directive to the Area Electricity Boards to improve their load factors (p. 283), even hinting that the American success with air conditioning might be applicable to the British climate. Perhaps it would be just as well to delegate such decisions to those closer to the problem.

The author complains that there is little general literature on British nationalization (p. 303) and asserts that his book is the first substantial effort to interpret the annual reports and other primary publications available. His bibliography suggests that he is not sufficiently familiar with what has been written to make this claim—he lists no American publications on the subject. The book cannot be recommended to the serious student of the subject of nationalization, except as a source of illustrations and details not available elsewhere. Nor can it be recommended to the general reader without reference to other points of view. The book might serve as an antidote for extreme na-

tionalizers but will probably instead make them suspicious and more dogmatic than ever. The author of the book simply fails to achieve the careful and dispassionate review of events that he set as his objective.

W. W. HAYNES

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*Economics and Social Reform*. By ABRAM L. HARRIS. New York: Harper & Bros., 1958. Pp. xvi, 357. $5.00.

One of the unsettled questions in economics is the extent to which economists should take sides on social issues. It could be argued that economists are most correctly and usefully practicing their craft when they provide only the optimum solutions to problems set by the public, the government, or some interest group. Alternatively, it can be argued that economists, being more scientifically trained to recognize social, political, and economic maladjustments, ought to be entrusted with setting the goals, as well as the pathways, to their fulfillment. In any case, whatever contemporary economists may think they ought to do, the fact is that their professional ancestors have usually been concerned with social issues, whether or not they have made explicit the programs for social reform which have led to or sprung from their economic analysis. We should all know by now, certainly since the appearance (in English since 1954) of Gunnar Myrdal's *The Political Element in the Development of Economic Theory*, that economics has never been neutral.

Although philosophic value judgments, class loyalties, and reform programs have been implicit in the works of most economists of the past, few economists have specified explicitly a program of social reform, probably on the assumption—or presumption—that economics could be fully value-free. Hence in choosing among systems of social reform set forth by economists, Professor Harris, of the University of Chicago, naturally has been led to the ready-made programs of John Stuart Mill, Karl Marx, Thorstein Veblen, John R. Commons, Werner Sombart, and the Catholic corporate model of Heinrich Pesch, all of which, but for that of Mill, lie outside the main stream of traditional economic analysis, and none of which, again with the exception of Mill and of Marx, are primarily concerned with economic theory per se. This is no criticism of Harris, who has written an interesting (even if sometimes repetitious) and valuable book. Still one would like to know if there was a basis for selection among the social reform programs of various economists other than that of ready accessibility. Further, one would like to have had from the author a systematic analysis of the historical or logical relations between economic theory and social policy as propounded by his subjects.

All of the social reform programs discussed are examined against the background of nineteenth century liberalism, with John Stuart Mill (1806-73) as the "guiding spirit," much admired by Harris, less for his capacity as a technical economist than for his "social philosophy which embraces most that is good in our nineteenth century libertarian inheritance" (p. xv). Nearly a third of the book is devoted to Mill, and this is natural enough, given the volume of Mill's work, the evolution of his thought—he abandoned much of

classical economics and became more socially radical as he grew older—and the interconnection of his economic theory (itself full of contradictions) and his social philosophy.

The essential features of Mill's social philosophy discussed in this book are: (1) his advocacy of a kind of voluntaristic, cooperative, *laissez-faire* socialism, still based on competition, (2) his specific reform measures, such as those pertaining to inheritance, land reform, education, and labor legislation, (3) his views on the progress of class relations and the role of the trade unions, and (4) his theory of economic progress, in which he expected social improvements (including mass education and the emancipation of women) and moral and aesthetic elevation to be combined with the stationary economic state. In conclusion, Harris points out that Mill was essentially a gradualist in economics and an idealist in philosophy.

Considerably less space and sympathy are spent on the social reform program of Karl Marx (1818-83). The author has serious doubts about the realm of freedom in Marx's classless utopia, which is to issue from the proletarian revolution and abolition of capitalism. One quite rightly can wonder about the source of decision-making and the extent of freedom to be afforded in a society in which the economy is at once organized and directed by workers in the individual factory and by a central economic plan. And certainly Marx was wrong in his estimate of the viability and flexibility of the capitalist economy and the timing of the socialist transformation. This is not emphasized by the author, although it is indicated that Marx was quite unrealistic in pointing to the Paris Commune as the model for future socialist organization. But, as the author says, Marx was not concerned primarily with blue prints for the future, and despite the volume of his philosophical and political writings he was essentially an economist, a theoretician of the *capitalist* economy.

It is in dealing with Marx qua economist that, in the opinion of this reviewer, Harris slips, especially in his assertion that Marx missed the crucial problem of investment, as postponed consumption, because "(1) he did not have a theory of interest and (2) his whole theory was based on the erroneous labor theory of value" (p. 131). Theories of value can not be proved true or erroneous; rather are they useful or not useful conceptual models. And as for Marx's theory of interest, he did not have a time-preference theory of interest, as Harris would prefer, precisely because, according to his theory of value, interest was simply the price of money (as in Schumpeter) paid out of the surplus value whose origin the labor theory seeks to explain. As regards investment, it would be hard to find another nineteenth-century economist who saw it more clearly as the crucial problem in economic development.

The extensive treatment of Veblen (1857-1929) and Commons (1862-1945) is especially interesting in view of the fact that these writers, both institutional rather than theoretical economists, are not widely read, except for some of Veblen's more popular sociological works, such as *The Theory of the Leisure Class*.

Harris appraises Veblen's program of economic and social reform within the framework of Veblen's (1) theory of history and social change, (2) his

critique of orthodox economics and of business institutions, (3) his conception of scientific method in economics, and (4) his explanation of the relation of social institutions and habits and the organization of knowledge. Veblen's work is considered "as intended to supply 'scientific' arguments in support of a program of industrial reorganization and reequipment under the guidance of technicians" (p. 157). For Veblen, who opposed traditional economics as a system of apologetics and who saw a persistent tendency to declining profits and chronic depression after 1870, due to continuous technical change which outstrips the market, the appropriate remedy was essentially that of replacing the "absentee owner" with the rational engineer. But, as Harris indicates (p. 188), Veblen completely confused the nature of economic decisions with technical know-how. Further, he points out that Veblen's hope for an ultimate return to the alleged peaceful, primitive, savage mentality, as opposed to that of competitive capitalism, was based completely on fantasy.

Commons, on the other hand, although equally critical of abstract economic theory, saw maximum efficiency and welfare not in giving greater authority to the engineers but in "widening the sphere of collective action of bargaining groups, such as the trade union and the business corporation, and at the same time expanding the economic powers of the state and federal legislatures" (p. 215). Commons' theory of collective democracy based on collective action was developed from his theory of legal evolution and of industrial change. This combined legal and historical economic approach makes Commons a unique figure in the history of economic thought. He can be compared with Marx in his theory of the "stages" of capitalism, with Veblen in his institutional approach. But his emphasis on law, and particularly on what Harris believes to be two fundamental misconceptions regarding the importance of the common law, is quite individual. It is perhaps the complexity and confusion in Commons' own theory that has led to what seems a disproportionately long discussion on law in this volume.

Harris' discussion of the economic theory and social reform program of Werner Sombart (1863-1941) takes us far from the libertarian ideals of the nineteenth century because Sombart was, par excellence, the theorist of twentieth century German fascism. Sombart, as is generally known in the profession, was long considered the theoretical expert on the subject of capitalism, and to the end insisted that his contribution to the history of economic thought drew most from the analysis of Karl Marx. Indeed much of Sombart's work, his theory of evolution, of the "stages" of capitalism (from early to "high" to late), and his emphasis on the relations of technological and ideological developments has a Marxian ring. But the fact is that Sombart was even more influenced by nineteenth century German romanticism, with its stress on *volk* and *geist*, and in the end turned systematically anticapitalist (capitalism being in his mind identified with Judaic tradition, talent, and interest). Happily enough for those of us who are not blinded by the gold of liberal tradition, and/or prejudice, Harris does in this chapter on Sombart bend over backwards to be fair to a social reform program for which he has no preference. It remains one of the ironies of intellectual history that Som-

bart remained outside the political movement for which his own theory, in part, provided justification.

The last chapter in Harris' book is devoted to the reform program of the Catholic corporate state, best explained in the writings of Heinrich Pesch, and formalized in the papal encyclical, *Quadrogesimo Anno* (1931). Pesch's point of view, elaborated in a series of works, of which the *Lehrbuch der Nationalökomie* (in 5 volumes, which appeared 1905-23) is the most important, provided the basis of the Italian corporate state, although like Sombart, he was much influenced by liberal, even radical ideas, and never would have justified the social and political brutalities of fascism. Pesch's program, an alternative to Marxian socialism, calls for joint control of industry by labor and management together with the government, in the interests of "just" prices and distributive shares, and the harmonization of private and national interests. But, as Harris points out, Pesch's futile attempt to rehabilitate scholastic value theory, his confusion between motivations in the medieval economy with those of modern capitalism, between interest and profits, and between money and capital all militate against his being taken seriously as an economist.

Economic literature is enriched by this stimulating discussion of some of the major social theorists of the last century who were, or considered themselves to be, economists. But one is somewhat disappointed that in this volume some of the crucial questions in the relation of economics to social reform programs are only implied rather than clearly stated. Without clear and appropriate questions we cannot have clear answers. Does the author believe that economics is a normative science, (or a science at all)? Should it be normative? If so, by what criteria? Perhaps the author will give us his view at a later time. Meanwhile, we are grateful for his discussion as it stands, certainly new to the literature, thorough and readable.

BERNICE SHOUL

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## Business Fluctuations

*Economic Forecasting*. By V. LEWIS BASSIE. New York: McGraw-Hill, 1958. Pp. ix, 702. $8.75.

Against a background of many books in the general field of business cycles and forecasting published during the past dozen years, this volume stands in marked contrast. The usual orientation for texts in forecasting has been associated with problems of the firm and, wherever the emphasis has been on aggregate changes, it has been accompanied by a comparative deficiency of detailed analysis and an overabundance of generalized statements. Bassie's book is new by more than its date of publication; it contains a refreshing and stimulating approach to a forecasting of the economy's income, production, employment, and price levels. Obscurities concerning specific methods for the application of forecasting procedures, almost invariably unexplained in previous books, are granted far more adequate treatment in this one by means of detailed explanations and illustrations.

The first main division, titled "The Essentials of Forecasting," is introductory, designed to acquaint the reader of comparatively little background in economic and statistical analysis with the prospects—and limitations—of analytical forecasting. Part II, under the heading "Expenditure Income-Flow Approach," forms the heart of the volume. The third part is devoted to several problems encountered at governmental, industry-wide, individual firm, and personal levels. Similar treatment of the material covered in this section can be found in several other works, and the unique quality of Bassie's text stems largely from its second part. Let us, therefore, look at the composition of this section in further detail.

Part II begins with an identification of what Bassie calls the "strategic points of attack"—a particularly apt phrase by means of which one is able to discern shortly both the general economic orientation of the author and the identity of his specific points of departure for prognostication of the economy's income changes. The ensuing chapters cover each of the strategic spending areas (government, consumer, and domestic and foreign investment) in extensive detail.

An interesting idea and, as Bassie develops it, a useful one for forecasting purposes as well, involves a consideration of the construction and capital-expenditure cycles as special kinds of inventory cycles. By means of a modified inventory-cycle approach, he is able to show why spending for residential construction is cyclical in character and also why this cycle is comparatively long. He clarifies what might otherwise be several obscure points in the analysis of capital expenditures. Nor does the cycle of consumer expenditures for durable goods go unnoticed. He maintains that consumer-credit variations have inventory-change characteristics in that "when a reversal takes place the total swing from accumulation to liquidation represents the appropriate measure of its effect" (p. 388), and he employs this as a point of departure for consideration of the most dynamic part of the great mass of consumer expenditures. The cyclical generating qualities of the several expenditure areas are well highlighted by this central device.

Such minor shortcomings as exist in this volume are for the most part in the writing style and are not substantive in character. Though the chapters are concisely organized there is a decided lack of highlighting of important points in the paragraph-by-paragraph presentations. Student reaction so far encountered by this reviewer has been uniform in its lack of success in singling out the substantive high points of the text. Perhaps this is a comparatively minor point but, despite the author's hope that the main topic of the book might, with the help of a partial review of elementary economics and statistics, be made understandable to the reader not trained in the field, rather close supervision will be required where this book is used as an undergraduate text.

An additional, though still minor, shortcoming of the book is its exclusion of statistical data beyond 1954. Many of the more interesting cyclical problems to which Bassie's analyses are applicable have developed since that year and reader interest would doubtlessly have been heightened by more extensive

reference to the growth years 1955-1957. The book's publication date, of course, precluded any significant treatment of the 1957-1958 recession.

Bassie is to be congratulated on a book which evidences extensive originality in research and which should serve to fill a place vacant until now in text material for general forecasting. With growing interest in aggregate income analysis in courses offered at the undergraduate level, this volume should receive wide attention.

J. WHITNEY HANKS

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*Business Forecasting* By ELMER C. BRATT. New York: McGraw-Hill Book Co., 1958. Pp. viii, 366. $7.50.

This work presents a logical and thorough development of business forecasting. The author begins with a statement of the forecasting problem as it confronts the business economist and the executive, and then proceeds to outline the phases of this problem from the general forecast of total spending to the specific forecast of company sales.

Professor Bratt has apparently read widely over a period of years in preparing for this book. The coverage is excellent, and it may be turned to with profit for a number of purposes in forecasting. An analyst concerned with forecasts of the general economy can obtain much useful help from Chapters 2 to 6. These discuss the forecasting of total industrial activity and total consumer spending, and then proceed to techniques for forecasting major areas of spending and activity in particular industries. The coverage of industries is excellent. Attention is given as well to the forecasting of gross national product. The author develops his own long-term projection of GNP through 1965 and compares his results with other well-known projections. A discussion of business indicators closes this group of chapters.

Chapter 7 has a comprehensive discussion of short-term forecasting of total business activity. The author relates the history of earlier attempts at forecasting and gives a critical account of the most widely used current approaches. Here, as elsewhere in the book, there is a wealth of material which cannot be absorbed at a single reading but which can be referred to with profit for help on particular forecasting problems.

A chapter is given to the forecasting of commodity prices. The author introduces a host of considerations influencing prices with little attempt to single out the most important or to measure the effects of relevant factors. More examples of actual price forecasts and the methods used would be helpful. This is, of course, an area where there is often small degree of forecasting success.

The chapter on the short-term forecasting of individual industries will provide the beginning industry analyst with information on what is being done currently in the forecasting of demand for steel, automobiles, residential construction, electric power, petroleum products, textiles, and farm products. This is followed by two chapters on sales forecasting for particular companies. These are among the best chapters in the book. A thorough discussion of sales

forecasting methods in use today is followed by statements from representatives of twelve companies on techniques presently used by them.

A chapter on the forecasting of various "economic processes"—labor force, unemployment, population, households, interest rates and stock prices—precedes the last chapter which is given to tests of the adequacy, accuracy and usefulness of forecasts. Here again, the author demonstrates a thorough acquaintance with his subject and discusses criteria for measuring the goodness of forecasts. The forecasting results of various surveys are then reviewed. These include the plant and equipment surveys of the Department of Commerce and the Securities and Exchange Commission; the consumer attitude surveys of the Survey Research Center of Michigan; and surveys of businessmen's expectations.

Bratt has provided a comprehensive text on the techniques and practice of business forecasting. The style is a little dry in some chapters but this is only a mild offset to the book's all-around excellence.

JOHN MCKNIGHT

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## Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

*Policy Against Inflation.* By R. F. HARROD. New York: St. Martin's Press, 1958. Pp. xiii, 257. $4.75.

Discussion of the problem of inflation has reached a crescendo in recent years. It is Sir Roy Harrod's opinion that much of this discussion has been misdirected through excessive attention to short-run expediency, and because of the lack of well-authenticated principles of policy.

His book supplies a retrospect of former systems, and the reasons are set forth as to why chronic inflation under the gold standard was impossible. Much of the book is devoted to reviewing the operation of the 19th century gold standard, the abortive re-adoption of the gold standard in the 'twenties, and the various monetary experiments and expedients resorted to during the 'thirties and 'forties. This extensive historical review is justified in the belief that in currency questions doctrine cannot be divorced from history. Being a realist, Harrod dismisses the possibility of the re-adoption of a free gold standard in the foreseeable future. He therefore endeavors to formulate a set of policy principles to cope with the problem of inflation in place of the principles of the gold standard. In his opinion, if these principles could be set forth clearly (as he has done in this book) and as clearly understood, much of the present irrational fear and pessimism would be removed. It is this irrational behavior that now makes the task of the monetary authorities virtually impossible.

Inflation becomes a problem, as contrasted to merely existing, when there is not only objective evidence of a persistent across-the-board increase in prices, but the public becomes conditioned to an expectation of a chronic decline in the purchasing power of money. Increases in particular prices do

not constitute inflation, nor is there an important inflation problem if price increases should be general but short-lived. It is only when price increases create permanent distortions and inequities, and the use of money as a store of value and investment in money obligations are compromised in the public mind, that the effects become pernicious and the problem becomes crucial.

Again and again Harrod emphasizes that the problem of inflation largely stems from the state of mind which chronic and inequitable price increases create—a state of mind which, if not corrected, becomes disastrously irrational. Until this ultimate loss of confidence seizes the people, however, all is not lost. Harrod is optimistic. In his judgment a clear statement of policy against inflation, an enunciation of the principles involved, and their unequivocal application will prevent a common demand-pull or cost-push inflation from becoming a hyperinflation, and will even stop a hyperinflation from running its full course (when money becomes utterly worthless) if the remedies for the disease are applied soon enough.

Most of Harrod's solutions to the inflation problem are in line with the old gold standard policy, and to many they will suggest a counsel of perfection. When applied to a specific situation, e.g., the 1949 devaluation of the pound, Harrod seems to fail to take cognizance of the world of sticky, administered prices which characterize the modern economies—including that of the United Kingdom. He is very adamant that the proper remedy in 1949 was not devaluation, the proper remedy was deflation. How this could have been accomplished without creating an intolerable level of unemployment is not indicated.

The unrefined outlines of policy which Harrod recommends are these:

1. If a country is suffering from internal inflation and external deficit (like the United Kingdom after the second world war) it should deflate.

2. If a country is suffering from internal depression and external surplus (like the United States in the 'thirties) a policy of monetary ease is advisable, combined with all available measures to stimulate investment. It should, in short, inflate, but not by devaluation.

3. If a country is suffering from internal depression and external deficit (like the United Kingdom in the 'twenties) it should adopt methods for getting costs down which will not accentuate its depression. Realizing that a universal reduction in wages and other charges entering into costs is impractical, Harrod in this instance advocates devaluation.

4. If a country is suffering from internal inflation and external surplus the remedy is to raise wages and costs. Presumably this will have an adverse effect on the terms of trade, the external surplus will be whittled down and the increased imports will reduce internal inflationary pressures.

As a method of correction for over-full or deficient demand, Harrod stresses the value of changes in the quantity of money as an instrument of policy. He prefers this to fiscal policy such as a budgetary surplus. From the former he expects a rather quick effect upon investment orders and also on orders for consumer durables. By contrast a budget surplus may be expected to react rather slowly on the general level of consumption. Furthermore, it is

difficult to plan and apply a timely fiscal remedy. But fiscal policy in the form of a large budgetary deficit would be required to counteract "a really deep slump." He notes that in the United Kingdom the unevenness of private investment can be compensated to a considerable extent by the government's control over investment in the nationalized industries. In the United States we have attempted to accomplish a similar end by varying the extent and terms of the government's debt-guaranteeing and underwriting program for the benefit of the private sector.

As a device for curbing investment Harrod eschews selective credit controls, believing these open the way to administrative caprice. General monetary restriction, he believes, offers ". . . a far finer and more accurate weapon. . . ." By resorting to deflation rather than devaluation to counteract an external deficit he believes that bank rate could be restored to its former important role. If the suspicion of devaluation were exorcized short-term capital movements would respond to changes in bank rate, thus providing a second line of reserve to meet temporary oscillations. This was the situation before 1931 and it would, if reinstituted, greatly diminish the need of the United Kingdom for physical gold reserves. To meet the inflation problem presented by an excess rise in wages and salaries over productivity he believes that a direct approach is necessary. The problem cannot be met by indirect credit controls which may, if used, even aggravate the problem.

On the whole Harrod makes an erudite and even witty presentation of his policy precepts. But this reviewer would have more confidence in his precepts if in formulating his policies he had taken cognizance of the price-push as well as the cost-push and the demand-pull types of inflation. Price-push inflation is perhaps the dominant type in the United Kingdom and the United States at the present time, and seems to be endemic to any economy where the government is committed to using monetary and fiscal devices to counteract the unemployment generated by an administered price structure—a price structure saturated with monopolistic elements.

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*Some Monetary Problems—International and National.* By PER JACOBSSON. New York: Oxford University Press, 1958. Pp. viii, 374. $6.75.

This book is a collection of articles, essays, and speeches by the distinguished Swedish economist, Per Jacobsson, who is presently managing director of the International Monetary Fund. The material brought together in this volume reflects the author's thought and experience during a long and productive career as an international civil servant. In his nearly forty years in active public life Jacobsson has served with the League of Nations and the Bank of International Settlements, as well as the Fund, and has thus been in the closest of contact with practically all the critical problems that have beset the international economy in this period. These eighteen articles and essays were written between 1925 and 1957 and range over such diverse topics

as the burden of armaments expenditures in the 1920's and the peculiar economic and social advantages of small nations to more strictly economic matters having to do with the rate of interest, price-cost relationships, questions of employment and investment, and problems of foreign trade and the external balance. The book is enhanced by the inclusion of a relatively long introductory essay which sketches the historical background for the various articles, and is of value and interest as a concise survey of major monetary and economic developments during the period covered. The book is well-suited as a supplement to any of the standard texts in courses in international trade and finance, and can certainly be considered as "must" reading for graduate students working in these areas.

Any comment upon the importance and significance of particular essays in a volume ranging over such a wide expanse of the recent international scene will obviously be colored by the reviewer's own preferences and prejudices. Thus, in this reviewer's judgment, the articles of most interest are those that touch upon various aspects of monetary and Keynesian theory, as well as more recent developments in the international economy. A few comments on some of these essays may be in order. One of the best articles in the book, "Theory and Practice," is an especially clear and lucid discussion of the monetary ideas of Knut Wicksell, particularly as they concern relationships between monetary and credit policy, the level of investment activity, and the balance of payments. In this essay, Jacobsson severely castigates Swedish monetary authorities for having forgotten the teachings of Wicksell and pursuing in the period immediately following the second world war a cheap money policy more appropriate to the 1930's than the boom years that came after 1945.

The author returns again in the essay "Problems of Employment" to the theme that monetary policies more appropriate to another era are still being followed in too many countries. This article first appeared in 1952 and in it he discusses various ways and means of maintaining employment. In particular, he argues that credit creation will not necessarily have a favorable effect upon employment if there is a "shortage of genuine savings." In some countries, such as Italy or Western Germany, employment opportunities are limited by a shortage of real resources for investment, rather than any deficiency of monetary demand in the Keynesian sense. Concern for a satisfactory flow of "genuine savings"—i.e., real resources—is also the theme of another essay, "The Monetary Significance of Investment." National income theorists will have special interest in the article, "Keynes: Costs and Controls," for in this interesting study Jacobsson sets forth the view that one of Keynes' major concerns in *The General Theory* was to bring about a proper balance between prices and costs. This, according to the author, was a matter that Keynes regarded as equally important for the achievement of satisfactory employment levels as a sufficiency of monetary demand. This will no doubt be disputed by many, although it is true that the price-cost aspects of Keynesian theory have generally been neglected in favor of demand factors.

The above comments can only convey in a very brief way some idea of the

content of the book. The topics covered are quite diverse, but even so there is a coherence or theme to the collection when viewed as a whole. Certain ideas consistently appear and reappear. In a very general sense most of Jacobsson's writings have a classical flavor, for he argues quite forcefully for a revival of monetary policy as a key instrument for the control of economic activity. He is highly critical of the view that monetary policy can or should be superseded by fiscal policy, and argues that much of Western Europe's postwar history is the story of nations relearning this lesson by the painful process of experience. He asserts that "*it has been one of the tragedies in the field of monetary policy since 1945 that ideas born in the depression have contiued to determine policy during a period in which basic circumstances have been completely different.*"

In a way this quotation is representative of the major theme of Jacobsson's book, for he is most strongly convinced that interest-rate policy and other monetary measures retain an importance at least equal to fiscal means for the control of economic activity. In reviewing recent European experience, for example, he argues that monetary measures are the key to a restoration of internal equilibrium, by which is meant a balance between the volume of money and the supply of goods and services; and this, in turn, is the key to a return to equilibrium in the balance of payments. The relationship between internal and external balance is a theme that frequently occurs in the volume, as is his emphasis upon price-cost relationships and the need for flexibility in the latter. All of these views have classical overtones. Jacobsson's faith in the efficacy of monetary policy will not be shared by all economists, particularly in view of the peculiar behavior of prices and costs during the recent recession in the United States, but one cannot disagree very much with the stress on the need for price stability and a sound currency. Much of the world seems genuinely tired of a continuously rising price level; and all too often, it seems to this reviewer, economists have sidestepped this problem by rather glibly assuming that this is the price that society must pay for continued full employment. This reviewer does not pretend to know the answer to this tough problem, but suspects it may lie somewhere in the uncharted jungle of oligopoly and administered prices.

The other part of the book that this reviewer regards as of special importance contains the conclusions drawn by Jacobsson from his long experience and participation in the economic and financial affairs of a great many countries during a highly eventful era in history. The most important conclusion is, perhaps, the most obvious, and yet it is worth repeating over and over again. This concerns simply the abruptness of economic change and the absolute necessity for flexibility with respect to policy. In economic matters, he says, "*one should always be prepared for the unexpected.*" Beyond this he sees the key problem of modern society to be that of finding a formula that permits change and growth in the economy and yet allows for the kind of "mutual assistance" embodied in the modern welfare state. A third important conclusion is that there has been a growing realization in the West of the advantages of a market economy. This has come about to such an extent, he says,

that today in the West there is no great difference between the parties on the Left and Right as to the desirability of properly functioning markets.

Jacobsson's book is eloquent and well written; it sets forth clearly the relatively orthodox point of view with respect to monetary and fiscal matters. It deserves respect because his views are fortified by long and distinguished experience with these matters as an international civil servant. It is an important document for any economist with an interest in these aspects of economics.

WALLACE C. PETERSON

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## Public Finance; Fiscal Policy

*Fiscal Policy and Politics.* By PAUL J. STRAYER. New York: Harper and Brothers, 1958. Pp. x, 305. $4.00.

As the author states in the preface, this book is a study in the field of political economy. It contains no tables or charts and few statistics. The primary focus is upon the efficacy of the various proposals that have been made in the last three decades for stabilizing the economy. Special emphasis throughout the book is placed upon the limitations of the fiscal powers of government.

The first six chapters are devoted to a detailed account and analysis of the growth of public expenditures from the great depression to 1958. The impact of the great depression is discussed in terms of the growing centralization of power in the federal government; the development of widespread fear of another depression; adoption of specific measures to minimize the risk of business failure; continuance of depression measures when the need was no longer so evident; growing skepticism about the effectiveness of the federal reserve controls over money and credit; and the increasing appeal of the views of Keynes, Hansen, and others relative to the use of fiscal tools to maintain "maximum production, employment, and purchasing power."

Among the major effects of the second world war cited by the author are the consolidation of the strength of labor and agriculture; delegation of great authority to the executive and the corresponding weakening of congressional leadership; and the adoption of and strong reliance upon direct controls. A brief discussion of war finance is presented with reference to the increase in federal expenditures, tax policy, and debt management. Attention is called to the failure of Congress to step up its pace in considering tax legislation during the war. This failure of Congress during a great emergency leads the author to suggest that some solution must be found to the political problem if taxation is to be effectively used as a means of correcting short-run swings in the business cycle. Greater emphasis might have been placed upon the political limitations which prevented heavier use of taxation in financing the war. More emphasis might also have been given to the political implications of the social security programs.

Following the background chapters are discussions of the political and

economic framework, stabilization theory as a policy guide, public expenditure policy, tax policy, and state and local tax problems. We are living, the author believes, in a period of revolution in the role of government, and that it will be one of the really great achievements of democracy if this revolution can be accomplished without a radical change in basic democratic procedures. Wide use of monetary and credit policy is recommended in order that the need for frequent use of fiscal action might be minimized. Primary emphasis should be placed, the author believes, upon adjustments in tax rates to achieve the objectives of countercyclical fiscal policy.

Three major areas of tax policy are discussed. The first of these pertains to the problem of equity in the distribution of tax burdens and includes both the question of the fairness of specific taxes and the impact of the over-all tax burden upon income classes. The second area pertains to the relation of tax policy to the stabilization of income, employment, and prices. In the third area, the relation of tax policy to economic incentives and economic growth is treated. The analysis of state and local tax problems leads the author to the conclusion that the ultimate solution must be found in more extensive use of federal grants-in-aid, particularly grants of the differential type.

This book reflects the wide knowledge of the author. It is clearly written and can be read with profit by laymen as well as specialists in the field.

H. Kenneth Allen

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*Taxation of Agricultural Lands in Underdeveloped Economies.* By Haskell P. Wald. Cambridge: Harvard University Press, 1959. Pp. xiii, 231. $4.50.

This interesting volume is a publication of the Harvard Law School International Program in Taxation. It is an outgrowth of the 1954 Conference on Agricultural Taxation and Economic Development held under the auspices of this program, which is part of the Harvard Law School's broad program in international legal studies.

In addition to a painstaking job of marshalling the facts on agricultural land taxation in many underdeveloped countries, Wald sets himself the task of (1) evaluating the principal tax methods and features against the criteria most relevant to the various countries, and (2) deriving a set of practical proposals for land tax reform that can be accommodated to differing country settings, which would also serve as efficient instruments of development financing, i.e., as a means of forced saving for productive investment.

The book is organized into three parts. Part I examines in panoramic fashion the morphology of agricultural land taxation in underdeveloped countries and describes the conceptual and structural characteristics which broadly dominate the various types of land taxes in these countries. Part II deals with basic policy consideration. Specifically, it examines the qualifications of land as an object of taxation and of the land tax as a fiscal instrument, and it analyzes the principal types of taxes from the viewpoint of (1) equity, (2) economic policy, and (3) administration. Part III attempts to show that "a properly

conceived and applied land tax offers a promising solution to some pressing fiscal problems of underdeveloped countries" (p. 180), and might also serve "as an efficient instrument for mobilizing savings for capital formation" (p. 208).

One of the most interesting aspects of this book is the emphasis Wald places throughout the study on the personalization of land taxes. A basic characteristic of land taxes, at least as understood in the United States, is their impersonal or *in rem* nature. Wald argues that a fundamental reason why most present-day land taxes are impersonal levies is that, in effect, they are discriminatory taxes; they single out for special taxation a single source of income or form of wealth (p. 43). To Wald, the view that land taxes fall on property rather than on persons and, therefore, need not take note of the landowner's position is not acceptable as an application of the concept of taxable capacity because land cannot have tax-paying capacity apart from that of its owners. And further, "administrative, economic, and equity considerations argue for a minimum deduction as a tax-free allowance" (p. 206).

The reviewer certainly does not wish to quarrel with the notion of personalizing land taxes, but there does not appear to be much evidence—or prospect—of such a trend in the United States where the *in rem* character of land taxes seems to be very firmly entrenched.

Wald criticises two widely used land-tax practices in the United States: the use of capital value as the tax base and the use of selling prices of land to determine the assessment. He argues, and with much merit, that it "is extremely difficult to construct an equitable and efficient system of land taxation by relying primarily on information on the selling price of land" (p. 197), and he holds, again with merit, that the tax base is largely artificial or hypothetical in the typical case of assessment according to capital value (p. 25).

As to the structure of land taxes, Wald proposes a combination of revenue measures, rather than relying on any one measure alone, including (1) a means of mass taxation on a broad segment of the agricultural population, (2) a more exclusive tax applying to the wealthier recipients of agricultural income, (3) one or more taxes designed to tap windfall incomes, and (4) special taxes for regulatory purposes (p. 183).

With respect to general improvements of land taxes, three broad sets of recommendations are made by Wald: (1) reform the tax base and improve the methods of assessment so that the tax rests on a sound land classification system and accords as closely as possible with presumptive net income; (2) make the tax yield responsive to changes in prices and production; and (3) relate the tax liability, to the extent practicable, to the personal circumstances of the taxpayer (p. 208).

Wald's presentation is careful, thorough, and systematic, giving attention to both administrative problems and economic policy considerations as well as the finer theoretical points. The book is a valuable addition to tax literature with much significance for general tax policy as well as for underdeveloped economies.

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*Problemi fiscali del mercato comune.* By CESARE COSCIANI. Milan: A. Giuffrè, 1958. Pp. vii, 136. L. 800.

This book, dealing theoretically with the problem of harmonizing tax structures and legislation among the six countries of the European Common Market, will be of interest to both the Italian and the non-Italian reader. Both will recognize that in this monograph "fiscal problems" refers only to some aspects of direct and particularly indirect taxation. Harmonization means equalization of tax burdens without distortion of trade and investment.

To the Italian reader, Professor Cosciani offers a survey of measures contemplated, and to some extent adopted, by the European Coal and Steel Community, the Benelux union, and parenthetically, by the abortive Franco-Italian customs union and the European Defense Community. But the author's primary concern is in the broad stipulations of the Common Market treaty, to which the major part of the text is devoted.

The non-Italian reader may be familiar with some of Cosciani's points from other (relatively few) publications which have appeared on the subject since 1953. He will be impressed by Cosciani's doubts as to the possibility of any, except an extremely gradual, harmonization. Yet, the goals of the Common Market, and promotion of intraregional competition in particular, cannot be achieved unless the participating nations attune their tax regulations. Disparate internal revenue systems may interfere with international trade as much as import and export restrictions.

A substantial portion of the author's analysis is focused on the "destination" and "origin" principles explaining the influence of indirect (sales, turnover, excise) taxation on costs as well as prices of international goods. According to the former principle, goods with the same destination bear the same tax in the importing (consuming) country, and thus compete with domestic products on equal tax terms. (Country A's exports to country B are exempt from A's tax and subject to B's tax, although country A may levy a compensating tax on its imports.) The "origin" principle postulates that goods of the same origin pay identical tax in the country of origin or production, regardless of destination. While the exporting country's imports are tax-free, it may impose tax refunds on exports. The importing country may use a compensation levy. Due to compensating taxes, refunds, exemptions and levies imposed at different stages of the production and commercial processes, neither of the two principles is simple of application in practice. It is, in fact, customary in Europe that on imported goods duties are imposed to compensate for internal taxes to which domestic production is subject and that, on the other hand, refunds are given on exported goods to minimize the burden of internal taxes. The arbitrary nature of turnover and other tax regulations makes it well-nigh impossible to determine what the tax burdens actually are, and thus makes difficult their international comparison. This leaves the problem of harmonization pretty much insoluble.

Another point which the author examines is whether Cassel's purchasing power parity theory may furnish a method for reduction of the tax differentials which account for international price differences. He looks to the

foreign exchange mechanism and changes in terms of trade for an automatic compensation of differences in fiscal pressures. But after he has listed the well-known limitations on the applicability of that theory, Cosciani arrives at the realistic conclusion that harmonization of tax systems must be sought through negotiation and multilateral conventions. Not all students of the subject have reached this conclusion yet.[1]

Cosciani emphasizes that it is futile to expect individual nations to give up autonomy in fiscal matters with all that this would mean not only in economic but also political and social terms. Revision of tax systems, even for the sake of regional economic unification, is a very difficult task, especially in this age of functional finance. Each nation has its own standards as to what constitutes a desirable tax structure to suit its economic organization. Cosciani maintains that fiscal harmonization must be preceded by social, economic, and possibly political harmonization. He apparently sets these conditions for an over-all, general harmonization, because surely harmonization (perhaps even equalization) of particular taxes may be attained with less effort.

The greatest difficulties facing harmonization will arise from the varying dependence of the six Western European countries on different types of taxes. For example, while in Luxemburg 66 per cent of total revenue comes from the income tax, Italy obtains only 32 per cent of total revenue from this tax. In this latter country as much as 47 per cent of total revenue originates in consumption taxes, whereas in Luxemburg the percentage is only 18. France depends on turnover taxes to the extent of 42 per cent, but in Luxemburg the proportion is much smaller, 15 per cent. The question may be asked whether the process of harmonization should be begun by urging uniformity in internal revenue systems. However, this is unfeasible in view of the basic differences (including those explained by historical and psychological factors) in the structures of the six economies.

Like other writers on the subject, Cosciani confines himself to general analysis and general conclusions. He has missed the opportunity of showing what would be the scope, potentialities and limitations of his own country's adjustment to fiscal harmonization. His readers would have appreciated a more detailed appraisal; they will find superficial his recommendations, made in "Concluding Considerations," that Italy change its tax laws to conform to the Common Market treaty, that direct taxation be increased, indirect taxes revised, etc.

Valuable as is Cosciani's contribution, this book is likely to confirm the view held by many that the Common Market idea owes much to its political appeal, since it is rather inadequately supported by economic analysis. This as well as the need for tax harmonization will doubtless become more obvious as the 12-15 year transition period proceeds.

KAREL HOLBIK

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[1] Cf. one of the most recent publications by the Ligue Européenne de Cooperation Economique, *La fiscalité indirecte dans le marché commun*, Brussels 1959.

*Capital Taxation in a Developing Economy (India)*. By I. S. GULATI. Calcutta: Orient Longmans, 1957. Pp. vii, 209. Rs 12.50.

The capital taxes discussed in this book are general taxes assessed on the whole corpus of the taxpayer's capital without differentiation between the constituent parts of his capital. Three forms of capital taxation are a capital levy, an annual capital tax or wealth tax, and death duties. The book includes a general discussion of capital taxation with special reference to its suitability in India, a fairly detailed critique of the Indian estate duty, and a brief postscript on certain taxes adopted in India in 1957 after the completion of the main text of the book.

Dr. Gulati argues convincingly that capital taxes are less damaging to investment incentives than income taxes. A capital tax discourages investment only to the extent that the investor is deterred by the thought of the tax that will apply if he saves part of the return. Capital taxes, moreover, do not infringe on the income that serves as a reward for assumption of illiquidity or risk; a person does not increase his tax liability by shifting from cash or government bonds to high-yield, high-risk shares. Among capital taxes, discouragement of investment is likely to be greatest for an annual tax, intermediate for death duties, and least for a capital levy which is not expected to be repeated. Because of its recurrent nature, an annual capital tax is also likely to reduce private consumption more than a capital levy or a death tax. The author concludes that an annual tax, although inferior as regards incentives, may be preferable to the other capital taxes for purposes of development finance because its influence on consumption makes the annual tax more anti-inflationary. Gulati, indeed, advises caution in the use of revenues from death duties for government expenditures because of the likely expansionary consequences. This line of reasoning seems generally valid, but the author may understate the probable influence of death taxes or a capital levy on private investment. As he recognizes, when taxpayers meet their liabilities by sale of existing assets, part of current savings will be diverted to purchases of these assets and new investment will be curtailed unless the buyers would otherwise have held idle savings.

Gulati is confident that the administrative difficulties of an annual capital tax can be overcome. He contends that the failure of the general property tax in the United States was due to local administration and that the American example is inapplicable to a national tax. While conceding that other countries are not necessarily condemned to repeat American mistakes, many readers may take a less optimistic view of the administrative problems. The unsatisfactory status of land tax assessments in many underdeveloped countries, including India, offers grounds for skepticism about the success with which all kinds of property can be discovered and valued.

Following a report submitted by Nicholas Kaldor, the government of India in 1957 imposed an annual wealth tax and a personal expenditure tax. In a postscript, Gulati, who assisted Kaldor in the preparation of his report, expresses disappointment that these measures did not go as far as Kaldor had

recommended. Since 1957 a gift tax has been imposed and rates of the wealth tax have been increased. The wealth tax, estate duty, and gift tax, however, still account for only 2.6 per cent of estimated central-government tax revenues in 1959-60.

This book can be read with interest and advantage by tax specialists and others who are concerned with the financing of economic development.

RICHARD GOODE

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## International Economics

*Economic Elements in the* Pax Britannica—*Studies in British Foreign Trade in the Nineteenth Century.* By ALBERT H. IMLAH. Cambridge: Harvard University Press, 1958 Pp. xiv, 224. $6.00.

The coverage and approach of this book, by a professor of history in the Fletcher School of Diplomacy, is best indicated by its subtitle. Chapter 1, "Distinctive Elements in the *Pax Britannica*" is the substance of an article published in the *South Atlantic Quarterly* in January 1951; Chapter 2, "Real Values in British Foreign Trade, 1796-1853," and Chapter 4, "The Terms of Trade of the United Kingdom, 1796-1913," are an expansion and extensive revision of articles that appeared in *The Journal of Economic History* in November 1948 and November 1950; Chapter 3, "British Balance of Payments and Export of Capital, 1816-1913" is a revision of an article in *The Economic History Review* in 1952; and parts of Chapter 5, "The Failures of the British Protectionist System" appeared in 1949 in "The Fall of Protection in Britain," a contribution to *Essays in History and International Relations in Honor of George Hubbard Blakeslee.* The final chapter is "The Success of British Free Trade Policy."

The opening chapter deals with the relation between British economic policy and world leadership in the century after Waterloo. Imlah admires the sure touch with which Britain, particularly in the period up to 1860, combined the concept of the balance of power, the threat of force against those who would disturb the peace, and the offering to all countries the opportunity to share in the benefits of peace and trade. As he phrases it:

> . . . [Britain] supplemented and strengthened her relatively weak military position for diplomatic mediation by exemplifying and promoting the attractive economic and social opportunities of the century which appealed to the peoples of Europe and of the world. This became a distinguishing feature of the *Pax Britannica* and a chief reason for the large measure of success which it achieved (p. 6).

It is a stimulating discussion and one that, even in the changed conditions of the mid-20th century, has solid fare alike for the economist, and for the student of international affairs who is trying to think through the critical problem of the proper role of economic policy in over-all foreign policy.

Chapters 2-4 are the products of research on which Imlah worked for the

better part of two decades—the calculation of the components of the British balance of payments for the years 1796-1913. A major part of this task, on which much of the other analysis depends, is a new set of figures for British commodity exports and imports up to 1853. The "official" values of exports and imports had, before 1854, been based on arbitrary unit values, many of which had been set in the seventeenth century. These official valuations showed a commodity export surplus through 1853, but Imlah's calculations indicate that as early as 1796 Britain had an import surplus; that in all but five years between then and 1822 it had an import surplus; and from 1823 through 1853 it had a continuous import surplus (pp. 37-38), as it also had in 1854-1913 on the basis of British official figures based on market values. However, invisible earnings, particularly those from shipping, gave Britain a surplus on current account in all years but four between 1816 and 1913 (pp. 70-75).

The calculation of the net barter terms of trade, with an 1880 base, is a careful study, and covers a longer period and uses more comprehensive data than the previous studies by Werner Schlote and F. W. Taussig. The figures will add to the growing doubts of economists whether long-run changes in net barter terms of trade, so frequently cited by economic development enthusiasts as evidence of "exploitation" of underdeveloped countries or as proof of increased "gains from trade," have much meaning in this connection. As Imlah indicates in several places, some periods of great domestic prosperity and trade expansion for Great Britain came with unfavorable shifts in Britain's net barter terms of trade. He also calculates indices of gross barter terms of trade, total gain from trade (net barter terms of trade times index of total trade volume), market or trade balance, and market gain from trade (market or trade balance times index of total volume). Many economists will question the usefulness of the concept gross barter terms of trade, and of the derivative concept, market gain from trade. Furthermore, it would have been helpful had there been more analysis of the concepts of net barter terms of trade and of total gain from trade, and also of their relation to the theoretically more important but operationally more difficult notions of single factoral terms of trade, and to a gain-from-trade concept that takes into account not only changes in the price relations of exports and imports and in the production functions of exports, but also changes in the relation of the production functions of exports and of goods that could replace imports. But it would be ungracious for an economist to criticize a historian for failing to come to grips with concepts where much analysis among economists is still fuzzy. Imlah has put economists in his debt by the meticulous care with which he has analyzed the figures of British commodity trade, service items, and investment, and any economist before he repeats old clichés about the British trade balance, the relation of tariffs to foreign investment, and the bearing of net barter terms of trade upon economic progress, would do well to check these preconceptions with the estimates produced by Imlah's scholarship.

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*Organizatsiya i teknika vneshnei torgovli SSSR* (The Organization and Technique of the Foreign Trade of the USSR). By PAVEL ALEKSEEVICH CHERVYAKOV. Moscow: Vneshtorgizdat, 1958. Pp. 295. Rbl. 7.95.

To convey an accurate impression of this book, the word "Ministry" should have appeared in the title, for it describes only the work of the Ministry of Foreign Trade of the USSR (of which the author is an official). The limitation is unfortunate, for Chervyakov has produced—it would seem—the first Soviet book in this field for 20 years (during which period, as he points out on page 8, Soviet foreign trade turnover has quintupled). Since the Ministry does not appear to play an active part in the process of determining the share of trade in the national planning balances, he confines himself to remarking (p. 63) that trade is an entry in both the physical (input-output) and the financial tables drawn up by the State Planning Committee—which, he says, takes account of commitments under long-term trade agreements (p. 65)—and to recording the concern (generally believed to be both political and economic) with such planning of the State Committee for Foreign Economic Relations (p. 37).

The picture which emerges is of a Ministry of Foreign Trade charged with the efficient execution of plans received from a superior authority (pp. 33, 65). There is a section entitled "The Study of Economics and Foreign Trade Conditions" (pp. 79-82), and the author cites aims such as "raising the profitability of exports . . . and rationalizing imports" (p. 40), the "economic analysis of foreign trade operations" (p. 45), and that "foreign trade plans must be particularly flexible and operational" (p. 65); but throughout he has in mind only business acumen in buying and selling from a "shopping list" and the minutiae of commercial costs such as storage and shipment charges. He nowhere mentions the consideration of comparative costs, even in his two examples on the choice between home output and imports. To demonstrate the integration of foreign trade with domestic planning in his country, he shows (p. 62) that the import content of the cost of the equipment used for the Stalingrad and Kharkov tractor plants in the '30's was less than the import bill during 1924-31 for the number of tractors produced annually in those plants. Later (p. 82) he illustrates the need for a thorough study of the economics of foreign trade with the comment "One must not, for example, buy machinery the technical and economic parameters of which are inferior to those of similar machines produced in the Soviet Union." One finds no defense of such recommendations on grounds, for example, of permanent autarky or temporary balance-of-payments difficulties. Indeed, the only statement on the balance of payments—other than pure definitions (pp. 63, 65, 69, 85)—declares that the State Bank has exclusive powers over foreign-exchange operations, that equilibrium is the objective and that, since "most capitalist countries have inconvertible currencies," balances must be established country by country (p. 69).

Chervyakov gives no indication as to whether his Ministry pursues a more sophisticated approach to foreign-trade economics. He is not alone, however, in expressing no opinions on the measurement of trade efficiency in a planned

economy: no Soviet economist appears to have written on the problem since the war, although the subject has been in active debate in the Eastern European economic journals for the past five years.

The interest of Chervyakov's study to the trade economist lies in his meticulous description of how a Soviet Trade Corporation executes an import or an export operation, and especially of how it fixes export prices (pp. 206-10). The criterion is exclusively what the market will bear, viz., as shown by prices on capitalists markets, by prices earlier obtained for exports by the Soviet Union or another Socialist country—whose trade files seem to be open to each other (p. 210)—or by offers to the Corporation from prospective capitalist purchasers. Nowhere is the domestic cost, absolute or relative, taken into consideration—although in passing the author, somewhat blithely in the context uses the term "profits" to describe the margin between export proceeds at the official rate of exchange and the price at which the Corporation buys from the Soviet producer (p. 206).

M. C. KASER

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*Factor Endowments and International Trade.* By SUBIMAL MOOKERJEE. Bombay: Asia Publishing House. Pp. viii, 99. Rs 8.75.

Ricardo's demonstration that trade will and should take place if comparative costs differ is best labeled a *theory of trade,* while his explanation of differences in comparative costs (in terms of labor productivities) is best thought of as a *theory of comparative advantage.* Ricardo's theory of trade has survived as a central part of international trade doctrine, but his theory of comparative advantage has suffered a more controversial history. The alternative, and more fundamental, theory of comparative advantage offered by Heckscher and Ohlin is the subject of Mookerjee's little book.

Recent interest in the Heckscher-Ohlin model has been stimulated by the theoretical surprises contained in Samuelson's and Lerner's celebrated papers on factor price equalization, and by the controversial empirical findings in Leontief's study of American factor proportions. The theoretical advantage of the model is that it for the first time adequately links the theory of international trade to the marginal productivity theory of functional income distribution. The practical implications are considerable. Because trade and factor movements are substitutes it follows that factor prices throughout the world will be closer together or farther apart the lower or higher, respectively, are impediments to trade; thus trade impediments stimulate factor movements and greater impediments to factor movements stimulate trade. A further implication of the theory is that the pressure of rapid population growth on wages, or the pressure of rapid accumulation on profits, can in part be relieved (for one country) by "vicarious emigration" in the form of labor- or capital-intensive exports. Whether these predictions would be warranted or not serves as a test of the usefulness of the Heckscher-Ohlin theorem that countries export abundant-factor-intensive goods.

Mookerjee's book, which is subtitled "A statement and appraisal of the

Heckscher-Ohlin theory," begins with a review of Ricardian doctrine and a statement of its shortcomings. The second chapter lists the assumptions of the simplified Heckscher-Ohlin model; close attention is paid to the possibility that factor intensities may be reversed. The next chapter outlines the conditions under which a country exports its abundant-factor-intensive commodity under both definitions of factor scarcity (factor returns and factor supplies). The fourth chapter considers some extensions of the simplified model (more goods, factors and countries; changes in factor supplies and "quality") and the conditions under which factor prices will be equalized. The final chapter reviews briefly the empirical findings of MacDougall and Leontief, and discusses the interpretations of the Leontief "paradox" which are consistent with the Heckscher-Ohlin theorem; the final section is an attempt to evaluate the contribution of Heckscher and Ohlin, but it pays more attention to the nature of the assumptions than to the predictive value of the conclusions.

Mookerjee accomplishes what he set out to do. In the preface he writes:

> . . . In recent years there has been a growing interest in this theory among economists and a number of important contributions on the subject have appeared, although the discussion is still mainly confined to the periodical literature. I have drawn freely on all of these with a view to bringing together at one place the many important aspects of the theory stressed by different writers.

Nothing new has been added to the literature either in the form of additional implications of the model or further empirical evaluation. Nevertheless the book is a competent and lucidly exposited survey of the subject and would be useful to students—especially those who do not have access to the periodical literature.

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## Industrial Organization; Government and Business; Industry Studies

*Effects of Mergers*. By P. LESLEY COOK AND RUTH COHEN. London: Allen & Unwin; New York: Macmillan, distrib., 1958. Pp. 458. $9.50.

Efforts to relate the conclusions of economic theory to the performance of business and to public policy toward business are plagued by complexities: causes and incentives are multiple; their relative importance is uncertain; the interval of time between cause and effect is undetermined and presumably varies from one instance to the next; the environment does not accurately match any theoretical model; theories of development, unlike static theories, are incompletely formulated. Theorists are tempted to avoid such difficulties by relying upon logic unchecked by facts. Empiricists are tempted to avoid them by substituting history for analysis or by assuming that sequence demonstrates causation.

In seeking to ascertain the effects of mergers from case studies of six Brit-

ish industries,[1] the authors of this book have been aware of such difficulties. They attribute to mergers the difference between an industry's actual development and the development that would have occurred if there had been no mergers. Thus they compare factual history with conjectural alternatives. They recognize the limitations of such a procedure. Factual history, they say, abounds with problems as to the relevance of its various aspects and as to the relative weight to be given to each. Their conjectural alternatives are presented with full awareness that such formulations rest upon "bold individual judgment based upon factors which can neither be measured nor weighted except by such impressions as can be obtained as to 'how the industry works' " (p. 12). They warn the reader, too, that their method necessarily assumes that an industry's development is determined by circumstances rather than unpredictable individual behavior; that results are often different in the short and in the long period; and that changing industries are likely to be best served by changing industrial structures. In the light of these limitations, they describe their conclusions as the results of "glorified guesswork" (p. 13). Seldom has a problem as to the impact of an economic institution been better conceived. Seldom has the attack upon such a problem been more cautiously and modestly formulated.

In avoiding Scylla, however, the authors have been caught in Charybdis. Their judgments depend upon the adequacy of the hypothetical alternatives to which the actual case histories are compared. Yet most of their space and effort are devoted, as was almost inevitable, to the factual studies. The hypothetical yardsticks are not fully examined. The crucial and controversial issues of the book are to be found between the lines as often as in the topic sentences.

This aspect of the matter is illustrated by the conclusions, of which there are two sets, one as to the situations that produce mergers, the other as to the effects of mergers. Mergers take place, the authors find, "in order to secure the economies of scale or to reduce intense short-period competition, or, more frequently, to do both" (p. 438). The economies of scale that furnish the first of these two incentives to merge are discussed as though they were pervasively present. Such treatment is the natural result of the use of a concept of economy that is unusually broad. It includes not only technological and organizational economies, but also marketing economies that appear to consist largely of manifestations of preponderant bargaining power. In the flat glass industry, for example, costs of selling and distribution have been reduced because it has been possible to disregard the behavior of competitors (p. 434). In the soap industry, Unilever's dominance has enabled it to offer the consumer "the selection which is thought to give the best compromise

[1] The volume, one of a series of studies in industry at Cambridge University, contains a chapter on motor cars by George Maxey, one on brewing by John Vaizey, one on soap by Ruth Cohen, and three on cement, calico printing, and flat glass by P. Lesley Cook. An introduction and a conclusion were written by Miss Cook. According to the preface, Miss Cohen supplied the original idea, Miss Cook most of the general ideas, and the two together the detailed development of the argument and the appraisals.

between choice and price," whereas competition probably would have provided "an unnecessarily large choice" (pp. 434-35). Thus "economy" is stretched to include reduction of the buyer's freedom of choice and of the seller's responsiveness to pressures from buyers and competitors. Apparently it follows that to reduce competition is automatically to realize "economies of scale" if the seller can thereby reduce his expenditures (or enhance his net income?). Where economies of scale are attainable, they may be realized either by the expansion of the more successful competitors or by mergers; but mergers are unlikely to take place unless the economies are difficult to realize without them.

The second incentive to merger is said to be intense and painful competition. This competition is said to be produced by such factors as large numbers of competitors (not in tacit agreement); strong and persistent incentives to discriminate in price; variation in the efficiency of competitors; high fixed expenses; idle capacity attributable to the staggered replacement of expensive equipment; fluctuating or declining demand; and fluctuating prices for raw materials. Since almost every industry displays several of these characteristics, the authors apparently mean that whenever competition is not reduced by agreement or by concentration, it is so intense and painful as to provide incentives for merger.

The effects of mergers are discussed in the light of three possible developments for an industry: (a) restrictive agreement by which price competition is diminished and the incentives to reduce competition by merger or by expansion are removed; (b) expansion by some enterprises that eliminate others until concentration reduces price competition and results in economies of scale; (c) mergers that have similar effects upon competition and perhaps similiar effects upon economies of scale. Since each course of action has a similar effect upon competition, the competitive impact affords no basis for preference among them. But concentration by expansion or merger is preferable to price agreement wherever economies of scale are impossible without it; and neither form of concentration is clearly preferable to the other.

In American thinking about mergers, three opinions (differing, of course, in plausibility) are common none of which appears to have been seriously explored in this book: First, that mergers are less desirable than other forms of concentration because, unlike these other forms, they have not been preceded by a period of competitive struggle, a period that is desirable in itself; second, that in combining activities that were not originally designed to fit together and have not been brought together under the pressures of competition, mergers are less likely than other forms of expansion to attain whatever economies are possible for a large business unit; third, that whereas concentration that develops from competition is likely to be limited by diseconomies of scale and by the stubborn competition of the remaining competitors, mergers have no such natural limits.

More striking than any of these omissions, however, is the central omission upon which the book's generally favorable judgment of mergers depends—omission of the hypothetical alternative of continuing vigorous competition.

One possibility is that, in the absence of mergers and price agreements, competitors may struggle to expand with such fluctuating fortunes and lack of sustained success that competition perpetuates itself. Not all industries have shown economies of scale (as distinguished from bargaining advantages) with each increase in concentration. Not all industries have shown a sustained trend toward concentration, either by expansion or by merger or by both. Not all relatively concentrated industries have ceased to be vigorously competitive. The competition that persists can be strengthened by public policy. In assuming that price agreements constitute a generally available alternative to mergers, the authors are true to historical fact in England; but by implication they discount, perhaps unduly, the cumulative impact of recent British legislation. In assuming that concentration without merger is likely to produce results substantially equivalent to merger they apparently assume that, whether or not public policy is applied in the future in such a way as to reduce mergers, it will not otherwise interfere with business concentration. This is possible; indeed, it is probable. But it is not inevitable.

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*Foreign Commerce and the Antitrust Laws.* By WILBUR LINDSAY FUGATE. Boston: Little, Brown and Co., 1958. Pp. xxiii, 384. $16.00.

This is an extremely important addition to the already overflowing literature of the antitrust field. In the author's own words:

> The application of the anti-trust laws to foreign commerce is still somewhat of a frontier area of law. Where there are few foreign cases, or sometimes none, dealing with a particular practice, the author has attempted to place the interstate commerce rules in the context of foreign commerce situations. . . .

This paucity of case material makes it difficult to write a legal handbook of neat and simplified summations of existing legal rules and precedents. However, the author has made an honest attempt.

First the book restates the U.S. antitrust laws, then goes on to their applicability to activities outside the United States. Then the Webb-Pomerene Law of 1919, which exempts export associations from the antitrust laws, is analyzed. The author next considers the extraterritorial aspects of patents and trademarks and finally, the antitrust aspects of international investment whether through subsidiaries or joint ventures with foreign partners. In general, the treatment is descriptive and legal, as would be expected. Statutes are dissected, and cases are briefed. But little attempt is made to analyze the economic effects of the statutes and decisions on American business abroad or on world trade in general.

Under international law, courts do not possess extraterritorial jurisdiction and may not punish for an act that is legal in the country of occurrence. American courts cannot punish an American for driving on the left side of

the road in England, for example. In the first extraterritorial case under the Sherman Act, the famous Banana Case (*American Banana Company v. United Fruit Company* 213 U.S. 347 [1909]) the Supreme Court held that the case did not come under the Act, because the acts complained of were not only committed in a foreign country, but involved the actions of a foreign government. The question of whether the creation of a banana monopoly in Central America adversely affected commerce in the United States where the bananas were ultimately sold, was not given much weight. Yet, in a series of later cases American courts have time and time again asserted jurisdiction over acts and contracts abroad if they felt that American trade and commerce were substantially affected. The Banana case was cited, but only to be distinguished in one way or another. For example, in the Alcoa case Judge Hand went so far as to say that American courts had jurisdiction over foreigners for acts committed abroad, stating in part:

> . . . that any state may impose liabilities, even upon persons not within its allegiance, for conduct outside its borders that has consequences within its borders which the state reprehends and these liabilities other states will ordinarily recognize. (*U.S. v. Aluminum Company of America*, 148 Fed. 2d, 1945 at p. 443.)

The test of applicability seems to be whether the acts complained of adversely affect competition and commerce in the United States or the export opportunities of American businessmen. In a 1952 case this position was clearly stated thus:

> . . . We are not restricted by the fact that what the joint companies may have done was or was not lawful in those countries wherein they are located. These actions, whether legal or not where committed, were had pursuant to an agreement unlawfully made and consummated in part by the acts of the defendants within our jurisdiction. (*U.S. v. Imperial Chemical Industries*, 105 Fed. Suppl. 215, 1952.)

Foreign governments have complained and made diplomatic representations when their nationals have been charged with violations of United States laws but they have not legally disputed the jurisdiction of American courts over foreign companies. In several cases the foreign government itself, or an agency thereof, has been involved in an antitrust case, and has successfully raised the question of sovereign immunity, directly or through diplomatic channels.

However, U.S. courts have not worried about the adverse effects of oil import restrictions, sugar quota restrictions, or sales of surplus agricultural commodities abroad on the economies of *other* nations. If our courts are going to claim international jurisdiction, perhaps they ought to be international in outlook.

Under the Webb Act export associations may be formed by U.S. companies for concerted action in the export field. The Federal Trade Commission, which administers the act, has long held that agreements made abroad by a

Webb export association and applying only to foreign markets were privileged under the amendment from prosecution under the antitrust laws. The author points out that the Department of Justice does not share this view, and cites the indictment of the Alkali Export Association in 1949. The defendants relied entirely on the Webb Act for their defense and lost. Only future cases will show whether this decision marks the beginning of judicial "repeal" of the Webb-Pomerene Act.

Of late there has been much interest in the economic development of backward areas and the use of joint ventures as an instrumentality of industrialization. Will the antitrust laws, as presently interpreted, inhibit such joint ventures, and hence the industrialization of underdeveloped areas? Let us suppose that Phelps Dodge Corporation, Phillips of Holland and a government development corporation in, say, Argentina set up a joint venture, in which each holds a third interest, to manufacture copper wire. This venture might conceivably limit the opportunities of other American companies to export copper wire to Argentina. Does this arrangement constitute a substantial adverse effect on U.S. commerce and hence violate the antitrust laws?

Such situations have resulted in advocacy of a less stringent antitrust approach for investment in foreign countries where the philosophy of market regulation is far different from ours, lest U.S. investment abroad be unduly inhibited. The author seems to feel that the answer would be for foreign countries to adopt antitrust legislation similar to ours. The principle of international comity, however, calls for compromise on our part as well as on the part of foreign nations. Economic development among equals leaves no room for an omniscient father figure.

Limitation of space precludes consideration of many other interesting topics covered by the author. The above-cited sections will, it is hoped, give the reader a sample of the book's contents and approach. It is a solid contribution, which every practicing lawyer and economist interested in problems of market regulation and economic development should have in his library. It is far too specialized for a college course in trade regulation, but it should be included in reading lists.

In this reviewer's opinion the book has several weaknesses. The author by his own admission is biased by a "background of prosecution experience." This has resulted in an ultraconservative view as to what modifications in our antitrust laws might make them more effective in the field of foreign commerce. This view is reinforced by the fact that the author regards cases in interstate commerce as analogous to those in foreign commerce, in spite of the marked difference that conflict between two philosophies of market regulation and control is rarely, if ever, involved in interstate cases. His knowledge of antitrust law in foreign countries leaves much to be desired, if the exposition in this volume is any guide. Our courts have no more right to force our legal concepts on other nations than they on us. An international approach to the entire problem of market power and regulation in the foreign sphere may

result in greater understanding, facilitate investment abroad and most important, remove sources of needless friction between nations.

PHILIP CHARLES NEWMAN

*General Secretariat*
*Economic Planning Board*
*Government of Guatemala*

*Business Enterprise: Its Growth and Organisation.* By RONALD S. EDWARDS AND HARRY TOWNSEND. New York: St. Martin's Press; London: Macmillan & Co. Ltd., 1958. Pp. xvii, 607. $12.50.

This text by two professors at the London School of Economics surveys ground that economists have left somewhat neglected. The authors systematically and at length apply economic analysis to problems of business development and organization, scrupulously avoiding pricing, capital budgeting and labor relations. These activities, which they regard as techniques of industrial administration, are reserved for a companion volume. J. S. Bain's *Barriers to New Competition,* Chapters 2 to 4 of M. W. Watkins' *Industrial Combinations and Public Policy,* and G. J. Stigler's article, "The Division of Labor is Limited by the Extent of the Market" are parallel treatments of some of the areas with which this treatise is concerned. Although the preface indicates that the book is written to help both businessmen and students, its major purpose is frankly to influence long-run governmental policy toward industry. This purpose infuses the appraisal of different forms of organization with an urgency not common among business texts.

Throughout the book, the authors have placed heavy reliance on historical, qualitative digests of business experience, rarely resorting to even simple models. In much of the discussion they have drawn on more than 200 papers presented at Edwards' seminar by leading British businessmen and a sprinkling of civil servants and academicians. The authors are convinced that it is their case material, not elsewhere available, that is of primary importance; "we need to find out much more about the actual operations of firms and industries before very confident conclusions are drawn about any of the big issues" (p. ix).

Parts I and II are concerned with problems of the birth, growth, identity, and size of private business firms, and the extent of their use of the market. There is ample and imaginative documentation of the point that viability is almost impossible to predict in advance, though with hindsight it is easy to classify firms according to factors responsible for survival and growth. The discussion of location, economies of scale, the merits and demerits of large organizations, and the varieties of integration is avowedly arranged so as to flesh out the skelton of R. H. Coase's 1937 article as the best embodiment of the economic theory of the organization of the firm. Here too the illustrations are marshaled to show that what may be economically wise for one firm need not be for another. Part II concludes with two perceptive chapters on integration

by trade associations, and alternative distribution patterns.

Part III is a study of the effect of different forms of government control on the efficiency of industry. Although, following their announced purpose, the authors are primarily concerned with the economics of alternative administrative organizations, and their discussion moves topically from governmental spending through compulsion, control and ownership to supervision, their shirt-sleeves familiarity with the economics of the industries involved makes these chapters exciting reading even for economists who may not be deeply stirred by an appraisal of the relative merits of part-time versus full-time directors. While the authors would like to have their public-policy conclusions regarded as deriving from their earlier generalizations on private industry, the judgments are still tentative and, in spite of their undisguised preference for a free market, the tone is moderate.

Government spending on research is selected as a case study of spending. In one of the most illuminating chapters in the book, ranging from the details of the institutions to the criteria for research budgeting, the authors indicate the respective decision areas for politicians and research directors, and the importance of using marginal rather than average returns in such analyses. Basic research expenditure is, they conclude, impossible to evaluate effectively but, in any event, it will be limited by the number of first-class men who can perform it. There follow intensive studies of the shifts in policy toward the nationalized coal, transport, gas, and electricity industries, concentrating mainly on the consequences of differing degrees of autonomy allowed regional or other subdivisions. The authors would prefer competition among regional Coal Boards, but acknowledge both the administrative obstacle raised by the shortage of trained personnel, and the political obstacle in the received doctrine of the miners' union. On the other hand, they feel that decentralization in gas has gone too far. Within the framework of nationalization, they advocate the fullest possible use of competition and the commercial principle—meaning payment for capital at a full market price, depreciation on a replacement cost basis, covering costs through revenues, and pricing to avoid favoritism. Hugh Gaitskell is quoted in partial support. On the whole the supervision that has been substituted for nationalization of the steel industry has worked well though there are some questions still unsolved—such as whether the industry should be permitted to price so as to obtain expansion funds from its customers.

Part IV, even more than Part III, is as the authors point out, a tract for the times. It emphasizes the need for taking steps to increase the number of scientifically and administratively trained personnel in business management, to improve incentives, and to increase the number of innovations.

The message of this book on the economics of organization is, then, that there are "no 'principles of industrial organization' that are both right and useful" (p. 574). The policy deductions which follow will be persuasive, as Edwards and Townsend recognize, only if their readers place consumer sovereignty and occupational liberty high among the scale of societal values. They have, however, built up their case with such insight, authority, and above all,

sweet reasonableness, that the book will prove influential even with readers disposed to give a high priority to industrial orderliness.

JOEL B. DIRLAM

*University of Connecticut*

## Land Economics; Agricultural Economics; Economic Geography; Housing

*Economie rurale.* By PIERRE FROMONT. Paris: Librairie de Médicis, 1957. Pp. 528. 3,000 fr.

*Economie Rurale* is based on Professor Pierre Fromont's lectures at the Faculty of Law of the University of Paris and at the National Institute of Agronomy. It is devoted to the subject of agricultural production. A second volume, which will appear later, will cover marketing and finance.

The book is divided into an introduction and two parts. In the introductory chapters, Fromont discusses the goals, means, and characteristics of agricultural production. Part I is concerned with methods of increasing the efficiency of production. It includes discussions of questions of farm management and organization, problems of choice of alternative production methods, programs of improvements, such as the land consolidation program, and chapters on capital, animal management, and mechanization.

Part II is on the framework of production. It includes discussion of optimum proportioning of factors, optimum dimensions of an exploitation, the legal framework of agriculture, the traditional nature of agriculture, agricultural credit, cooperatives, and a final chapter on the law of decreasing agricultural population.

The point of view which Fromont takes is common to French agricultural economic analysis. It is broad-gauge and includes consideration of such aspects of the rural economy as technology, psychology, laws, attitudes, and social institutions, plus discussions of the political and economic implications of these factors. This approach has the advantage of presenting the subjects discussed from several points of view. It is, however, open to the criticism that some aspects will not be treated in the same degree of detail as others. Fromont has achieved many of the advantages of the multiple discipline approach and, to a commendable degree, has avoided this characteristic pitfall.

A salient feature of this treatise is that the assumed model farm is a typical French or European family-operated farm, rather than a highly mechanized, highly capitalized, large-scale Midwest, U. S. farm. As a consequence, much of the information, theory, and analysis contained in this book will be relevant to less developed countries throughout the world where the organization of agricultural production is closer to the French model than to the Midwest, U. S. model.

The Sections of the book which will be of greatest interest to economists throughout the world are the three introductory chapters and the final chapter. The first three chapters provide an excellent analysis of the nature of agricultural production with special consideration of those aspects which differ-

entiate it from other types of economic activity and of the special nature of the economic and other social science problems which result from the peculiar characteristics of agriculture. In the final chapter on the law of decreasing agricultural population Fromont refines the concept of the rural exodus and relates it primarily to agricultural and industrial technology and secondarily to such factors as demand elasticities, birth rates, labor efficiency, man-land ratios, human wants and international trade. This conceptualization of the phenomenon of decreasing agricultural population suggests the further use of this law as an analytical tool in the study of economic development.

Although this book is designed to provide a theoretical framework for analysis rather than to supply data on the progress of current programs, a reader might wish that more recent data were given concerning the progress of major agricultural programs. A case in point is the discussion of the *remembrement* program. The analysis of this interesting and exemplary program of land consolidation would have been materially strengthened by inclusion of recent statistics on rates of consolidation and totals of acres, farms and towns consolidated in relation to the area in which consolidation is deemed necessary.

*Economie rurale* has a clear style, is interestingly written, well documented, contains a commendable amount of creative and original analysis in the organization and presentation of interpretation and concepts and will be an important addition to the French literature on the subject of rural economy. It should be included in the libraries of those interested in European agriculture, agricultural-production economics and farm management. It will also be of interest to those interested in agricultural development as a facet of economic development.

FREDERIC O. SARGENT

*Agricultural and Mechanical College of Texas*

*Economics of American Forestry*. By ALBERT C. WORRELL. New York: John Wiley, 1959. Pp. x, 441. $9.75.

The author makes it clear that this is a text for students in the nation's forestry schools, and that its purpose is to give them a sound economic perspective for their work as professional biologists, land managers, and engineers. It is a book of economic principles, specifically oriented toward problems that arise in the field of forestry. Two portions of the book should be especially valuable as teaching aids. These are the discussions of price determination in Chapter 3, and of average versus marginal costs in Chapter 8. The latter type of problem occurs frequently in forestry, in such matters as determining lower limits of logging utilization, and in planning and design of logging roads.

While social benefits of forests are noted, emphasis is given to forestry as an economic activity of producing wood. Attention is focused on such problems as how much timber the nation, or a given firm, can afford to produce; determination of how much to spend on planting, pruning, thinning, or other cultural measures to increase yield and quality; and how much forest capital to accumulate in growing stock. The answer rests on the principle that applica-

tion of effort and other resources to timber production is justified as long as returns are greater than those available in alternative fields, and that an interest-rate concept can be employed to measure these alternative rates of return.

An important aspect of forestry is the long time-period of production, often ranging over many decades. This means that costs and corresponding returns or benefits are widely separated, and that a compound-interest technique, or discounting, must be used to compare these values. Over a long time-period the interest factor often grows to huge proportions, and practicing foresters have hesitated to accept the short rotations or felling ages that result from an economic solution.

Other sections deal with labor in forestry, marketing of forest products, integrated production, and analysis of forest-product price trends.

Excellent as this book is in presenting basic economic principles for foresters, this reviewer feels a little disappointed that the author has not drawn more from his own experience and technical writings in applying the economic principles he so well defines, and that he has not given sufficiently detailed examples to guide the practicing forester in a professional analysis of the real problems he will face.

The general economist should not expect to find answers here to the economic problems of forestry. He will, however, quickly become aware that basic economic teachings are taking effect, and that at least some foresters are becoming skilled in the application of economic methodology in allocation of resources, in production economics, and in appreciation of the long-term effect of time on problems of investment and capital accumulation in forestry. Just as economic analysis has contributed to the field of water development, so also is it now beginning to serve as a guide to development of forest resources.

THOMAS C. ADAMS

*Portland, Oregon*

## Labor Economics

*The Labor Force Under Changing Income and Employment.* By CLARENCE D. LONG. National Bureau of Economic Research, General Series No. 65. Princeton: Princeton University Press, 1958. Pp. xxiv, 440. $10.00.

For well over a century, debates about the behavior of the supply of labor have been conducted largely in an atmosphere of armchair reasoning, without benefit of adequate empirical data. It has only been within the last few decades, and only in a few countries, that social scientists have begun to have access to data which lend themselves to careful analysis of the relationships between labor force behavior and other significant social and economic variables.

The long-awaited volume by Clarence D. Long reports the findings of probably the most comprehensive analysis of labor force data ever attempted. Conducted under the auspices of the National Bureau of Economic Research, Long's study is based primarily on American statistics, including decennial

census data from 1820 on and monthly labor force data from 1940 on. In addition, the data available for Canada, Germany, Great Britain and New Zealand have been intensively analyzed, while the study also incorporates the results of comparisons for 16 countries around 1930 and 12 countries around 1950. Particularly useful for the serious student of labor force problems are the lengthy appendices, which include detailed statistical tables and critical discussions of the data for the five countries with which the study is chiefly concerned.

At the outset Long sets forth three central questions on which the analysis will be focused: (1) Has labor force participation been influenced by changes in income and in employment, and, if so, how and under what circumstances? (2) Are these two influences powerful enough to stand out over other possible influences? (3) Does any other possible factor furnish a fairly complete explanation of labor force behavior, or must the explanation be sought in some combination of forces?

Long's most important positive finding tends to confirm Paul Douglas' earlier finding that at any given time labor force participation is inversely related to income. This conclusion is based not only on intercity comparisons of the type used by Douglas but also on interstate and international comparisons, as well as intensive analysis of nationwide labor force and income data for 1940, 1951, and 1956. Despite the wealth of evidence marshaled in support of the author's conclusion, the reader is left with an uneasy feeling that it may require substantial qualification or restatement. The failure of an inverse relationship to appear in intercity comparisons for white persons in 1950 suggests the possibility that the relationship between labor force participation and income may be changing, as does Gertrude Bancroft's finding that in recent years the labor force participation of wives in the 20 to 44 age group has increased most sharply in the upper-income brackets.[1] Earlier studies by N. B. Belloc[2] and Sanford Dornbusch[3] also raised important questions about the Douglas thesis.

Turning from static to dynamic relationships, Long finds that labor force participation is explained "only feebly or not at all" by dynamic changes in income and employment. Although he concedes that labor force participation may be affected by a severe depression or by the employment changes associated with major changes in the size of the armed forces, he argues that it does not appear to be significantly affected by moderate employment changes. This negative finding with respect to the influence of moderate employment changes rests on an analysis of the comparative behavior of unemployment rates and labor force participation rates. Yet this reviewer is impressed by the fact that since 1947 the female labor force participation rate has risen appreciably in the years which have been characterized by the most marked in-

[1] *The American Labor Force*, New York 1958, pp. 124-25.

[2] "Labor Force Participation and Employment Opportunities for Women," *Jour. Am. Stat. Assoc.*, Sept. 1950, *45*, 400-10.

[3] "Correlations between Income and Labor Force Participation by Race," *Am. Jour. Soc.*, Jan. 1956, *61*, 340-44.

creases in total employment (1947-48, 1949-50, 1950-51, 1954-55, and 1955-56) and has changed only slightly or not at all in other years. In this comparison, total employment is defined as civilian employment plus members of the armed forces—an appropriate measure of the total needs of the economy for manpower. It would be interesting to see the results of a careful analysis based on this approach.

With respect to long-run relationships also, one suspects that changes in employment opportunities have played a more important role than Long concedes. Admittedly he presents an interesting analysis of the relationship between the long-run stability of the over-all labor force participation rate, on the one hand, and the divergent trends for women versus youths and older men, on the other. Yet his failure to stress long-run changes in the occupational structure as an important underlying factor making for expanding employment opportunities for women as compared with youths and older men strikes this reviewer as a basic weakness in his interpretation.

To express a degree of skepticism about Long's major conclusions is not to deny that the study is immensely valuable in illuminating many significant relationships between labor force participation and other important variables such as sex, age, race, income, and education. Yet some of the relationships which appear superficially to be somewhat inconsistent with one another might fall into a more consistent pattern if it were possible to develop a broad and unifying hypothesis that would embrace both static and dynamic relationships. Briefly, the type of hypothesis I have in mind would run somewhat as follows: As industrialization proceeds, with its interrelated effects on real income, the occupational structure, and educational levels, the relationship between income and labor force participation changes; more and more people come to value work for its intrinsic interest as well as for the income received, while relatively fewer people view work as drudgery. Such a hypothesis would have to be amplified to bring in changes in hours of work, the size of families, and other significant factors. But it would be consistent with research findings about occupational differences in attitudes toward work, as well as with much of what we have learned about labor force behavior. For the economist seeking to pursue such an approach. Long's book will provide an invaluable storehouse of information.

MARGARET S. GORDON

*University of California, Berkeley*

*Wage Determination—An Analysis of Wage Criteria.* By JULES BACKMAN. Princeton: Van Nostrand Co., 1959. Pp. xv, 316. $6.75.

Professor Backman is not concerned with wage theories but with the criteria that are used by labor and employer negotiators in the setting of wages. Aside from occasionally citing an academic economist and referring to government statistics, he relies mainly on the arguments of wage negotiators and members of arbitration panels. Invoking the necessity for balanced economic growth and the need for a wage policy that will achieve this laudable aim, Backman devotes eleven chapters to examining wage criteria and "the facts

and factors which are usually considered by the parties in collective bargaining." One might without pejorative intent call this work a handbook for wage negotiations. He points out that the criteria have no specific weights and managements and unions are unlikely to adopt similar views on the importance of any specific criterion. In fact, the weight attached to any criterion is governed by its potential use as an argument rather than its objective value as a determinant of wages. At times the author leans a bit in the employer direction, out of habit perhaps rather than out of a desire to be unfair, and warns that the criteria presented by unions do not always meet the public interest.

Wage comparisons as a standard of wage adjustment are the first group examined. First, the author warns that the proper comparison is frequently unknown and unavailable if known, and that considerable "jugglery" is inherent in this criterion. Perhaps an even more serious limitation is the lack of uniformity in job titles, which means that the same job title may in fact designate a different job content. The author calls attention to the absence of uniformity in wage policies of unions. Some strive for equal rates for all workers at the same job irrespective of locality or size of firm; others allow for wide differences in rates between firms. He mentions that a free economy is incompatible with uniform wage rates, but he never explains the reason for this view.

The author argues against the thesis that wage changes can be explained by pattern bargaining; that is, a given arrangement is made by a lead firm which subsequently becomes the guide or pattern for a large number of others. Citing statistics for more than three decades, including periods of war and peace, he shows that the bargains reached have been diverse and many-sided. Backman calls attention to the level of local rates as a significant wage criterion, and this influence manifests itself in the higher rates paid for service workers in the areas where manufacturing wage rates are high. As all firms must draw their labor from the local pool, the service trades must be cognizant of the rates paid in manufacturing and adapt their wage policies to them.

A discussion of nonwage benefits is included, largely to show that since they are costs they will influence the wages industry can pay. In addition, the cost of living, ideal family budgets, and the annual improvement factor, and productivity are examined. Backman explains these criteria fairly and well, but somehow he always finds they are not conclusive reasons for raising wages. An effort to maintain real wages by increases in money wages only leads to providing additional fuel for price increases. Ideal budgets are arbitrary and depend to some extent upon the opinion or caprice of government boards or private groups. Productivity looks like a fair standard, but we are warned that it is difficult to define, and its increase depends upon greater capital investment. There is another dilemma: whether the gains should be distributed in higher money wages or lower prices. Backman tells us he is not interested in "academic exercises," but of course the policy chosen may have effects on investment and inflation, and consequently one cannot simply leave it at the post chosen by the author; and perhaps a bit of "academic exercise" might be useful and enlightening. He finds the annual improvement factor faulty be-

cause wages are the resultant of many factors and he opposes the use of a simple formula. But the use of the annual improvement factor does not rule out other criteria as well.

Professor Backman is also concerned with the effect of wages on inflation, and authorities who prefer strikes, hard bargaining and unemployment are cited. Of course, there are also authorities who have other preferences, so "you pay your money and takes your choice."

Finally ability to pay is rejected as a guide. Backman criticizes unions for not taking account of the absence of ability to pay in years of poor earnings. He also warns that high profits are needed for investment and high employment. It should be noted that ability or nonability to pay is also at times stressed by employers. Backman, despite any shortcomings of his work, shows the methods and arguments used in the actual wage setting. Anyone interested in the process of wage determination will find his work rewarding and useful.

PHILIP TAFT

*Brown University*

*Labor Union Theories in America—Background and Development.* By MARK PERLMAN. Evanston, Ill.: Row, Peterson and Company, 1958. Pp. xv, 312. $6.00.

There has been no dearth of "theories" of the labor movement. Karl Marx, the revolutionist, and Sidney Webb, the investigator and social reformer, each left a different imprint of the basic motivations and social goals of wage earners and their unions. And in this country, beginning with R. T. Ely's *Labor Movement in America,* published in 1886, the more important explanations of American unionism are found in the research and writings of Jacob Hollander and G. E. Barnett and their collaborators at Johns Hopkins more than a generation ago, J. R. Commons and his colleagues at the University of Wisconsin, especially Selig Perlman, Robert Hoxie, Frank Tannenbaum, Carlton Parker, Thorstein Veblen, H. C. Adams, J. A. Ryan, J. B. S. Hardman, and in more recent years John Dunlop and Lloyd Ullman.

Mark Perlman in *Labor Union Theories in America* reviews this large accumulation of writing and arranges it "into natural theoretical groupings." He presents five basic theories of unionism and in formulating them, discusses the work of about twenty individuals whose research and writing occupy an important place in the literature. Some of these, like Ely and Adams, played a prominent part in the early years of the American Economic Association. Others like Tannenbaum and Ryan are primarily known for their writings in other fields. The reviewer found the biographical summaries of the various writers refreshing; the references to the pioneering work in the labor research fields at Johns Hopkins and Wisconsin, the origin of state departments of labor and the early personalities of the Bureau of Labor Statistics interesting reading. This is also true of the 60-page appendix which reviews four important Congressional investigations into labor conditions in the United States beginning with the Hewitt Hearings, 1878-79, and concluding with the historically

important U. S. Commission on Industrial Relations, 1912-15.

Perlman's five groupings of basic approaches to unionism are: the moral conditioning theory, primarily Ely and Ryan; unionism as a revolutionary institution, primarily Daniel DeLeon, the socialists, especially Max Hayes and other Marxians of various hues; the psychological environment interpretation, primarily the work of Hoxie, Parker, Veblen, and Tannenbaum; the economic welfare theory, largely associated with the Webbs in England and Barnett in this country; and the social institution theory, whose roots are in the German historical school of Wagner, Roscher and Bucher and represented in this country by Adams, Commons, and Perlman.

The Commmons-Perlman school views the organized labor movement "not simply as a means to moral, political, or economic reform, nor as a direct psychological protest against the dehumanizing consequences of the industrial and machine society. Though it is a means to these ends as well as a protest, it is most of all something of an end in itself" (pp. 209-10).

In his final chapter in which he compares these five interpretations, he observes that there is no General Theory, that the sole common denominator of all five is that each discussed unions, "and there the common feature ends." The approach each one takes to the trade union movement is determined by the "postulate" concerning the social structure he is willing to accept. It is this difference in postulates which has led to the nonexistence of a general theory about the trade union movement. He concludes that there are only two theories, the economic welfare and the social institution theories, which have current relevance. Yet these two theories are not adequate to explain "member lethargy, anti-union political activities by members and the like."

This is not an original book. Nevertheless, it represents a worth-while contribution to the scant literature on the subject. The reviewer regrets the omission of J. B. S. Hardman, an outspoken critic of the most widely accepted explanation of trade unions goals, and the failure to mention another critic, Charles Gulick, who receives only a scant footnote. The reviewer also questions the assurance with which the author states that each of the theories "was associated in its formation with characteristics or premises peculiar to a particular academic discipline or to a particular general social movement." There are too many exceptions, even among the writers cited, to justify such a generalization.

William Haber

*University of Michigan*

*Labor Unions and Public Policy.* By Edward H. Chamberlin, Philip D. Bradley, Gerard D. Reilly, and Roscoe Pound. Washington, D.C.: American Enterprise Association, 1958. Pp. v, 177. $4.50.

These four essays probe different aspects of the problem of union power. Convinced that union power has had harmful consequences for the community, each author has sought for court or, more often, legislative actions that will remedy the particular problems with which he deals.

Edward Chamberlin argues that monopoly income is commonly found in

our economy because of imperfect competition, and that collective bargaining is a means of determining the distribution of this income within the firm. Because of the employer's natural bargaining power, he accepts the need for worker organization at this level. However Chamberlin feels that larger bargaining units need further scrutiny. Some, depending on the industry and circumstances, may be acceptable; others result in the destruction of desirable intra-industry competition, remove resistance to increasing labor costs, and yield excessively high product-price levels. These latter should be restricted, along with accretions of union power not directly associated with collective bargaining—boycotts, picketing, violence, etc. He concludes that legislation should immediately remedy abuses revealed by the McClellan Committee and, in the longer run, impose major restrictions on the monopoly power of labor.

Philip Bradley examines the "free-rider" argument for compulsory unionism. He finds no proof that collective bargaining has yielded greater benefits, either in size or in composition, than individual bargaining. In addition, conflicts of interest within a union often lead to policies detrimental to the welfare of some members. On these grounds, Bradley concludes that individuals remaining outside the union may be doing so as a matter of rational self-interest rather than as a means of avoiding their share of the costs of collective bargaining. His final plea is for the courts to place more emphasis on the rights of individuals and less on the rights of the majority.

Gerard Reilly deals with federal-state jurisdictional conflicts in labor law. In a major section on compulsory unionism, he approves the Taft-Hartley delegation of power to the states. On other issues (such as boycotts, picketing, collusion, unfair labor practices, and local emergency strikes), he would like to see a clearer statement of state powers, either exclusively reserved to the state or exercised concurrently with federal jurisdiction. This would reduce the "no-man's land" in labor law and "affirm the right of local governments to protect the legitimate interests of their citizens."

In the last essay, Roscoe Pound surveys the history of legal immunities for special groups and persons. He finds labor law an exception to the general trend of declining special privileges. "The labor leader and labor union now stand where king and government and land owner and charity and husband and father stood at common law." Declaring this exception undesirable, Pound argues for legal responsibility for unions for torts, contracts, restraint of trade and several other issues discussed in the essay.

For a volume titled *Labor Unions and Public Policy*, the public policy recommendations are notable for their paucity and vagueness. One can accept Chamberlin's suggestion that certain obvious abuses should be remedied. However the relationships between size of bargaining unit and union power are complex. One can conceive of a reduction in bargaining-unit size that might increase labor wage demands through interunion rivalry; and conversely creation of industry-wide bargaining units might yield "more responsible" bargains. Even accepting the thesis that there is an optimum (but variable) size of bargaining unit from a public interest viewpoint, Chamberlin gives no clue as to how it might be determined. Shall traditionally weak office workers'

unions be encouraged to expand beyond the firm? Shall powerful teamster units be fractionalized? Will the answer differ if the current merger movement among trucking firms continues? What is the proper bargaining unit for an industry with multiplant firms that follow pattern (but not collusive) product pricing?

Part of Bradley's argument rests on an opposite assumption—that unions do not yield wage benefits. However the point ignores fringe benefit gains and union service activities in the plant. His conflict-of-interest argument rests on stronger grounds: whether a member whose interests differ from the majority has the right to withdraw his financial support. The point on the other side is that the union is legally required to represent him in bargaining and grievances whether he is a member or not. Clearly there are arguments on both sides of this issue.

Reilly's essay does an excellent job of highlighting problems in federal-state labor law relationships. However, one might well dissent from his conclusion that jurisdictional clarifications should take the form of increasing state jurisdiction, particularly when collective bargaining increasingly covers multiplant firms and multifirm bargaining units. Pound's essay is a highly exaggerated account of union powers and immunities. The courts in recent years have reversed their earlier trend and have tended to narrow the powers and increase the liabilities of labor unions—a fact which finds little reflection in this essay.

MELVIN ROTHBAUM

*University of California, Los Angeles*

*Théorie du salaire et conventions collectives.* By MAURICE BOUCHARD. Montreal: University of Montreal, 1957. Pp. 322.

This is an ambitious and provocative book. It merits the serious attention of all economists interested in oligopoly theory, labor market analysis, and union impact on wages and employment. Professor Bouchard has utilized purely deductive methods in an effort to provide a more coherent, systematic and truer body of wage theory than currently exists. He has developed theoretical models designed to eliminate the indeterminacy that now characterizes oligopoly theory and labor market analysis. The author considers the absence of a general theory of labor supply and product demand as the most important weakness of pure theory in seeking to explain wage and price formation. His own approach to oligopoly theory stresses the interrelations between factor markets and product markets.

Bouchard's analysis is divided into three main parts, developed here in the reverse order from that employed in his investigation. Approximately half the book is devoted to price theory and contains an excellent critical analysis of the weaknesses and shortcomings of traditional oligopoly theory and labor market analysis. In developing his own theory the author makes extensive use of mathematical analysis to facilitate correspondence between hypotheses and essential reality and also between hypotheses and conclusions. In Part II, the author develops and analyzes principles of collective choice and decision-making on the part of the trade union and considers thoroughly the founda-

tions and incidence of union economic power. Part III is a short analytical summary of the concept of "bargaining power." A chapter of conclusions and a rather complete international bibliography complete the volume.

Bouchard does not regard his study as theoretically revolutionary nor does he present it as a complete theory of collective wage determination. He does maintain, with considerable justification in the reviewer's judgment, that his contribution is a necessary addition to the traditional analytical system of pure theory. The author's analysis contains three principal innovations: (1) the re-introduction of the labor supply curve as an explanation of wage determination under collective bargaining, (2) development of a principle of collective maximization, and (3) integration *as given* into the theory of wages of the element of bargaining power.

This reviewer found the critical analysis of leading oligopoly theorists to be the most stimulating and challenging part of the book. Bouchard begins with Cournot and comes right down the line, dealing lusty critical blows (sprinkled with some compliments) at Chamberlin, Mrs. Robinson, Triffin, Fellner and many others. His critique of the bargaining power theories of Pigou and Hicks is also incisive and illuminating.

In a brief incursion into the realm of sociology to analyze more precisely certain realities that escape the economist, Bouchard displays considerable insight into the nature of social groups, collective action, and the collective objective. In analyzing the latter, Bouchard concludes that the group as such detaches itself entirely from particular objectives and decides exclusively in terms of its stability, strength and existence as a social reality (p. 186). Yet, achievement of individual objectives is the *raison d'être* of the group, and the existence of the group operates as a "liberty factor" for its members (p. 194).

Bouchard's analysis of union economic power leads him to reject as "insufficient and illogical" traditional theory as to the capacity of unions to influence wages and as to the harmful economic effects of union influence. His principle of collective maximization of labor is that the union will fix the price of labor at the highest point compatible with the employment of all whom it represents. Thus, in terms of this principle, there is no reason for a union under perfect competition. The need for a union arises from imperfect competition among employers which produces some inelasticity in the supply of labor for the firm and a monopolistic profit, according to Bouchard.

If one grants an author his assumptions, it is difficult to quarrel with his conclusions when the analysis is logically pursued. In this case, one can find no flaw in the logic of Bouchard's analysis once his assumptions are granted. He intentionally clings throughout his study to purely economic analysis, while clearly recognizing the importance of psychological and sociological variables of an institutional character. By asking his readers to avoid giving his propositions a meaning extending beyond the limits fixed by his hypotheses (p. 15), Bouchard has skillfully stifled in advance the normal critical impulses of an institutionalist reviewer.

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*Sourcebook on Labor*. By NEIL W. CHAMBERLAIN. New York: McGraw-Hill, 1958. Pp. xiv, 1104. $9.75.

In the last two years several first-rate textbooks on labor economics have appeared, notably those by Paul Sultan, Melvin W. Reder, and Neil W. Chamberlain. The volume reviewed here was designed as a supplement to Chamberlain's *Labor*, and as such serves its purpose very well. The reviewer's task, however, is to determine how useful it is as an independent work. According to Chamberlain, the collection has two purposes: (1) to provide readier access to materials otherwise difficult to obtain and (2) to afford students greater opportunity to make informed judgments on controversial issues and appraise for themselves the judgments of "experts."

The book contains over 220 items. The first chapter contains a Bureau of Labor Statistics study of the changing patterns of industrial employment and a speech by Arthur Larson. Chapter 2 is devoted to historical backgrounds on unionism and includes some specimens of 19th century anti-union opinions and some 20th century materials, including a 1937 article from *Fortune* giving the circumstances of the negotiations between John L. Lewis and Myron C. Taylor leading to U.S. Steel's recognition of the Steel Workers Organizing Committee. This chapter would perhaps have benefited from inclusion of more material from Commons' *Documentary History of American Industrial Society* to give a better picture of 19th century unionism.

Chapters 3-6 deal with unions, union leadership, management outlook, and union political activities. The heart of the book, however, lies in the chapters that follow. These deal with the various aspects of collective bargaining. Topics include the bargaining unit, representation, unfair labor practices, management rights, grievance, seniority, productivity, strikes, and wage negotiations. The selections consist largely of speeches and publications of interest-group representatives, NLRB decisions, and labor arbitrations. The last few chapters are devoted to such diverse social problems as labor monopoly, minimum wages, unions and inflation, pensions, unemployment insurance and supplementary unemployment benefits, and the role of unions in society. Throughout the book, sections on discussion and analysis present penetrating questions designed to force students to give the problems under discussion some thoughtful analysis.

The collection is not, nor is it intended to be, a book of readings in the ordinary sense. The materials are essentially from public discussions of labor issues. In this respect, the author accomplishes his goal: the book definitely affords students an opportunity to appraise for themselves the judgment of "experts." Teachers of labor problems who laboriously gather clippings and pamphlets for use as teaching aids will find that Chamberlain has done a most excellent job for them.

It would be easy, and it is certainly tempting, to quarrel with Chamberlain's choice of materials. Every teacher undoubtedly has his pet samples, and some of this reviewer's favorite items are missing. A sourcebook might, perhaps, contain more empirical material. On the other hand, such gems as the aforementioned *Fortune* story, George Brooks' study of seniority in Southern pulp

mills, and Harry Shulmen's arbitration in the Case of the Lady in Red Slacks (and many others) more than offset the dross.

The *Sourcebook* lends itself well as a supplementary text in labor courses oriented toward collective bargaining and to labor relations in terms of current problems. Not only can the student learn to cope with the special pleading, sloppy logic and unstated values which go into so much current public debate; he has an opportunity to get the "feel" of the subject in a way which a textbook alone cannot provide.

BRUNO STEIN

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## Population; Welfare Programs; Standards of Living

*The Population Ahead.* Edited by ROY G. FRANCIS. Minneapolis: University of Minnesota Press, 1958. Pp. x, 160. $3.75.

This volume is the outcome of The Second Symposium on Population Problems held at the University of Minnesota in 1957. According to the introductory comments by the editor, the purposes of the conference were to provide an occasion for a nontechnical interchange of interests and findings among specialists in disciplines bearing on population and, hopefully, to present to the general public a reasonably integrated published account of leading population issues. As an indication of the scope set for the conference, the resulting papers are by a demographer, physiologist, eugenicist-demographer, zoologist, geographer, botanist, anthropologist, and economist. A summary of discussions occupies the final sixth of the volume.

The results are about what might have been expected. On the favorable side, a quick entry is afforded to seeing the breadth of the topic, to a number of useful insights and to a scattering of significant findings. The unfavorable side, however, is more prominent. The professional reader will suspect, and rightly, that oversimplification and asseveration have carried the day. The lay reader can pick conclusions to suit almost any taste or, if more careful than usual, will find the going hardest in the places where improved public understanding could begin to count.

P. K. Whelpton's lead paper begins with an informative if brief review of international vital trends, and then turns to a discussion of recent and prospective fertility patterns in the United States. His over-all judgment is that the odds favor a decline in our birth rate during the 1960's, though attention should have been called to the fact that similar forecasts, using much the same lines of argument, have been made for a decade and been continually wrong. Ancel Keyes, in an excellent piece of exposition, runs over some fundamental physiological factors affecting world nutritional needs. An obvious corollary, forebearingly not drawn by the author, is that most widely used estimates of such needs could stand substantial modification.

The one paper to pay any extended attention to optimum population, the announced topic of special interest to the conference, is by Frederick Osborn. Short shrift is given the concept of optimum size of population, as neither

meaningful for analysis nor practical for policy; however, Osborn believes that a closer investigation of optimum growth rates of population (presumably between zero and the growth rate of output) might be fruitful. The many limitations also inherent in this second concept are not cited and it is a safe guess that the author's main concern is with improving population quality, the need for which is urged eloquently toward the close of the paper.

Jan O. M. Broek deals with the inadequacies of crude and refined density measures for analyzing population pressure; on a world-wide basis, inadequate food supply is still the main shortage but nonrenewable resources may become the more important limitational factor in the future. Edward S. Deevey presents an intriguing account of the equilibrating and disequilibrating factors which may affect the size of various animal populations. His discussion of a dynamic system, in which adjustment processes can alternate in overshooting and undershooting an optimum steady-state position, illustrates the growth of interest among ecologists in the uses of formal model-building. Some recent developments in this area might well repay closer attention by economists.

Karl Sax sees nothing but PROBLEMS. For the world as a whole, "The innate fecundity of primitive man, which was essential for the survival of the species, is now the greatest threat to modern civilization." For Western societies, where birth rates are much lower than in other regions, ". . . genetic abnormalities of mind and body are a major problem of our modern civilization, where natural selection no longer operates in the traditional fashion." By its omission of central variables, its failure to distinguish between hypotheses and facts, and its unwillingness to make even the simplest needed qualifications, this paper could serve as a model of the attrition that may occur when a scientist in one discipline, even an eminent one, attempts to become a popularizer in another. Robert F. Spencer's representation of the cultural anthropologist's contribution to demographic thinking might be appropriate many centuries from now, provided that our knowledge of twentieth-century population patterns were to be as meager then as our knowledge of the Holy Roman Empire is today.

The paper of most direct interest to economists is by Arnold Harberger. Malthus was wrong: although world population has increased "dramatically," death rates have fallen, levels of living have risen almost everywhere and the price of food has fallen relative to labor and probably also relative to prices of other commodities. It may be worth inserting that this is at least somewhat unfair both to Malthus and the facts. Declining death rates in most underdeveloped areas have been mainly the result of changes in medical technology and the willingness to apply such changes. Malthus would not have denied the possibility of the observed trends (along with current observers he would certainly have underestimated them) but would have claimed that death rates in these areas were far higher than they would have been in the absence of population pressures. As to levels of living, the presumptive evidence is that India, Egypt and a number of other countries have suffered declines during most of the last half century, at least in the rural sectors. Again, although

population growth was not the only factor in these situations, it was clearly significant and unfavorable. For the future according to Harberger, if we allow for international trade possibilities, neither food nor minerals shortages are likely to inhibit world population growth over the next fifty years or so.

Having disposed of drastic limitational factors, Harberger then attempts a more general look at population-output interrelations. Making ingenious use of a Cobb-Douglas aggregate output function, modified to allow for technological change, he estimates that population could grow at rates up to 6 per cent without impairing living standards in economies such as our own, and up to 3 per cent in India-type economies. The difference is attributed more to inadequate capital formation and other obstacles to innovation in the underdeveloped areas than to population factors.

One can agree with the general tone of Harberger's approach, which seems a welcome antidote to the population-scare talk prevalent in many academic and nonacademic circles, and yet wish for more caution about details. Cobb-Douglas functions may be inadequate, even as first approximations; both social and private marginal productivity of labor may be poorly reflected by wage returns; relative factor shares in national income need not remain constant; capital formation and technology are not independent of population. Harberger's further simplifying assumption that population and labor force increase at equal rates may be relevant enough for the very long run, but may wash out much of the problem of demographic-economic interrelations over quarters or thirds of a century. Moreover, even if we accept the framework of all these assumptions, we still need to recognize that our knowledge of parameters may be too inaccurate for applications; rather small variations in estimates can lead to large differences in judgments.

The reader of this volume who seeks ready answers will find no dearth of opinions, even on the broadest issues. Not surprisingly, the opinions differ widely. Some relate to quality of population, some to quantity, some to the short run, some to the long run, and not all indicate which.

The sorrowful conclusion of one reader is that the art of integrating disciplines still has a long way to go, even when the occasion is as comparatively favorable as the present one for making the attempt.

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*Economics of International Migration.* Proceedings of a Conference held by the International Economic Association. Edited by BRINLEY THOMAS. London and New York: Macmillan, 1958. Pp. xiii, 502. $9.00.

This book contains twenty-four essays on various aspects of international migration presented to a conference of the International Economic Association at Kitzbühel, Austria, in September 1955. Following the essays, there is a lengthy (115 pages) "Summary Record of the Debate" (by Douglas Hague) on the more significant of these papers. The editor, Brinley Thomas, remarks in his introduction, "Plenty of statistical and demographic matter will be found in this book; but it is subordinate to the treatment of an economic

theme, except in Part VI . . ." I am not sure that this statement is literally true, but if it is, then it must be added that the economic theme is frequently treated in a perfunctory manner.

Parts II-V, about two-thirds of the book, contain a series of accounts of the history and prospects of migration in a number of countries (Great Britain, Eire, Italy, Netherlands, United States, Canada, Australia, South Africa, Brazil, Germany, France, Indonesia and India). They also contain a description of the activities of "The Intergovernmental Committee for European Migration," by its deputy director, Pierre Jacobsen; an essay on "Intra-European Migration and the Prospects of Integration" by F. Edding, and one on the "Migration Problems of the Far East," by T. H. Silcock; I found this last paper very interesting. Most of the country studies contain pertinent demographic information, historical summaries of migration data, and some discussion of public policy on international migration. In this respect, they are rather like encyclopedia articles; useful, but not path-breaking.

In varying degrees, these studies also attempt economic analysis. Here they are disappointing; in general, they just don't go very deep. For all they tell one, migration is unrelated to wage differentials, either as cause or effect. Remarks are frequently made about the "need" for certain types of immigrants, about "surpluses" of other kinds of workers, or the "capacity" of a country to absorb immigrants, but these comments are made entirely without attempt to relate prices and quantities.

In Part VI, A. Sauvy engages in some speculation on xenophobia, immigration and assimilation; W. Langrod of the United Nations argues that in order to absorb immigrants properly, it is necessary that they be properly selected and that the receiving country engage in "long-term planning" to ensure social conditions favoring assimilation; R. Bachi concludes the section with a detailed discussion of the history of Israel's immigration and the present characteristics of its population.

Part I, Analytical Survey, and Part VII are the sections of most interest to economists. Part I consists of three essays, by Brinley Thomas, J. J. Spengler and A. P. Lerner respectively. Thomas ("Migration and International Investment") advances the thesis that prior to 1914, international migration induced capital exports from advanced countries, especially Great Britain, causing construction booms in underdeveloped areas and export booms in the capital exporting countries. Thomas believes that this relationship is of great importance in explaining long swings in the growth rates of British and American economies in the 19th century; as he recognizes, others disagree. The argument offered is largely a brief restatement of a position he has expounded elsewhere in greater detail.

Spengler's essay ("Effects Produced in Receiving Countries by Pre-1939 Immigration") is a comprehensive survey of the literature, past and present. It is an excellent starting point for anyone wishing to become acquainted with the subject. Spengler discusses the effect of immigration on capital accumulation, population growth, social mobility, etc. His only possible sin of omission is failure to consider the implications of the growing body of literature on

internal migration, and its determinants, for the understanding of international migration; his brief section on "Immigration and Internal Migration" is much less comprehensive and rich in footnote references than the others. The breadth of Spengler's coverage in an essay of 34 pages unfortunately precluded any serious analysis; difficulties are noted, but no attempt is made to overcome them.

Lerner's paper ("Immigration, Capital Formulation and Inflationary Pressure") is largely an exposition of the genesis of cost-push inflation. As might be expected, the exposition is excellent; however there is not much that is new. The crucial link between a high rate of immigration and cost inflation is alleged to be a social determination to maintain higher living standards for immigrants than either their marginal productivity enables them to earn, or tax policy can transfer to them without resort to inflation. The recent experience of Israel is admittedly the inspiration for Lerner's model, though he believes it to be of general applicability. There can be no doubt but that an inflation could develop in the manner described; but whether many inflations have been of this kind is still to be shown. The Israeli case may well be as Lerner says; but there are not likely to be very many other examples. When countries are determined to maintain high minimum living standards, they are likely to curb immigration rather than permit inflation.

Part VII consists of a concluding essay by Howard Ellis. He argues that exports of capital and productive techniques have, in recent decades, become substitutes for large-scale migration as an engine of economic development. Ellis obviously feels that they are satisfactory substitutes, and his prudent remarks merit careful attention.

The Summary Record of the Debate is disappointing. The discussants largely confined themselves to ex-cathedra remarks with little in the way of detailed analysis. There was obviously a great deal of argument at cross-purposes, and some of the participants appear to be specialized in fields other than economics. All in all, this is a useful book; but I can't help feeling that it should have been a better one.

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# TITLES OF NEW BOOKS

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This is a collection of short papers presented at a workshop for teachers of science and social studies at the secondary level which was held over a period of three weeks. "The *Proceedings* represent the contribution of outstanding economists and scientists in presenting a blending of these two areas of knowledge for use of interested teachers."

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# NOTES

## SEVENTY-SECOND ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

### Sheraton-Park Hotel, Washington, D. C., December 28-30, 1959

*Preliminary Announcement of the Program, July 15, 1959*

The sessions are to be focused on the broad problem of improving the performance of the American economy. Section I of the program deals with basic issues of economic philosophy and policy. Section II deals with problems surrounding the major objectives of our nation's economic policy. Section III is concerned with means of improvement in specific areas of the economy. Section IV deals with some research in process, which it may be expected will eventually benefit our economy. Section V is self-explanatory.

I

STANDARDS FOR THE PERFORMANCE OF OUR ECONOMIC SYSTEM

*Time:* 9:30 A.M., December 28
*Chairman:* FRANK H. KNIGHT, University of Chicago
*Papers:* Changing Standards of Economic Performance
DONALD J. DEWEY, Duke University
A Critique of Existing and Proposed Standards
TIBOR SCITOVSKY, University of California, Berkeley
*Discussants:* VINCENT W. BLADEN, University of Toronto
KENNETH E. BOULDING, University of Michigan
MARTIN BRONFENBRENNER, University of Minnesota

RELATIONS BETWEEN ECONOMIC THEORY AND ECONOMIC POLICY

*Time:* 2:30 P.M., December 30
*Chairman:* MORRIS A. COPELAND, Cornell University
*Papers:* The Influence of Economic Theory on Economic Policy
CLAIR WILCOX, Swarthmore College
The Influence of Policy and Events on Economic Theory
GEORGE J. STIGLER, University of Chicago
*Discussants:* PAUL T. HOMAN, Southern Methodist University
FRITZ MACHLUP, Johns Hopkins University
JOSEPH J. SPENGLER, Duke University

INCENTIVES AND ECONOMIC GROWTH: CHANGING ROLES AND PUBLIC POLICIES

*Time:* 9:30 A.M., December 29
*Chairman:* SOLOMON FABRICANT, New York University
*Papers:* Incentives in the United States
ARMEN ALCHIAN, University of California, Los Angeles
WILLIAM H. MECKLING, Rand Corporation
Incentives in the Soviet Union
GREGORY GROSSMAN, University of California, Berkeley
Incentives in Underdeveloped Economies
SIMON ROTTENBERG, University of Chicago
*Discussants:* JOSEPH S. BERLINER, Syracuse University
BENJAMIN HIGGINS, University of Texas
HERBERT STEIN, Committee for Economic Development

II

PROBLEM OF ACHIEVING AND MAINTAINING A HIGH RATE OF ECONOMIC GROWTH

*Time:* 2:30 P.M., December 29
*Chairman:* MOSES ABRAMOVITZ, Stanford University
*Papers:* Rapid Growth as an Objective of Economic Policy
WILLIAM J. FELLNER, Yale University
The Conditions of Economic Progress in the United States
W. W. ROSTOW, Massachusetts Institute of Technology
*Discussants:* PAUL A. BARAN, Stanford University
PER GORAN OHLIN, Columbia University
RUTLEDGE VINING, University of Virginia

PROBLEM OF ACHIEVING AND MAINTAINING FULL EMPLOYMENT

*Time:* 8:00 P.M., December 29
*Chairman:* GEOFFREY H. MOORE, National Bureau of Economic Research
*Papers:* Some Questions Emerging Under the Employment Act
EDWIN G. NOURSE, Joint Council on Economic Education
The Power of Our Economy to Generate Employment
CLARENCE D. LONG, Johns Hopkins University
Problems in Reducing Unemployment in Depressed Areas
ISADOR LUBIN, Franklin D. Roosevelt Foundation
*Discussants:* GOTTFRIED HABERLER, Harvard University
JACOB J. KAUFMAN, Pennsylvania State University
RUTH P. MACK, Office of the Mayor, City of New York

PROBLEM OF ACHIEVING AND MAINTAINING A STABLE PRICE LEVEL (Joint session with Econometric Society)

*Time:* 9:30 A.M., December 28
*Chairman:* G. L. BACH, Carnegie Institute of Technology
*Papers:* Analytical Foundations for Anti-Inflation Policy
PAUL A. SAMUELSON AND ROBERT M. SOLOW, Massachusetts Institute of Technology
What About Wage-Push?—Analysis and Policy Implications
LLOYD G. REYNOLDS, Yale University
What Can We Learn from Foreign Experiences?
ARTHUR W. MARGET, Board of Governors of the Federal Reserve System
*Discussants:* LESTER V. CHANDLER, Princeton University
ABBA LERNER, Michigan State University
JOSEPH A. PECHMAN, Committee for Economic Development

PROBLEM OF RAISING INCOMES IN LAGGING SECTORS OF THE ECONOMY

*Time:* 2:30 P.M., December 28
*Chairman:* ALFRED C. NEAL, Committee for Economic Development
*Papers:* Lagging Sectors and Regions of the American Economy
HARVEY S. PERLOFF, Resources for the Future, Inc.
The Evolving Low Income Problems of Agriculture
FRANK J. WELCH, University of Kentucky
Low Incomes in Urban Areas
ELEANOR M. SNYDER, Franklin D. Roosevelt Foundation
*Discussants:* GEORGE H. BORTS, Brown University
GEORGE E. BRANDOW, Pennsylvania State University
VICTOR ROTERUS, U. S. Department of Commerce

PROBLEM OF INTERNATIONAL HARMONY: ECONOMIC POLICIES FOR A LASTING PEACE

*Time:* 8:00 P.M., December 29
*Chairman:* GARDNER PATTERSON, Princeton University

*Papers:* America's Economic Responsibilities as a Great Power
RAYMOND F. MIKESELL, University of Oregon
Promoting Free World Economic Development Through Direct Investment
JACK N. BEHRMAN, University of Delaware
The Revival of International Capital Markets
PAUL MEEK, Federal Reserve Bank of New York
*Discussants:* ARTHUR I. BLOOMFIELD, University of Pennsylvania
EMILIO G. COLLADO, Standard Oil Company (N.J.)
J. RICHARD HUBER, University of Washington

PROBLEM OF SOCIAL PRIORITIES

*Time:* 9:30 A.M., December 29
*Chairman:* JOSEPH L. FISHER, Resources for the Future, Inc.
*Papers:* The Balanced Budget as a Test of Social Priorities
ARTHUR SMITHIES, Harvard University
Criteria for Undertaking Water Developments
JOE S. BAIN, University of California, Berkeley
Criteria for Appraising Governmental Housing Programs
LEO GREBLER, University of California, Los Angeles
*Discussants:* SAMUEL M. COHN, Bureau of the Budget
OTTO ECKSTEIN, Harvard University
NEAL J. HARDY, National Housing Center, Washington, D.C.

III

INVESTING IN EDUCATION AND RESEARCH

*Time:* 9:30 A.M., December 30
*Chairman:* SIMON KUZNETS, Johns Hopkins University
*Papers:* The Role of Scientific Research in Stimulating Economic Progress
IRVING H. SIEGEL, Council of Economic Advisers
Evidence on Underinvestment in Education
GARY S. BECKER, Columbia University
The Outlook for Expenditures on Research and Development During the Next Decade
DEXTER M. KEEZER, McGraw-Hill Publishing Company
*Discussants:* ROGER A. FREEMAN, Institute for Social Science Research
BURTON H. KLEIN, Rand Corporation
HENRY H. VILLARD, City College of New York

FACILITATING MOVEMENTS OF LABOR OUT OF AGRICULTURE (Joint session with American Farm Economic Association)

*Time:* 2:30 P.M., December 29
*Chairman:* THEODORE W. SCHULTZ, University of Chicago
*Papers:* Migration from Agriculture: The Historical Record and Its Meaning
DALE E. HATHAWAY, Michigan State University
The Private and Social Cost of Migration of People Out of Agriculture
JAMES G. MADDOX, North Carolina State College
Policies to Improve the Labor Transfer Process
D. GALE JOHNSON, University of Chicago
*Discussants:* EARL O. HEADY, Iowa State University, Ames
MELVIN W. REDER, Stanford University
GEORGE P. SHULTZ, University of Chicago

REDUCING IMPEDIMENTS TO FOREIGN TRADE

*Time:* 9:30 A.M., December 28
*Chairman:* HOWARD S. PIQUET, Library of Congress

*Papers:* Employment Effects of Unilateral Reductions in U.S. Tariffs
WALTER S. SALANT, Brookings Institution
Trade Barriers and National Security
WILLARD L. THORP, A[illegible]herst College
A Canadian Looks at [illegible] [illegible]ade Policy
JOHN J. DEUTSCH [illegible] [illegible] British Columbia
*Discussants:* JAY W. WILEY, [illegible] [illegible]versity
(Others to be [illegible])

REFORMING THE TAX SYSTEM

*Time:* 2:30 P.M., [illegible]
*Chairman:* LOUIS S[illegible] [illegible]diana University
*Papers:* Incidence [illegible] [illegible]orporation Income Tax
CARL S. [illegible]P, Columbia University
A Program for Federal Tax Reform
DAN THROOP SMITH, Harvard University
*Discussants:* CARMAN G. BLOUGH, American Institute of Certified Public Accountants
RICHARD B. GOODE, Brookings Institution
RICHARD A. MUSGRAVE, Johns Hopkins University

IMPROVING THE UNEMPLOYMENT INSURANCE SYSTEM (Joint session with Industrial Relations Research Association)

*Time:* 2:30 P.M., December 28
*Chairman:* J. DOUGLAS BROWN, Princeton University
*Paper:* Issues in the Improvement of the Federal-State Unemployment Insurance Program
HERMAN M. SOMERS, Haverford College
*Discussants:* WILBUR J. COHEN, University of Michigan
NELSON H. CRUIKSHANK, A.F. of L.-C.I.O.
CLARK KERR, University of California
RICHARD A. LESTER, Princeton University

IMPROVING THE EFFICIENCY OF THE TRANSPORTATION AND UTILITIES SYSTEMS

*Time:* 8:00 P.M., December 29
*Chairman:* CHARLES L. DEARING, Illinois State Toll Highway Commission
*Papers:* Effects of Public Regulation on Railroad Performance
JAMES C. NELSON, State College of Washington
Competition or Regulation of Natural Gas Production and Transportation
ALFRED E. KAHN, Cornell University
*Discussants:* BURTON N. BEHLING, American Association of Railroads
ROBERT W. HARBESON, University of Illinois
RICHARD B. HEFLEBOWER, Northwestern University

IV

RESEARCH ON THEORY OF THE FIRM (Joint session with American Statistical Association)

*Time:* 2:30 P.M., December 29
*Chairman:* RICHARD M. CYERT, Carnegie Institute of Technology
*Papers:* Sequential Decision Making in the Firm
JULIUS MARGOLIS, University of California, Berkeley
Simulation of the Firm
KALMAN COHEN, Carnegie Institute of Technology
Theory of the Efficient Several-Person Firm
JACOB MARSCHAK, Yale University
Managerial Economics and the Firm
THOMSON WHITIN, Massachusetts Institute of Technology

Game Theory as an Approach to the Firm
MARTIN SHUBIK, General Electric Company
*Discussant:* GUY H. ORCUTT, University of Wisconsin

RESEARCH ON INCOME, CONSUMPTION, AND SAVINGS (Joint session with American Statistical Association)

*Time:* 9:30 A.M., December 29
*Chairman:* MILTON FRIEDMAN, University of Chicago
*Papers:* Tests of Some Basic Propositions in the Theory of Consumption
ARNOLD ZELLNER, University of Washington
Labor Supply, Family Income, and Consumption
JACOB MINCER, City College of New York
Transitory Income and Expenditures on Consumption Components
ROBERT C. JONES, University of Pennsylvania
Trends in British Income Size Distributions since the 'Thirties
JOHN A. BRITTAIN, Vanderbilt University
Relationship Between Fulfillment of Consumer Buying Intentions and the Incidence of Unexpected Events
F. THOMAS JUSTER, National Bureau of Economic Research
*Discussant:* MARC NERLOVE, University of Minnesota

RESEARCH ON PRODUCTIVITY, WAGES, AND PRICES (Joint session with American Statistical Association)

*Time:* 9:30 A.M., December 30
*Chairman:* To be announced
*Papers:* To be announced

RESEARCH ON ECONOMIC DEVELOPMENT (Joint session with American Statistical Association)

*Time:* 2:30 P.M., December 28
*Chairman:* MAX F. MILLIKAN, Massachusetts Institute of Technology
*Papers:* Changing Parameters to Variables in Economic Development
EVERETT E. HAGEN, Massachusetts Institute of Technology
Agriculture in Economic Development
WILLIAM H. NICHOLLS, Vanderbilt University
The Role of Government
EDWARD S. MASON, Harvard University
The Factor Proportions Problem
RICHARD S. ECKAUS, Brandeis University
Mathematical Models of Economic Development
HOLLIS B. CHENERY, Stanford University
*Discussant:* ALBERT HIRSCHMAN, Columbia University

V

PRESIDENTIAL ADDRESS

*Time:* 8:00 P.M., December 28
*Chairman:* JOHN M. CLARK, Columbia University
Presidential Address
ARTHUR F. BURNS, Columbia University

SELECTED PAPERS: OPEN COMPETITION

*Time:* 9:30 A.M., December 30
*Chairman:* RICHARD RUGGLES, Yale University
*Papers:* To be announced
(A second session on selected papers may be arranged)

JOINT LUNCHEON SESSION WITH AMERICAN STATISTICAL ASSOCIATION

*Time:* 12:30 P.M., December 28
*Chairman:* MARTIN R. GAINSBRUGH, National Industrial Conference Board
*Address:* To be announced

JOINT LUNCHEON SESSION WITH A[illegible] FINANCE ASSOCIATION

*Time:* 12:30 P.M., Decemb[illegible]
*Chairman:* JAMES J. O'LE[illegible] Insurance Association of America
*Address:* To be announce[illegible]

BUSINESS MEETING

*Time:* 5:00 P.M., [illegible]mber 30

EXECUTIVE COMMITTEE DINNER MEETINGS

*Time:* 6:00 P.M., December 27, and 6:00 P.M., December 30

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The range of activity of the Association's Committee on Economic Education (Ben W. Lewis, Oberlin College, chairman) was expanded by the Executive Committee at its March 1959 meeting, to include the teaching of economics generally in the colleges and university. The Committee on Economic Education has previously confined its attention to economics in the secondary schools and in the training of secondary school teachers.

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The Metropolitan Economic Association has elected the following officers for 1959-60: Norris O. Johnson, president; Emanuel Stein, vice president; Bruno Stein, secretary; Benjamin Klebaner, treasurer.

## FELLOWSHIPS AND GRANTS

Social Science Research Council's fellowship and grant programs for the coming year embody some departures from previous policies and procedures. Prospective applicants are therefore urged to consult the detailed announcement, which is furnished on request, in order to avoid missing deadlines. Closing dates for applications for some types of grants are as early as November 1. Inquiries should be addressed to 230 Park Avenue, New York 17, N.Y.

Predoctoral and postdoctoral research training fellowships will again be offered. Grants-in-aid of research and faculty research fellowships will be offered but the terms of faculty research fellowships have been radically changed. Awards will be made directly to individuals for periods not exceeding two years; part-time appointments for three-year periods will not be offered. Several categories of specialized awards will also be offered, such awards to be restricted to mature research scholars of postdoctoral status; among these will be a new program of grants for research on national security policy with particular emphasis on economic problems.

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The next closing date for receipt of proposals in the social science research program of the National Science Foundation is October 1, 1959. Proposals received by that date will be evaluated in the fall. Approved grants will be activated in time for work to begin in the second semester or the summer of 1960. Proposals received after October 1, 1959 will be reviewed following the winter closing date of February 1, 1960, with approved grants effective in the summer and fall of 1960. Inquiries should be addressed to National Science Foundation, Washington 25, D.C.

## *New Publications*

The first issue of the *Journal of Industrial Relations*, professional journal of the Industrial Relations Society, was published in April 1959. The journal is to be published twice yearly. Editorial communications and manuscripts should be sent to the editor, Mr. Kingsley M. Lafier, Department of Economics, University of Sydney, Sydney, Australia.

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Beginning in January 1960 a new journal *International Economic Review* will be published jointly by the Kansai Economic Federation, the University of Pennsylvania and Osaka University. The editors are interested primarily in articles on quantitative economics and related problems but welcome contributions in mathematical economics and statistical theory related to quantitative aspects of economics. Manuscripts may be sent to the chief editor: L. R. Klein, Wharton School, University of Pennsylvania, Philadelphia 4, Pa., or to the co-editor M. Morishima, The Institute of Social and Economic Research, Osaka University, Toyonaka, Osaka, Japan. The subscription price for three issues per year is $4.00.

## *Deaths*

John E. Candelet, Trinity College, April 16, 1959.

Carson S. Duncan, October 1, 1958.

Roy J. W. Ely, Montana State University, May 17, 1959.

Miriam Hussey, University of Pennsylvania, January 7, 1959.

Richard A. Kahn, August 20, 1958.

Russell C. Larcom, University of Alabama, March 27, 1959.

E. Malone, University of Kansas, November 29, 1958.

Edward E. Rhodes.

Tillman M. Sogge, St. Olaf College, December 15, 1958.

## *Retirements*

Hugh B. Killough, Brown University, June 1959.

Vincent W. Lanfear, University of Pittsburgh, June 1959.

Joseph L. McDonald, Dartmouth College, June 1959.

Thomas F. McManus, Internal Revenue Service.

Gustav T. Schwenning, University of North Carolina. Continues as editor of *The Southern Economic Journal*.

## *Visiting Foreign Scholars*

Paolo Baffi, chief economic adviser to the Bank of Italy: visiting NATO professor, Cornell University, fall semester.

Gordon R. Fisher, of England: lecturer in economics, University of Michigan.

Shinichi Ichimura, Osaka University: visiting lecturer, the Johns Hopkins University 1959-60.

Benjamin C. Roberts, London School of Economics: visiting lecturer in industrial relations, Massachusetts Institute of Technology, spring term 1959.

Leslie Wright, University of Edinburgh: lecturer in economics, Rice Institute, fall semester.

## *Promotions*

Morris A. Adelman: professor of economics, Massachusetts Institute of Technology.

William R. Beaton: associate professor, College of Business Administration, University of Georgia.

Julian H. Bradsher: associate professor of economics, Oklahoma State University.

Stanley I. Buchin: research associate in business administration, Harvard Graduate School of Business Administration.

Colin D. Campbell: associate profess[illegible] economics, Dartmouth College.

Alpha C. Chiang: associate profe[illegible] economics, Denison University.

Avery B. Cohan: associate [illegible]or, School of Business Administration, University of North Carolina.

Lance E. Davis: associ[illegible]sor of economics, Purdue University.

John S. Day: professo[illegible]ustrial management, Purdue University.

John E. Elliott: assis[illegible] professor of economics, University of Southern California.

Ragaei El Mallakh: assistant professor of economics, University of Colorado.

David Felix: associate professor of economics, Wayne State University.

Frank H. Gane: professor of finance, School of Business, Northwestern University.

Claude S. George: professor, School of Business Administration, University of North Carolina.

Abraham L. Gitlow: professor of economics, School of Commerce, Accounts, and Finance, New York University.

William P. Glade: assistant professor of economics, University of Maryland.

Arthur L. Grey, Jr.: associate professor of economics, University of Southern California.

Daniel Hamberg: professor of economics, University of Maryland.

Osmond L. Harline: associate professor of economics, University of Utah.

Leonard Hein: associate professor of accounting, Los Angeles State College.

K. H. Helleiner: professor, department of political economy, University of Toronto.

Howard L. Hoag: associate professor of economics, Purdue University.

Leon Hollerman: associate professor, Claremont Men's College.

William C. Hood: professor, department of political economy, University of Toronto.

Morris A. Horowitz, professor of economics and chairman of the department, College of Business Administration, Northeastern University.

George Horwich: associate professor of economics, Purdue University.

David B. Johnson: associate professor of economics, University of Wisconsin.

Manley H. Jones: professor, department of business and economics, Illinois Institute of Technology.

Edmund R. King: assistant treasurer, Eastman Kodak Company.

Mordechai E. Kreinin: associate professor of economics, Michigan State University.

Leonard C. R. Langer: associate professor of business administration, Harvard Graduate School of Business Administration.

Robert G. Layer: professor of economics, Southern Illinois University.

Wayne A. Leeman: professor of economics, University of Missouri.

Leland C. Lehman: professor of economics, Denison University.

Richard I. Levin: assistant professor School of Business Administration, University of North Carolina.

Ivory L. Lyons: assistant professor of economics, College of Business Administration, Northeastern University.

Thomas Mayer: associate professor of economics, Michigan State University.

Donald A. Moore: associate profesor of economics, Los Angeles State College.

John C. Murdock: professor of economics, University of Missouri.

E. P. Neufeld: assistant professor, department of political economy, University of Toronto.

S Daniel Neumark: professor and economist, Food Research Institute, Stanford University.

Jack N. X. Oanh: associate professor of economics, Trinity College.

James R. Omps: assistant professor of accounting, School of Business Administration, University of Pittsburgh.

Robert W. Ozanne: professor of economics, University of Wisconsin.

Richard L. Pfister: assistant professor of economics, Dartmouth College.

E. Bryant Phillips: professor of economics, University of Southern California.

Paul Pigors: professor of industrial relations, Massachusetts Institute of Technology.

Spencer D. Pollard: professor of economics, University of Southern California.

Harry V. Roberts: professor of statistics, Graduate School of Business, University of Chicago.

Eugene Rotwein: professor of economics, University of Wisconsin.

Frederic M. Scherer: research associate in business administration, Harvard Graduate School of Business Adminstration.

Charles Schertenleib: associate professor of economics, Georgetown University.

Harry Shaffer: assistant professor, University of Kansas.

Ansel M. Sharp: associate professor of economics, Oklahoma State University.

William D. Shipman: assistant professor, Bowdoin College.

George H. Sorter: assistant professor of accounting, Graduate School of Business, University of Chicago.

Peter O. Steiner: professor of economics, University of Wisconsin.

George J. Stolnitz: professor of economics, Indiana University.

W. Paul Strassmann: associate professor of economics, Michigan State University.

Richard K. Stuart: professor of business and economics, University of Maine.

Renato Tagiuri: associate professor of business administration, Harvard Graduate School of Business Administration.

William A. Terrill: professor, School of Business Administration, University of North Carolina.

Wilbur R. Thompson: associate professor of economics, Wayne State University.

Randall Tucker: associate professor of economics, Trinity College.

William Voris: professor of management and head, department of management and marketing, Los Angeles State College.

Edward E. Werner: associate professor of commerce, University of Wisconsin.

Norman J. Wood: associate professor, College of Business Administration, University of Georgia.

John R. Yeager: associate professor of business administration, Harvard Graduate School of Business Administration.

Leland B. Yeager: associate professor of economics, University of Virginia.

Cyril A. Zebot: professor of economics, Georgetown University.

Reuben A. Zubrow: professor of economics, University of Colorado.

### *Administrative Appointments*

Corliss D. Anderson: chairman, finance department, School of Business, Northwestern University.

Thomas J. Anderson, Jr.: chairman, department of economics, School of Commerce, Accounts and Finance, New York University.

C. A. Ashley: chairman, department of political economy, University of Toronto.

Harold Barnett, of Resources for the Future: chairman, department of economics, Wayne State University, succeeding L. H. Seltzer, now full-time professor of economics.

Bernard Berelson, of Graduate School of Business, University of Chicago: director of the Bureau of Applied Research, Columbia University.

V. W. Bladen, formerly chairman of department of political economy: dean of the Faculty of Arts, University of Toronto.

Robert S. Bowers: head of the department of economics, Western Michigan University.

Samuel E. Braden: vice president and dean for undergraduate development, Indiana University.

Arthur Butler: acting chairman, department of economics, University of Buffalo.

Edgar O. Edwards, Princeton University: Reginald Henry Hargrove professor of economics, chairman of department of economics and director of graduate program, Rice Institute.

Ralph C. Epstein: resignation as chairman of department of economics, appointment as research professor of economics, University of Buffalo.

Roland Gibson, of University of North Dakota: professor of economics and chairman of department of economics and sociology, Washington College, Chestertown, Maryland.

Raymond H. McEvoy: acting chairman of department of economics, Montana State University.

Edward G. Nelson: director of the Center of Business Research, University of Kansas.

G. Warren Nutter: acting chairman, department of economics, University of Virginia 1959-60.

Robert W. Paterson: professor of business research and director, Bureau of Business and Economics Research, University of Missouri.

Paul W. Paustian: acting director of Bureau of Business Research, University of Alabama, in absence of Henry B. Moore current year.

H. Austin Peck: director, School of Business Administration and head, department of business, economics, and sociology, University of Maine.

Janus Poppe: assistant director and comptroller, Far East Division, University College, University of Maryland.

Harold L. Wattel: chairman of department of economics, Hofstra College, 1959-1960.

T. F. Wise: deputy director, Institute of Development Economics, Karachi, Pakistan.

A. M. Woodruff, University of Pittsburgh: dean of the School of Government, George Washington University.

## *Appointments*

Irma Adelman, of Mills College and University of California, Berkeley: acting assistant professor of economics, Stanford University.

William W. Alberts: instructor in finance, Graduate School of Business, University of Chicago.

Robert L. Allen, University of Virginia: visiting associate professor of economics, University of Oregon.

Edward Ames, Purdue University: visiting associate professor, The Johns Hopkins University 1959-60.

Jay C. Anderson: USDA collaborator with the Farm Economics Research Division, USDA, stationed at Iowa State University of Science and Technology (formerly Iowa State College).

Ralph L. Andreano: instructor in economics, Northwestern University for 1959-60.

T. Asimakopulos: assistant professor of economics, McGill University.

James W. Baker, University of Michigan: assistant professor of accounting, College of Business Administration, University of Georgia.

Robert A. Battis: assistant professor of economics, Trinity College.

Edwin F. Beal, Western Michigan University: associate professor of management, School of Business Administration, University of Oregon.

Martin J Beckmann: associate professor of economics, Brown University.

Lorenzo M. Belotti: assistant professor of economics, Santa Clara University.

Arthur Benavie: instructor in economics, University of Michigan.

Gorden W. Bertram, University of Washington: assistant professor of economics, Los Angeles State College.

John E. Bishop: lecturer on business administration and member of faculty, Harvard Graduate School of Business Administration.

B D Bixley: lecturer, department of political economy, University of Toronto.

Stanley M. Block: associate professor of production management, Graduate School of Business, University of Chicago.

William C. Bonifield: instructor in economics, University of Minnesota.

J Carroll Bottum: visiting professor in Agricultural Adjustment Center, Iowa State University of Science and Technology (formerly Iowa State College).

Allen J. Braff, formerly University of Wisconsin: assistant professor of economics, College of Commerce, West Virginia University.

Warren C. Bray, University of Akron: associate professor of accounting, Los Angeles State College.

Tony Brouwer, of Calvin College: visiting lecturer in economics, University of Michigan.

Douglass V. Brown, Massachusetts Institute of Technology: Ford visiting professor, Graduate School of Business, University of Chicago, 1959-60.

Roger L. Burford: lecturer in economics, Indiana University.

Arthur F. Burns: John Bates Clark professor of economics and in current academic year holds Ford Foundation endowed chair for research at Columbia University.

Arch C. Callaway, formerly Montana State University: research position, Center for International Studies, Massachusetts Institute of Technology.

Cono Casella, Marquette University: associate professor, C. W. Case College, Long Island University.

Wayne R. Chapin: assistant professor of accounting, Los Angeles State College.

K. J. Charles, McGill University: assistant professor of economics, University of Manitoba.

Sam Chase, University of Illinois: Federal Reserve Bank, Kansas City, Missouri.

Dolores S. Cheek: research associate, University of Kentucky Bureau of Business Research.

Emily T. Chen: assistant professor of accounting, Los Angeles State College.

Charles J. Christenson: lecturer on business administration and member of the faculty, Harvard Graduate School of Business Administration.

Gordon C. Church: instructor in economics, department of economics and political science, University of Saskatchewan.

Alan B. Coleman: instructor in business administration, Harvard Graduate School of Business Administration.

Robert L. Comeau, Brown University: appointment at Carleton College, Ottawa.

Alfred H. Conrad: lecturer on business administration and member of the faculty, Harvard Graduate School of Business Administration.

Dale L. Cramer: visiting associate professor of economics, University of Alabama, current academic year.

Donald C. Darnton, University of Michigan: instructor in economics, Ohio University.

Lewis E. Davids, Agricultural and Mechanical College of Texas: professor of finance, College of Business Administration, Univers[illegible]y of Georgia.

John Dearden: lecturer on business [illegible]nistration and member of the faculty, Harvard Graduate School of Business Admin[illegible]ion.

John D. DeForest: assistant p[illegible]r of economics, Denison University.

Lynn E. Dellenbarger, Jr.: as[illegible]ant professor of economics, Oklahoma State University.

Michael O. DePrano: [illegible] in economics, Michigan State University.

Ramon J. de Reyna, [illegible]structor in accounting, Los Angeles State College.

Robert E. Dickerson, [illegible]ccidental College: assistant professor of economics, Humbolt State College.

Frank G. Dickinson, formerly of the American Medical Association: member of research staff, National Bureau of Economic Research.

Adolf Diegel, formerly Western Reserve University: instructor in management, School of Business Administration, University of Oregon.

Harry R. Dodge: assistant professor of marketing, Los Angeles State College.

Bruce Edwards, of the University of Illinois: member of the project staff, Nommensen University, Madan, Sumatra, Indonesia, for two years.

Robert B. Egelston: instructor in finance, Wharton School, University of Pennsylvania, spring term 1959.

E. John Enright: lecturer in business administration, Harvard Graduate School of Business Administration.

Alexander Erlich: associate professor to teach soviet economics at Columbia University.

Gerald J. Eskin: instructor in economics, University of Minnesota.

James A. Estey, emeritus Purdue University: visiting professor of economics, Indiana University, first semester 1959-60.

Robert C. Evans, University of Chicago: assistant professor of industrial relations, Massachusetts Institute of Technology.

Donald E. Farrar: instructor in economics, University of Wisconsin.

Edward C. Fei: visiting associate professor of economics, Swarthmore College.

Winston M. Fick: assistant professor of economics and government, Claremont Men's College.

E. I. Fjeld, City College of New York: visiting professor of accounting, Los Angeles State College.

Robert F. Fleischer, formerly University of Wisconsin: lecturer, University of Minnesota.

Daniel O. Fletcher, University of Michigan: assistant professor of economics, Ohio State University.

Max E. Fletcher: associate professor of economics, College of Business Administration, University of Idaho.

Herbert W. Fraser: assistant professor of economics, Washington University, St. Louis.

Victor R. Fuchs, Columbia University: associate professor of economics, New York University.

Daniel Garnick: lecturer in economics, University of Buffalo.

George Garvy, advisor Federal Reserve Bank of New York: visiting professor of economics, Columbia University.

Ronald R. Gist: instructor in marketing, University of Missouri.

Meredith B. Givens, retired from position with State of New York: appointment with Harvard University's Advisory Group in Pakistan, Overseas Development Program of the Ford Foundation.

Sonia Gold: assistant professor of economics, Carnegie Institute of Technology.

Samuel Goldberg; visiting associate professor of business administration, Harvard Graduate School of Business Administration.

Marvin E. Gollob: assistant professor accounting, Illinois Institute of Technology.

Leland J. Gordon: first occupant of John E. Harris chair of economics.

Fred M. Gottheil, Duke University: assistant professor, department of economics, University of Illinois.

David Granick, formerly Carnegie Institute of Technology: associate professor of economics, University of Wisconsin.

H A. J. Green: special lecturer, department of political economy, University of Toronto 1959-60.

Clifton M. Grubbs: assistant professor, department of economics, University of Colorado.

William D. Guth: instructor in business administration, Harvard Graduate School of Business Administration.

Pavlos Haggipavlou: instructor in economics, Columbia University.

Louis Hamill: instructor in business economics, School of Business Administration, University of Oregon.

Bernard Hanes: associate professor of management, Los Angeles State College.

Joseph E. Haring: assistant professor of economics, Occidental College.

James J. Healy: professor of industrial relations, Harvard Graduate School of Business Administration.

Erich A. Helfert: assistant professor of business administration, Harvard Graduate School of Business Administration.

James M. Henderson, formerly Harvard University: associate professor of economics, University of Minnesota.

Ervin Hexner, formerly International Monetary Fund: visiting professor of economics, The Pennsylvania State University, 1959-60.

James E. Hibdon, Georgia State College of Business Administration: associate professor of economics, Texas A. and M. College.

Albert O. Hirschman, formerly Yale University: professor of international economic relations, Columbia University.

Paul T. Homan, retired University of California, Los Angeles: professor of economics, Southern Methodist University, in connection with initiation of a graduate program.

Edgar Hoover, of Harvard University: director of Pittsburgh Regional Economics Study; also professor of economics, University of Pittsburgh and lecturer in Graduate School of Industrial Administration, Carnegie Institute of Technology.

Charles T. Horngren: associate professor of accounting, Graduate School of Business, University of Chicago.

Harry D. Hutchinson, University of Michigan: assistant professor of economics, University of Delaware.

Thomas Iwand, University of Oregon: assistant professor of economics, The Pennsylvania State University.

Thomas G. Johnson, formerly Standard Oil Company of Indiana: associate professor of marketing, School of Business Administration, University of Oregon.

Thomas H. Jerdee: lecturer, School of Business Administration, University of North Carolina.

Easo John: lecturer in finance, School of Business, Northwestern University.

H. Milton Jones, Muskingum College: assistant professor of accounting, Los Angeles State College.

Allen F. Jung: visiting assistant professor of marketing, Graduate School of Business, University of Chicago, 1959-60.

F. Thomas Juster, Amherst College· [illegible]mber of research staff of National Bureau of Economic Research.

Murray C. Kemp, McGill Uni[illegible]y: visiting associate professor of economics, Massachusetts Institute of Technolog[illegible] [illegible]wo years.

M. Thomas Kennedy: prof[illegible]r of business administration, Harvard Graduate School of Business Administrati[illegible]

[illegible]Omesh Khanna: instr[illegible]n economics, University of Michigan.

Hugh B. Killough: p[illegible]sor of economics, College of Business Administration, Northeastern University.

John Korbel, formerly Massachusetts Institute of Technology: assistant professor of economics, University of Wisconsin.

Anne O. Krueger, University of Wisconsin: assistant professor of economics, University of Minnesota.

Marian Krzyzaniak: assistant professor of economics, Montana State University.

Norman E. Landgren: USDA collaborator with the Farm Economics Research Division, USDA, stationed at Iowa State University of Science and Technology (formerly Iowa State College).

Richard L. Larson: lecturer on business administration and member of the faculty, Harvard Graduate School of Business Administration.

Bernhard C. Lemke: Commission to Study the Turin Transit System, Turin, Italy.

James R. Leonard: instructor in economics, Montana State University.

Harvey J. Levin, Hofstra College: Brookings Institution professorship 1959-60.

Theodore Levitt: lecturer in business administration and member of the faculty, Harvard Graduate School of Business Administration.

Robert Lindsay: chief, Domestic Research Division, Federal Reserve Bank of New York.

Stanley G. Long: instructor in economics, Iowa State University of Science and Technology (formerly Iowa State College).

Robert Lorimer: assistant professor of accounting, Los Angeles State College.

Bevers D. Mabry: assistant professor in economics, Bowling Green State University.

Duane F. Marble, University of Washington: assistant professor of real estate, School of Business Administration, University of Oregon.

Thomas A. Markinsek, Montana State University: associate professor of economics, Southern Illinois University.

Peter Max, Cornell University: assistant professor of economics, Carnegie Institute of Technology.

Frederick E. May: assistant professor of marketing, University of Missouri.

Maurice L. McCarty: instructor in management, Los Angeles State College.

Harold F. McClelland: assistant professor of economics, Claremont Men's College.

Floyd B. McFarland: instructor in economics, Texas College of Arts and Industries.

Robert B. McKersie: assistant professor of industrial relations, Graduate School of Business, University of Chicago.

Robert S. Merrill, University of Chicago: assistant professor of economics and anthropology, University of Minnesota.

Jerome W. Milliman, University of California, Los Angeles: associate professor, School of Business, Indiana University.

David Moment: instructor in business administration, Harvard Graduate School of Business Administration.

Victor P. Morris, University of Oregon: chief of Party of University of Oregon Advisory Group to Korean Economic Development Council.

George G. S. Murphy, University of Washington: assistant professor of economics, Stanford University.

Alan P. Murray: instructor in economics, Lafayette College.

Ana P. Navarro: instructor in economics, University of Michigan.

Edward A. Nelson, Baylor University: associate professor of finance, Los Angeles State College.

Paul E. Nelson, Jr., Denison University: Department of Agriculture, Washington, D.C.

Ralph L. Nelson, Northwestern University: research staff of the National Bureau of Economic Research.

Raymond E. Nelson: visiting assistant professor of insurance, School of Business Administration, University of Oregon.

Alvan J. Obelsky, University of Miami: instructor in economics, University of Michigan.

Goran Ohlin, Stanford University: associate professor of economics, Columbia University.

James A. Papke, Cornell University: assistant professor of economics, Wayne State University.

Robert W. Paterson, University of South Carolina: professor of business research and director, Bureau of Business and Economic Research, University of Missouri.

William A. Paton, University of Michigan: visiting professor of accounting, Graduate School of Business, University of Chicago.

Arnold A. Paulsen: research staff, Agricultural Adjustment Center, Iowa State University of Science and Technology (formerly Iowa State College).

Harold A. Petersen: appointed to faculty, Wooster College.

Robert B. Pettengill, recently Rollins College: visiting professor of economics, American University of Beirut.

Robert Piron: instructor in economics, Iowa State University of Science and Technology (formerly Iowa State College).

Mark A. Plivelic: instructor, department of accounting, School of Business Administration, University of Pittsburgh.

Hoyt Price: counselor of Embassy for Economic Affairs, American Embassy, Bern, Switzerland.

Howard Raiffa: professor of business administration, Harvard Graduate School of Business Administration.

Kenneth J. Rea: special lecturer in economics, University of Saskatchewan.

Marcel Richter, Brandeis University: assistant professor of economics, University of Minnesota.

William J. Ruckstuhl: assistant professor of business and economics, University of Maine

Angel Rugina: associate professor of economics and finance, College of Business Administration, Northeastern University.

Harvey B. Safeer: instructor in economics, University of Minnesota.

Frank J. Scardilli: lecturer in business administration and member of the faculty, Harvard Graduate School of Business Administration.

Donald Scoles, University of Southern California: visiting associate professor of insurance, Los Angeles State College.

Robert P. Shay, University of Maine: research staff, National Bureau of Economic Research.

Shih-Yen Wu, University of Minnesota: assistant professor of economics and statistics, Los Angles State College.

Carl S. Shoup: McVickar professor of political economy, Columbia University.

Bernard Shull: lecturer in economics, Wharton School, University of Pennsylvania, spring term 1959.

Franklin R. Shupp: instructor, department of economics, University of Illinois.

Robert A. Sigafoos, The Pennsylvania State University: urban economist, Stanford Research Institute, South Pasadena, California.

Norman J. Simler, St. Thomas College: assistant professor of economics, University of Minnesota.

Vidyapati Singh, Western Reserve University: assistant professor of business and economics, Illinois College.

Robert L. Slighton: assistant professor of economics, Stanford University.

Paul E. Smith, University of Michigan: assistant professor of economics, San Fernando Valley State College.

William P. Smith: instructor in economics, Michigan State University.

Winfield Smith: assistant professor of business economics, Graduate School of Business, University of Chicago.

Donald M. Soule, University of Illinois: staff of Bureau of Business Research, University of Kentucky.

Richard C. Spangler, Whittier College: assistant professor of economics, Montana State University.

John H. D. Spencer, Mississippi State University: associate professor of economics, College of Business Administration, University of Georgia.

Walter A. Spivey: visiting associate professor of business administration, Harvard Graduate School of Business Administration.

William R. Stevenson; instructor in marketing and foreign commerce, Wharton School, University of Pennsylvania, spring term 1959.

Max D. Stewart, Waterloo College: economist on staff of the Combines Investigation Branch, Department of Justice, Ottawa, Ontario.

Arthur M. Stupay: instructor in economics, Wharton School, University of Pennsylvania, spring term 1959.

David M. Sturges: instructor in finance, Wharton School, University of Pennsylvania, spring term 1959.

James E. Sutton: instructor in economics, University of Michigan.

Paul M. Sweezy: visiting professor of economics, Cornell University.

Perry D. Teitelbaum: two-year appointment as professional officer, Division of Economic and Technical Assistance, International Atomic Energy Agency.

Raymond D. Thomas, emeritus Oklahoma State University: professor of economics, Drury College.

Gerald Thompson, Ohio Wesleyan University: associate professor of applied mathematics and industrial administration, Graduate School of Industrial Administration, Carnegie Institute of Technology.

Hans B. Thorelli: professor of business administration, Graduate School of Business, University of Chicago.

Arthur N. Turner: assistant professor of business administration, Harvard Graduate School of Business Administration.

Raymond Vernon: professor of international trade and investment, Harvard Graduate School of Business Administration.

John R. Virts: visiting lecturer in economics, Indiana University, first semester 1959-60.

Lewis B. Ward: professor of business research, Harvard Graduate School of Business Administration.

Stanislaw Wasowski: assistant professor of economics, Georgetown University.

Max J. Wasserman, U.S. Department of Commerce and Howard University: visiting professor of diplomacy and international commerce, Patterson School of Diplomacy and International Commerce, University of Kentucky.

Robert H. Watson: assistant professor of accounting, Los Angeles State College.

Richard S. Weckstein, University of Rochester: economic department, Williams College.

Paul A. Weinstein: instructor in economics, Oklahoma State University.

Byron White, University of Texas: associate professor of economics, Appalachian State Teacher College, Boone, N.C.

Raymond E. Willis, formerly Boston University: lecturer, University of Minnesota.

H. Lawrence Wilsey: partner of Booz, Allen & Hamilton, management consultants.

David M. Winch: special lecturer in economics, University of Saskatchewan.

Alan R. Winger, University of Michigan: associate economist, Federal Reserve Bank of San Francisco.

Donald Winkelmann: instructor in economics, University of Minnesota.

Francis O. Woodard: instructor in economics, University of Nebraska.

H. Edward Wrapp: professor of business administration, Harvard Graduate School of Business Administration.

Wesley Yordon: instructor in economics, University of Colorado.

Shih-Cheng Yu: assistant professor of business and economics, University of Maine.

Victor Zarnowitz: associate professor of finance, Graduate School of Business, University of Chicago.

Manuel Zymelman: assistant professor of economics, College of Business Administration, Northeastern University.

### *Leaves for Special Appointments and Assignments*

Frank C. Child, Michigan State University: assigned to Vietnam Project for two years.

Joseph W. Conard, Swarthmore College: to serve on staff of the Commission on Money and Credit 1959-60.

James S. Earley, University of Wisconsin: on staff of the National Bureau for Economic Research first semester current academic year.

Andrew G. Frank, Michigan State University: to participate in establishing Monteith College at Wayne University.

James K. Hall, University of Washington: project director of Korean Tax Advisory Group, which Brookings Institution will maintain in Seoul under contract with International Cooperation Administration and the Republic of Korea.

R. M. Havens, University of Alabama: to lecture in economics, University of Dublin, as a Fulbright scholar.

Don D. Humphrey, Duke University: member of senior research group in India arranged by The Center of International Studies, Massachusetts Institute of Technology.

John M. Hunter, Michigan State University: to continue a second year as director of the Institute of Economic Research, University of the Andes, Bogota, Colombia.

William O. Jones, Stanford University: visiting lecturer in Agricultural Economics Research Institute, University of Oxford.

Svend Laursen, Brandeis University: chief economist, Tanganyika Mission, International Bank for Reconstruction and Development.

Harold M. Levinson, University of Michigan: appointed to a special staff of the Joint Economic Committee for first semester.

Peter F. M. McLoughlin, University of Texas: lecturer in economics, University of Khartoum, Sudan.

Chandler Morse, Cornell University: to head a mission this fall to Bechuanaland on behalf of the British government.

Anthony M. Tang, Vanderbilt Universtiy: to join staff of the Institute of Social and Economic Research, Osaka University, 1959-60.

### *Resignations*

Merrill D. Bartlett: School of Business Administration, University of Maine.

David S. Carlson: School of Business Administration, University of Pittsburgh.

Daniel C. Hamilton: Wharton School, University of Pennsylvania.

Robert A. Mundell: Stanford University—at Bologna Center of the Johns Hopkins University 1959-60.

Ralph L. Nelson: School of Business, Northwestern University.

Alan R. Plotnick: University of Maine.

James T. S. Porterfield: Harvard Graduate School of Business Administration.

Francis J. Riepl: Wharton School, University of Pennsylvania.

Richard H. Slavin: School of Business Administration, University of Pittsburgh.

Theodore R. Wilson: Wharton School, University of Pennsylvania.

### *Miscellaneous*

Carl C. Malone, Iowa State University of Science and Technology (formerly Iowa State College): award for Superior Service from U.S. Department of Agriculture for role in program carrying out interdepartmental approach to problems of economic and social growth in Iowa.

Mabel F. Timlin, University of Saskatchewan: elected president of the Canadian Political Science Association and given one of the first Senior Awards of the Canada Council.

# FIFTY-SIXTH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1959, and theses undertaken in the same period.

## General Economics; Methodology

### *Degrees Conferred*

ROBERT D. BELL, Ph.D. Cornell 1959. Methodological problems and possibilities in farm business analysis using inter-farm production functions.

JOHN A. DAVIS, Ph.D. Alabama 1957. Ethics, economics & individualism.

PETER P. DORNER, Ph.D. Harvard 1959. An analysis of the economic position and potential of the American Indians and their resources.

### *Thesis in Preparation*

EDWIN L. CAREY, B.S. Illinois State Normal Univ. 1949; M.B.A. Indiana 1950. An identification of the issues and points of view relative to the teaching of elementary economics. *Indiana.*

## Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

### *Degrees Conferred*

K. JANAKI KUTTY AMMA, Ph.D. Fletcher School 1959. United Nations technical assistance: a study in decision-making for allocation of resources

ALBERT K. ANDO, Ph.D. Carnegie Inst. Technology 1959. A contribution to the theory of economic fluctuations and growth.

JOHN A. BERGERON, Ph.D. Mass. Inst. Technology 1959. Classical theory of economic growth.

WILLIAM G. BOWEN, Ph.D. Princeton 1959. The wage-price issue: a theoretical analysis.

KALMAN J. COHEN, Ph.D. Carnegie Inst. Technology 1959. Computer models of the shoe, leather, hides sequence.

WILLARD L. EASTMAN, Ph.D. Harvard 1959. Linear programming with pattern constraints.

THOMAS J FINN, JR., Ph.D. Harvard 1959. An application of the theory of production to the analysis of inventory fluctuations.

LOUIS A. FOURT, Ph.D. Chicago 1959. Empirical income elasticities of demand for food and its component values produced by Farmers Manufacturers, and other marketing agencies in the United States, 1929-1956.

ARTHUR M. FREEDMAN, Ph.D. Pennsylvania 1959. Issues of modern interest doctrines.

FLOYD E. GILLIS, JR., Ph.D. Harvard 1959. The practice of accounting and the theory of the firm.

CRAUFORD D. W. GOODWIN, Ph.D. Duke 1958. Canadian economic thought 1814-1914.

FRED M. GOTTHEIL, Ph.D. Duke 1959. The economic predictions of Karl Marx: an examination of Marxian economic theory.

MARY T. HAMILTON, Ph.D. Pennsylvania 1959. The empirical bases of demand theory.

ARTHUR W. HARRIGAN, Ph.D. New York 1959. The economic criteria of wage determination.

RASOOL M. H. HASHIMI, Ph.D. Wisconsin 1958. Studies in functional income distribution.

THOMAS IWAND, Ph.D. Oregon 1959. Studies in the pure theory of capital.

HARRY L. JOHNSON, Ph.D. Virginia 1959. Price behavior and product differentiation.

DALE W. JORGENSON, Ph.D. Harvard 1959. Duality and stability in dynamic input-output analysis.

TOSHINOSUKE KASHIWAZAKI, Ph.D. North Carolina 1958. A critical study of recent contributions to the theory of welfare economics.

ALBERT R. MILLER, JR., Ph.D. Harvard 1959. Doctrinal origins of Say's law of markets.

EDMUND S. PHELPS, JR., Ph.D. Yale 1959. A test for the presence of cost inflation in the U.S. economy 1955-57.

OWEN H. SAUERLENDER, Ph.D. Minnesota 1958. Level of aspiration and classical utility analysis.

NESTOR TERLECKYJ, Ph.D. Columbia 1959. Factors underlying changes in productivity.

DESIDER VIKOR, Ph.D. St. Louis 1959. Austrian romantic school (Adam Mueller, Spann and the present universalists).

## *Theses in Preparation*

JOHN R. ALLAN, B.A. McMaster 1955. The economics of Irving Fisher. *Princeton.*

HELEN BOTSAI, B.A. Allegheny College 1955; M.A. Pittsburgh 1956. Reappraisal of the theory of retardation and industrial growth. *Pittsburgh.*

GLEN L. BURRESS, M.A. Cincinnati 1958. The disaggregation of the consumption function. *Cincinnati.*

YOUNGIOB CHUNG, B.S. California (Los Angeles) 1952. The role of the state in formation and allocation of capital. *Columbia.*

MARTIN H. DAVID, B.A. Swarthmore 1955; M.A. Michigan 1957. Demographic factors in consumer expenditure.

RICHARD G. DAVIS, B.A. Amherst 1955. Studies in the application of utility theory to the formation of objective functions in military assignment problems. *Princeton.*

LOUIS DE ALESSI, B.A. California (Los Angeles) 1954; M.A. 1955. Inflation-caused wealth redistribution: an empirical test of the debtor-creditor hypothesis based on a sample of British firms. *California (Los Angeles).*

MADELINE DONNER, B.A. Texas 1930; M.A. 1931. Consumer decisions, intervening experience and market action. *Columbia.*

DANIEL ELLSBERG, B. A. Harvard 1952; M.A. 1954. Non-cooperative games and bargaining. *Harvard.*

JOHN HALDI, B.A. Emory 1951; M.A. Stanford 1956. Increasing returns, external economies, and economic growth. *Stanford.*

VSEVOLOD HOLUBNYCHY, B.S. Columbia 1953. The Soviet theory of value. *Columbia.*

ESTELLE JAMES, B.S. Cornell 1956. A reconsideration of theoretical criteria for optimum investment planning. *Mass. Inst. Technology.*

ARNOLD B. LARSON, B.S. Minnesota 1949; M.S. 1951. Evidence on the temporal distribution of price effects of new information. *Stanford.*

RICHARD A. MILLER, B.A. Oberlin 1952; M.A. Yale 1957. An empirical study of exclusive dealing in commodity distribution. *Yale.*

DANIEL ORR, B.A. Oberlin 1954; M.A. Princeton 1956. Production stability and inventory variation. *Princeton.*

MICHAEL RIEBER, B.A. Syracuse 1952; M.S. Tennessee 1954. Competitive bidding: an analysis of problems. *Mass. Inst. Technology.*

KAZUO SATO, B.A. Hokkaido Univ. 1954; M.A. Yale 1956. Prices, costs and margins in manfacturing. *Yale.*

CHARLES L. SCHULTZE, B.A. Georgetown 1948; M.A. 1949. A theory of inflation. *Maryland.*

EUGENE SNOLENSKY, B.A. Brooklyn College 1952; M.A. American 1956. Economic location and the size distribution of income, 1919-1956. *Pennsylvania.*

RICHARD E. VOSBURGH, B.S. Miami 1955; M.B.A. Indiana 1956. A history of the theories of personal consumption from 1919-1959. *Indiana.*

## Economic History; Economic Development; National Economies

### *Degrees Conferred*

ALI AHMED ATTIGA, Ph.D. Wisconsin 1959. Opportunities and problems of using the United States surplus food to increase capital formation in underdeveloped countries.

MARTO A. BALLESTEROS, Ph.D. Chicago 1958. Argentine agriculture, 1908-1954; a study in growth and decline.

EVELYN M. BARAN, Ph.D. Radcliffe 1959. The economic development of Venezuela.

STERLING BRUBAKER, Ph.D. California (Berkeley) 1959. Impact of federal government activities on California's development since 1930-1956.

CHARLES CONKLIN, Ph.D. Pittsburgh 1959. A study of the economic development of the northern panhandle of West Virginia.

CHESTER COOPER, Ph.D. American 1959. The influence of the Indian money lenders in the economic development in Burma.

IAN M. DRUMMOND, Ph.D. Yale 1959. Capital markets in Australia and Canada 1895-1914: a study in colonial economic history.

MOSTAFA ELM, Ph.D. Syracuse 1959. Governmental economic planning in Iran.

MAURICE ERNST, Ph.D. Columbia 1959. Measurement of Polish industrial growth, '37, '46-'55.

MARK KARP, Ph.D. Fletcher School 1959. The economic development of Somalia under the trusteeship.

BERDJ KENADJIAN, Ph.D. Harvard 1959. Disguised unemployment in underdeveloped countries.

GEORGE K. LEWIS, Ph.D. Texas 1959. An analysis of the institutional status and role of the petroleum industry in Mexico's evolving system of political economy.

EDWARD S. LITTLE, Ph.D. American 1959. National resources of Spain as a basis for an industrial economy.

JOHN E. MOES, Ph.D. California (Berkeley) 1959. Economic development of Indonesia.

OTTO MORGENSTERN, Ph.D. California (Berkeley) 1958. Problems of economic development in South Africa.

ALEX H. MORRISON, Ph.D. Virginia 1959. The impact of industry on a rural area in northern Virginia: a case study of developments in Warren and surrounding counties, 1930-1954.

SAMUEL PAUL, Ph.D. Syracuse 1959. Investment decisions in Indian economic planning.

RICHARD L. PFISTER, Ph.D. Mass. Inst. Technology 1959. The commodity balance of trade of the Pacific northwest for selected years, 1929-1955.

HENRY ROSOVSKY, Ph.D. Harvard 1959. Japanese capital formation 1868-1940.

MARVIN E. ROZEN, Ph.D. California (Berkeley) 1958. British national investment policy, 1945-51.

JULIUS RUBIN, Ph.D. Columbia 1959. Imitation by canal or innovation by railway: a comparative study of the response to the Erie Canal in Boston, Philadelphia and Baltimore.

NORTON C. SEEBER, Ph.D. California (Berkeley) 1959. Investment criteria and economic development.

GEORGE STALLER, Ph.D. Cornell 1959. Czechoslovak industry after the war.

ALFRED L. THIMM, Ph.D. New York 1959. The role and opinion of the businessman during the reform-progressive era (1800-1913).

EDGAR L. TURGEON, Ph.D. Columbia 1959. Labor productivity in basic industries of USSR, 1928-1955, as measured by production costs.

CLIFTON R. WHARTON, JR., Ph.D. Chicago 1958. A case study of the economic impact of technical assistance: capital and technology in the agricultural development of Minas Gerais, Brazil.

BYRON WHITE, Ph.D. Texas 1959. Cuba and Puerto Rico: a case study in comparative economic development policy.

## *Theses in Preparation*

ROBERT K. ARNOLD, B.A. California 1947. The future growth of selected industries in California: a study on the future economic development of California to 1980. *California (Berkeley).*

ROBERT T. AVERITT, B.A. North Texas State 1951; M.A. Texas 1957. Contributions of the institutionalist school to development theory. *Texas.*

THEODORE BOREK, B.A. West Liberty 1947; M.A. Pittsburgh 1948. Factors contributing to the growth and development of the arid southwest, with particular reference to the Phoenix metropolitan area. *Pittsburgh.*

LILLIAN BRIDGER, B.A. Hunter 1956; M.A. New York 1958. The Histadrut and Israel's industrial development in the first post-independence decade. *New York.*

MUHAMMAD I. CHAUDRY, B.A. Lahore Univ. 1951; L.L.B. 1953; M.A. 1955. An evaluation of the first five year plan of Pakistan. *Harvard.*

YU MIN CHOU, B.A. Illinois 1954; M.A. 1955. The role of long term capital movement in the economic development of Southeast Asia. *Illinois.*

JAMES N. DAVIS, JR., B.S. Arkansas 1952; M.B.A. 1952. Economic impact of industrialization on a community. *Arkansas.*

JIM E. DAVIS, B.A. Oklahoma 1952; M.A. 1957. Economic development of southern Italy. *Wisconsin.*

JOHN D. DEFOREST, B.S. Kansas State 1955; M.A. 1957. Obstacles to economic development in underdeveloped countries. *Iowa.*

JOSÉ ENCARNACION, JR., Ph.B. Univ. Philippines 1950; M.A. 1954; M.A. Princeton 1958. Investment criteria for underdeveloped countries. *Princeton.*

JOHN S. EVANS, B.A. Texas 1953; M.A. 1955. Mexican public finance and its relation to economic development. *Wisconsin.*

OLADUNJOYE FASHOLA, B. A. Ohio Wesleyan 1954; M.A. Illinois 1956. Income taxation in Nigeria in relation to economic development. *Illinois.*

MOSTAFA FIKRY, B.S. Fouad I Univ. 1942; M.S. Univ. Alexandria 1953. The place of cooperatives in the Egyptian economy. *Wisconsin.*

HENRY E. FINLEY, B.A. Florida A & M 1952; M.B.A. Indiana 1957. Role of domestic resources in economic development of selected underdeveloped countries: India, Pakistan and Ceylon. *Indiana.*

ROBERT W. FOGEL, B.A. Cornell 1948. The influence of the railroads on American economic growth, 1830-1890. *Johns Hopkins.*

CHARLES C. FROST, B.A. Tufts 1951; M.A. Fletcher School 1955. The economic development of communist China: a case study in the application of Soviet development doctrine to a labor-surplus economy. *Fletcher School.*

MARVIN GOODSTEIN, B.S. New York 1950. Estimation and analysis of Philippine real product. *Cornell.*

EDMUND HILL, B.Comm. McGill 1950; M.A. Pittsburgh 1955. A comparative study of the United States and Canadian economies as based on key indicators since 1926. *Pittsburgh.*

LIANG-LIN HSIAO, M.A. Michigan 1948. China's foreign trade 1875-1936. *New York.*

MOHAMMED IMADY, M.A. New York 1958. Economic programming in the Syrian region of the United Arab Republic. *New York.*

BADRI DASS JAIN, B.A. Panjab Univ. 1939; M.A. 1942. The deficit finance and economic development with special reference to India. *Vanderbilt.*

SUN M. KAHNG, B.S. Seoul National Univ. Korea; M.A. Indiana 1957. A serviceable economic model for Korea. *Indiana.*

YOKO KAWASHIMA, B.A. Keio Univ. 1955; M.A. Radcliffe 1957. Productivity change during the economic development: textile and steel in Japan. *Harvard.*

TAUFIG KHAN, B.A. Osmania Univ. (Hyderabad) 1945; M.A. Georgetown 1954. Consumption patterns in underdeveloped countries. *Pennsylvania.*

AUGUSTIN KINTANAR, JR., B.A. Univ. Philippines 1951; M.A. Chicago 1954. An analysis of the effects of certain modifications in the tax structure on the rate of economic development in the Philippines. *Yale.*

PONG SEOK LEE, B.A. Simpson 1956; M.A. Yale 1958. The docrtine of balanced growth as an investment criterion: the cases of Japan and Korea. *Yale.*

SAMIR MAKDISI, B.A. American Univ. Beirut 1953; M.A. 1955. Syria: the role of government in economic development, 1945-57. *Columbia.*

PETER F. M. MCLOUGHLIN, B.A. British Columbia 1951. Comparative factors in the economic development of three East African tribes. *Texas.*

AHMAD MINAI, B.A. Tehran Univ. 1943; M.A. Glasgow Univ. 1946. Economic development of Iran under the reign of Raza Shah, (1920-1940). *American.*

ROBERT A. MINICK, B.S. North Texas State 1951; M.S. 1955. The relationship of foreign direct investments to the development of manufacturing in Brazil. *Texas.*

SIDDHESHWAR MITTRA, B.C. St. Johns College (India) 1949. Role of banking in the economic development of under-developed countries with special reference to India. *Florida.*

RICHARD MORSE, B.A. Dartmouth 1946; M.A. Harvard 1958. Indian industrial development. *Harvard.*

TOYOKI OKABAYASHI, M.Econ. Tohoku Univ. 1950. The role of resources in the economy of the United States and Japan. *Oregon.*

P. G. K. PANIKAR, B.A. Annamalai Univ. (India) 1945. Rural savings in India. *Vanderbilt.*

PUSHKAR NATH PANT, B.A. Patna Univ. 1953; M.A. Banaras Hindu Univ. 1956. Fiscal policy and economic development of Nepal. *Vanderbilt.*

ERIC E. PEDERSEN, B.A. Queens 1953. Denmark's planned economy, and its relationship to her international financial position. *New York.*

ZORA B. PROCHAZKA, B.A. Univ. Geneva 1949; M.A. Univ. Paris 1951. Economic development of Czechoslovakia during the interwar period. *Harvard.*

BURNS R. RAFFERTY, B.B.A. New Mexico 1950; M.A. 1956. Theories of economic development. *Texas.*

KINICHIRO SAKURAI, M.A. Hitotsubashi Univ. 1947; M.A. Syracuse 1954. Financial aspect of economic development in Japan from 1869 to present. *Syracuse.*

MARTIN H. SEIDEN, B.A. City (New York) 1954. The role of military spending in the United States 1948-1958. *Columbia.*

MAURICE SELDIN, B.S. California (Los Angeles) 1953; M.B.A. 1957. An analysis of the impact of policies of business firms on urban problems. *Indiana.*

LUIS F. SILVA-RECIO, B.A. Univ. Puerto Rico 1951; M.S. Wisconsin 1952. Labor costs and industrialization in Puerto Rico. *Wisconsin.*

ERNEST STERN, B.A. Queens 1955; M.A. Fletcher School 1956. Theory and reality in development: case studies in Latin America. *Fletcher School.*

DONALD TAILBY, B.A. Rutgers 1950; M.A. 1956. Chapters from the business career of William Constable. *Rutgers.*

KENJI TAKEUCHI, B.A. Kwansei Gakuin Univ., Nishinomiya 1956. The special features of foreign investment in Japan 1950-1959 and its effects upon her economic growth. *Duke.*

CHARLES M. THOMPSON, JR., M.L. Pittsburgh 1956. The influence of location theory on local economic problems. *Pittsburgh.*

JOHN C. THOMPSON, B.A. Illinois 1954; M.B.A. Harvard 1956. An examination and appraisal of the application of national income and product accounting and balance of payments analysis to the problem of regional income and growth. *Illinois.*

MIKOTO USUI, M.A. Nagoya Univ. 1955. The process of self-sustained growth under conditions of economic dualism: with a special emphasis on the pliopoly patterns. *Mass. Inst. Technology.*

MANUEL VELEZ-MONTES, B.S. Columbia 1950; M.A. Columbia n.d. Strategic factors governing the economic growth of underdeveloped regions. *Rutgers.*

GEORGE WANG, M.A. St. John's 1945. A comparison of the first five-year plans of China and India. *Columbia.*

DAVID ZUTA, LL.M. Hebrew Univ. 1955; B.A. 1957. Economic development and the structure of manufacturing. *Johns Hopkins.*

## Statistical Methods; Econometrics; Social Accounting

### *Degrees Conferred*

KIICHIRO KOGIKU, Ph.D. Wisconsin 1959. A critique of world-wide econometric forecasting.

MARION C. PHILLIPS, Ph.D. Oklahoma 1959. Methods of estimating professional income in the counties of Oklahoma.

SIDNEY SONENBLUM, Ph.D. Columbia 1959. Regional economic analysis and social accounts: an interindustry application.

BENJAMIN H. STEVENS, Ph.D. Mass. Inst. Technology 1959. Interregional linear programming.

### *Theses in Preparation*

JOHN O. BLACKBURN, B.A. Duke 1951. A balance sheet for the nation: a study in concepts. *Florida.*

JOHN S. Y. CHIU, B.A. National Taiwan Univ. 1953; M.A. Kentucky 1955. An application of mathematical economics. *Illinois.*

MICHIO HATANAKA, M. A. Tokyo Univ. 1949. The workability of the input-output analysis. *Vanderbilt.*

RICHARD MARTIN, B.A. Harvard 1950; M.S. Cornell 1955. The distribution of proceeds in the steel industry 1935-1955: a study in methodology. *Cornell.*

JAMES L. MCKENNY, B.S. Oklahoma A. & M. 1948; M.S. Purdue 1952. Simultaneous program scheduling on electronic computers. *California (Los Angeles).*

EUSTACE G. PANAS, B.A. National Univ. Athens 1950; M.A. Harvard 1958. A survey of recent developments in the field of social accounting. *Harvard.*

BERNT P. STIGUM, B.A. Dartmouth 1956; M.A. Harvard 1959. The application of stochastic processes to economic time series. *Harvard.*

BENJAMIN TENCER, B.S. Hobart 1956. A study of economic development problems by use of electronic analog simulation. *Mass. Inst. Technology.*

RAYMOND E. WILLIS, JR., B.S. Rensselaer 1951; M.B.A. Boston Univ. 1956. The use of harmonic analysis in analyzing phase relationships between economic series. *Mass. Inst. Technology.*

## Economic Systems; Planning and Reform; Cooperation

### *Degrees Conferred*

BELA A. BALASSA, Ph.D. Yale 1959. Planning in theory and practice: the Hungarian experience.

GEORGE DALTON, Ph.D. Oregon 1959. Robert Owen and Karl Polanyi as socio-economic critics and reformers of industrial capitalism.

JOHN S. HOYT, JR., Ph.D. American 1959. An investigation of the economics of Soviet locational theory and practice.

### *Theses in Preparation*

LORENZO M. BELOTTI, B.A. Midwestern 1954; M.A. 1955. The influence of Keynesian "Dirigisme" on Christian socialism in the postwar Italian economic policies. *Texas.*

ZBIGNIEW FALLENBUCHL, B.S. Univ. London 1951; M.A. Université de Montréal 1957. Capital formation and investment in the socialist economy. *McGill.*

MARSHALL I. GOLDMAN, B.A. Pennsylvania 1952; M.A. Harvard 1956. The economics of marketing in the Soviet Union: trade and distribution in a planned economy. *Harvard.*

GLOM ISARAPANDH, LL.B. and LL.M. Univ. Tammasat (Thailand). A comparison of the legal economic features of cooperative organization in the United States and Thailand. *Wisconsin.*

FREDERIC L. PRYOR, B.A. Oberlin 1955; M.A. Yale 1957. Long range economic planning in the German Democratic Republic. *Yale.*

FRED F. SCHILLER, B.S. St. Louis 1950. The emergence of economic rationality in Soviet collective farms 1955-1959. *Pennsylvania.*

## Business Fluctuations

### *Degrees Conferred*

LEO I. BAKONY, Ph.D. Washington 1959. The quarterly econometric model of the Canadian economy.

ROY J. CAMERON, Ph.D. Harvard 1959. Inflation in a dependent economy: Australia 1945-55.

MORRIS COHEN, Ph.D. Harvard 1959. Forecasting consumer and business behavior.

EDWARD CROSBY, Ph.D. New School for Social Research 1959. Post-World War II demand for automobiles in the United States.

MICHAEL C. LOVELL, Ph.D. Harvard 1959. Inventories and stability: an inter-industry analysis.

HERRMANN O. STEKLER, Ph.D. Mass. Inst. Technology 1959. Essays in economic forecasting.

### *Theses in Preparation*

HSUEH-LIN CHANG, B.A. National Chengchi (Nanking) 1946; M.A. New School 1956. The nature and causes of the post-war inflation in the United States. *New School.*

WARD S. CURRAN, B.A. Trinity 1957. The business community and the policy of full employment. *Columbia.*

CHARLES G. DRAKE, B.A. Westminster 1935; M.A. Washington (St. Louis) 1952. Unemployment insurance as a built-in stabilizer. *Missouri.*

EDWARD M. FOSTER, B.A. Claremont Men's College 1956. Sales anticipations and investment demand. *Mass. Inst. Technology.*

RICHARD A. HOLMES, B.A. Saskatchewan 1954; M.A. 1956. An appraisal of the Canadian survey of business investment intentions. *Indiana.*

ANTHONY SHUEN, LL.B. National Szechwan Univ. 1946; M.A. Fordham 1950. The influence of structural and institutional changes upon the cyclical stability of post-World War I American economy. *Fordham.*

RONALD P. WILLETT, B.S. Miami 1954; M.B.A. 1956. The application of local and regional business indicators for local and business forecasting. *Indiana.*

WESLEY J. YORDON, JR., B.A. Colorado 1956; M.A. Harvard 1958. The role of monopoly power in the wage-price spiral. *Harvard.*

## Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

### *Degrees Conferred*

CARL T. BREHM, Ph.D. Indiana 1958. An evaluation of the voluntary credit restraint program of 1951-1952.

JOHN T. BURKE, Ph.D. Michigan State 1958. Commercial bank treatment of bad debt losses, its relationship to the economic and accounting concepts of income measurement, and the effects of the federal income tax requirements.

JOHN J. CLARK, Ph.D. New York 1959. The variable bank account in mutual savings banking.

DANIEL COLDWELL, 3rd, Ph.D. Virginia 1959. The effects of general credit controls on nonfarm residential construction.

RUPERT M. CRAIG, Ph.D. California (Los Angeles) 1959. The uses and limitations of lease financing.

CLAYTON C. CURTIS, Ph.D. Indiana 1959. Institutional lenders in a local residential mortgage market Los Angeles County, 1946-1951.

DENNIS B. FORD, JR., Ph.D. Texas 1958. A study of functional growth in savings and loan organizations.

JOHN A. GALBRAITH, Ph.D. McGill 1959. Study of Canadian banking.

HARMON H. HAYMES, Ph.D. Virginia 1959. An investigation of the alleged discriminatory impact of general credit controls in periods of monetary stringency.

BUD R. HUTCHINSON, Ph.D. Southern California 1958. A study of debt adjustment in Michigan.

JAMES K. KINDAHL, Ph.D. Chicago 1958. The economics of resumption: the United States 1865-1879.

ALBERT LEVENSON, Ph.D. Columbia 1959. Differential in interest rates and the cost of commercial bank lending.

MOHAMED SAMI MOHAMED, Ph.D. New York 1959. The commercial banking system of Egypt.

BASIL J. MOORE, Ph.D. Johns Hopkins 1959. The effects of counter-cyclical monetary policy on the Canadian chartered banks, 1935-1957.

HANS O. SCHMITT, Ph.D. California (Berkeley) 1959. Monetary policy in the political economy of Indonesia.

STANLEY C. SILVERBERG, Ph.D. Yale 1959. Asset preference and the structure of interest rates.

DAVID C. SMITH, Ph.D. Harvard 1959. Monetary policy and economic growth in an open economy: the Canadian experience 1950-56.

DONALD TYREE, Ph.D. Texas 1959. The small loan industry in Texas.

### *Theses in Preparation*

RACHEL B. BALBACH, B. A. Reed 1953. The effect of inflation on the wealth of commercial banks. *California.*

ABDELHAK BELKORA, B.S. Colorado 1956. Monetary policy in two post-war recessions: 1953-54, 1957-58. *Colorado.*

JAMES R. BOBO, B.S. Florence State 1950; M.A. Peabody 1952. Financial intermediaries and the problem of monetary control. *Louisiana State.*

NORMAN N. BOWSHER, LL.B. Ohio Northern 1947; B.A. 1948; LL.M. Indiana 1949. Flows of funds between regions of the United States. *Indiana.*

ALFRED BRANDT, J.D. Univ. Bonn (Germany) 1934. Central banking in Germany from 1948 to 1958. *New York.*

WILLIAM R. BRYAN, B.S. Ball State Teachers 1954; M.S. Wisconsin 1958. An analysis of deposit flows and portfolio composition of member banks in the 8th Federal Reserve district, 1951-1959. *Wisconsin.*

ROBERT L. COMEAU, B.A. St. Francis Xavier 1947; M.A. 1952. Financial intermediaries as a factor in monetary instability: the Canadian case. *Brown.*

ARNOLD C. COOK, B.A. Univ. Western Australia 1948; M.A. Harvard 1958. Some aspects of central banking in Australia, 1945-57. *Harvard.*

JOSEPH L. CRAYCRAFT, M.A. Chicago 1957. The effect of Federal Reserve policies on the money supply. *Cincinnati.*

ABEL GARRIDO-RUIZ, B.A. Univ. Mexico 1953; M.S. Wisconsin 1958. Problems of central banking in Mexico. *Wisconsin.*

GEORGE G. KAUFMAN, B.A. Oberlin 1954; M.A. Michigan 1955. Federal Reserve policy and effects on commercial banks and selected financial intermediaries, January 1957-March 1959. *Iowa.*

MAW-LIN LEE, B.A. National Taiwan 1954; M.A. Montana State 1956. Consumer financing and economic development—an empirical study of the American economy since 1900. *Wisconsin.*

SEUNG-YUN LEE, B.A. Adams State 1955; M.A. Missouri 1957. Objectives and potentialities of monetary policy in underdeveloped countries. *Wisconsin.*

CHARLES P. LEON, B.S. City (New York) 1942; M.A. Columbia 1945. The National Bank of Cuba: a study in institutional change. *New York.*

BRUCE K. MACLAURY, B.A. Princeton 1953; M.A. Harvard 1958. Canadian postwar monetary policy. *Harvard.*

JOHN J. MCCALL, B.A. Notre Dame 1955; M.B.A. Chicago 1957. Demand for money and the wealth hypothesis. *Chicago.*

DAVID B. MCCALMONT, B.A. Yale 1932; LL.B. 1935. Federal Reserve policies concerning the gold reserves of individual Reserve banks. *Johns Hopkins.*

MARK T. MCDONNELL, B.A. St. John's 1948; M.A. Minnesota 1955. Principles of finance planning by financial institutions. *Wisconsin.*

ROBERT J. OPPITZ, B.A. McKendree 1947; M.A. Washington (St. Louis) 1952. Rate regulation in automobile sales financing. *Northwestern.*

DANILO ORESCANIN, B.S. West Virginia 1953; M.B.A. Indiana 1954. The management of savings and loan associations. *Indiana.*

DAVID J. OTT, B.A. North Texas State 1955; M.A. 1956. Financial intermediaries in Japanese economic development. *Maryland.*

BRAXTON I. PATTERSON, B.S. Bradley 1949; M.A. 1955. Cash assets of selected financial institutions. *Michigan State.*

RICHARD S. PETERSON, B.A. Washington State 1953. Demand for money by firms in the United States. *California (Berkeley).*

MURRAY R. ROSEMAN, B.A. Wisconsin 1953; M.A. Harvard 1959. Mutual savings banks as source of savings funds. *Harvard.*

RATEB SHALLAH, B.A. Oxford 1955. Monetary policy of the United Arab Republic. *California (Berkeley).*

BYONG SHIN, B.A. American 1953; M.A. 1954. Monetary and fiscal policy in South Korea, 1954-58. *Columbia.*

DONALD A. WALKER, B.A. Southwest Texas State Teachers 1952; M.A. Texas 1956. Nacional Financiera of Mexico. *Harvard.*

## Public Finance; Fiscal Policy

### *Degrees Conferred*

NORMAN ADLER, Ph.D. Wisconsin 1958. The tax administration of the individual income tax with respect to the receipt of wages and salaries, interest and dividends.

WILLIAM B. BENTSEN, Ph.D. Wisconsin 1959. Capital allowances and the concept of income: a study in British taxation.

ROBERT A. CARR, Ph.D. Southern California 1959. An economic appraisal of the public financial agencies in the housing sector of the United States economy; their origin, operations, and contributions since 1930.

PRABHAS CHAKKAPHAK, Ph.D. New York 1959. The major economic and financial problems of Thailand.

SO YONG CHUNG, Ph.D. Washington 1959. Impacts of federal-state reciprocal deduction of income tax.

SEYMOUR FIEKOWSKY, Ph.D. Harvard 1959. On the economic effects of death taxation in the United States.

WAYLAND D. GARDNER, Ph.D. Wisconsin 1958. The income tax compliance of farm income.

IRVING GOFFMAN, Ph.D. Duke 1959. Erosion of the personal income tax base in Canada and the United States.

SEVIM GORGUN, Ph.D. Syracuse 1959. Classical and contemporary theories of taxation.

CHARLES M. HUMMEL, Ph.D. Virginia 1959. The development of a quantitative criterion for the allocation of highway investment in Virginia.

JANET A. K. MESSING, Ph.D. New York 1959. History of federal taxation in the United States, a theoretical and historical evaluation of the causes and effects of the federal tax structure; a review of attitudes towards taxation.

ROY E. MOOR, Ph.D. Harvard 1959. Federal income taxation of life insurance companies.

JAMES PAPKE, Ph.D. Cornell 1959. An empirical analysis of the relative impact on corporations of a tax on value-added product.

HECTOR J. RIVERA, Ph.D. New York 1959. Income tax development and procedure in Puerto Rico.

JERRY P. SIMPSON, Ph.D. Oklahoma 1959. Oklahoma state debt.

HERBERT STEIN, Ph.D. Chicago 1958. The federal budget, debt and economic stability.

CLARA SULLIVAN, Ph.D. Columbia 1959. The value added tax.

RAYMOND F. VALENTI, Ph.D. Syracuse 1959. Federal taxation of life insurance companies: the savings institution approach.

### *Theses in Preparation*

LAURIE D. BELZUNG, B.J. Texas 1948; M.A. 1950. The economic implications of oil and gas taxation. *Texas.*

PETER C. BRIANT, B. of Commerce McGill 1951. The effects of the corporation income tax on the rate of return on common stock equity between the years 1926-1955. *Michigan.*

ALVIN A. BURNETT, B.Sc. McGill 1955; M.A. 1956. Financing municipal government in Canada. *McGill.*

So Yong Chung, B.A. Washington 1956; M.A. 1957. Impacts of federal-state reciprocal deduction of income tax. *Washington.*

Oliver F. Guinn, B.B.A. Oklahoma 1952; M.B.A. 1953. State debt creation in the United States since 1929. *Texas.*

Arthur J. Heins, B.S. Wisconsin 1953; M.S. 1957. Constitutional debt limitations in the states: the costs of avoidance. *Wisconsin.*

Frederick E. Kottke, B.A. George Pepperdine 1952; M.A. Southern California 1957. An economic analysis of financing an interstate highway system. *Southern California.*

Natalie Marshall, B.A. Vassar 1951. Public finance in English economic treatises from J. S. Mill to Alfred Marshall. *Columbia.*

Phillip H. C. McIntyre, M.B.A. Chicago 1950. Effects of federal income taxes on work incentives of physicians. *Cincinnati.*

Daniel C. Morgan, Jr., B.A. Texas 1953; M.A. 1955. Comparative aspects of alternative sales taxation and value-added taxation schemes as revenue sources for the State of Wisconsin. *Wisconsin.*

Alan P. Murray, B.A. Dartmouth 1955. Cumulative withholding and its role in economic stabilization. *Columbia.*

Robert F. Shannon, B.S. Holy Cross 1954; M.B.A. Detroit 1956. Capital gains taxation. *Illinois.*

David S. Staiger, B.A. Wisconsin 1951; B.A. Oxford 1953. Determinants of state and local expenditure. *Mass. Inst. Technology.*

Douglas Y. Thorson, B.A. Gustavus Adolphus College 1955; M.A. Nebraska 1957. The selection of taxpaying units for the income tax: an analysis of economic and institutional factors. *Wisconsin.*

E. Glenn Whitlock, B.A. Virginia 1957. Effect of federal income tax on competition and business concentration. *Columbia.*

Francis O. Woodard, B.S. Nebraska 1941; M.S. 1958. Selected taxes in relation to FHA borrowers in the Great Plains region. *Nebraska.*

## International Economics

### *Degrees Conferred*

Eirik Furnbotn, Ph.D. Columbia 1959. Effects of import restrictions on resource allocation and economic development.

William R. Gardner, Ph.D. Indiana 1959. The Canadian dollar, 1950-57; a study of rate stabilizing factors in a flexible exchange market.

Joseph E. Haring, Ph.D. Columbia 1959. External trade, capital imports and economic growth: the case of Puerto Rico.

Mitchell Harwitz, Ph.D. Mass. Inst. Technology 1958. On some problems in the dynamic theories of international trade and economic growth.

William B. Kelly, Jr., Ph.D. Fletcher School 1958. The 1927 convention on the abolition of import and export prohibitions and restrictions: its significance in United States commercial policy.

James W. Nordyke, Ph.D. Princeton 1959. International finance and New York: the role of the outflow of private financial capital from the United States in the determination of the significance of New York as a center of international finance.

Rudolph A. Postweiler, Ph.D. Wisconsin 1959. Problems concerning the supply and demand for direct U.S. private investment in Latin America for the years 1957-1965.

Emile E. Quevrin, Ph.D. Princeton 1959. Terms of trade and economic development.

Warnasena Rasaputram, Ph.D. Wisconsin 1959. National income and foreign trade of Ceylon—a structural analysis.

OTTO REISCHER, Ph.D. Columbia 1959. Federal aid to dislocation by freer trade.

RUDOLF R. RHOMBERG, Ph.D. Yale 1959. Fluctuating exchange rates in Canada: short-term capital movements and domestic stability.

SEH HYUK RYU, Ph.D. Cincinnati 1959. Underdeveloped countries in relation to the gains from trade.

FRANCIS X. SCAFURO, Ph.D. New York 1959. Comprehensive political and commercial risk guarantees for American export trade credits.

DONALD F. WAHL, Ph.D. Harvard 1959. Productivity and the structure of Canada's foreign trade.

KORNELIS WALRAVEN, Ph.D. Syracuse 1959. International long-term capital movements of the Netherlands in the postwar period, 1946-1956.

IMANUEL WEXLER, Ph.D. Harvard 1959. Some aspects of Western Europe's economic integration.

GORDON P. WONNACOTT, Ph.D. Princeton 1959. The Canadian dollar, 1948-1957.

RONALD J. WONNACOTT, Ph.D. Harvard 1959. An input-output analysis of the relationship of the Canadian and U.S. economies.

WILLIAM J. WOODFINE, Ph.D. Mass Inst. Technology 1959. The adequacy of international monetary reserves.

## *Theses in Preparation*

ROBERT Z. ALIBER, B.A. Wiliams 1952; B.A. Cambridge 1954; M.A. 1958. An inquiry into the stability of flexible exchange rates. *Yale.*

GERASSIMOS D. ARSENIS, Univ. Athens (Greece) 1954. Skewness of resources, trade and growth. *Mass. Inst. Technology.*

DAVID J. ASHTON, B.S. Tufts 1942; M.B.A. Boston 1950; M.A. Fletcher School 1952. The meaning of export origin. *Fletcher School.*

ROBERT F. BARLOW, B.A. Colby 1950; M.A. Fletcher School 1951. United States exports and imports of cotton textiles: a study of the bases of trade. *Fletcher School.*

RAMIRO CABANAS PINEDA, B.A. Univ. Nacional de Honduras 1955; M.A. Harvard 1958. General problems of economic union and the Central American case. *Harvard.*

GEORGE K. CHACKO, B.Comm. St. Xavier's (India) 1952; M.A. Madras Christian (India) 1950. International trade aspects of Indian burlap—an econometric study. *New School.*

ROSEMARY COWARD, B.A. Texas 1948. Factors affecting mainland manufacturing investment in Puerto Rico. *Harvard.*

CARLOS F. DÍAZ-A., B.S. Miami Univ. (Ohio) 1957. Devaluation: a study of Brazilian and Mexican experiences. *Mass. Inst. Technology.*

HARALD EINSMANN, Diplom Kaufmann Univ. Hamburg 1957; M.B.A. Florida 1959. The effect of the European common market on the U.S. economy. *Florida.*

HORST H. H. ESCHENBERG, M.S. Purdue 1957. German balance of payments problems since 1950. *Purdue.*

H. G. GEORGIADIS, B.A. Cornell 1954. Balance of payments equilibrium and U.S. money flows. *Cornell.*

ROBERT W. GILLESPIE, B.A. Stanford 1956. Simulation by IBM 704 of a developing economy with special reference to balance of trade problems. *Mass. Inst. Technology.*

HERBERT A. GOERTZ, B.A. Bowling Green State Univ. 1953; M.A. 1955; M.A. Yale 1958. Changes in the structure of Europe's postwar trade. *Yale.*

HANS E. JENSEN, B. of Comm., Business Graduate School, Copenhagen, 1949; M.A. Minnesota 1951. Denmark's postwar balance of international payments. *Texas.*

ABDUL GAFFAR KHAN, B.A. Madras (India) 1953; M.A. New York 1958. United States demand for imports in the post-war period. *New School.*

RICHARD I. LEIGHTON, B.A. Tufts 1957. Some aspects of the welfare state and international trade. *Duke.*

BERNARD L. MARTIN, M.A. Xavier 1955. Analysis of U.S. capital movement into the Latin American area. *Cincinnati.*

JAMES B. NEIGHBOR, B.A. Illinois 1952; M.A. 1953. State-side assignments for executive development of nationals employed by United States business organizations operating overseas. *Cornell.*

ROBERT S. OZAKI, B.A. Ohio Wesleyan 1956; M.A. Harvard 1958. Japan's recovery in international trade, 1946-56. *Harvard.*

SEH HYUK RYU, M.E. Waseta Univ., Tokyo. The gains from foreign trade in regard to the underdeveloped countries. *Cincinnati.*

MARTIN SCHNITZER, B.A. Alabama 1949; M.B.A. 1951. Locational factors influencing the location of branches of American concerns in Latin America. *Florida.*

JOHN C. SHEARER, B.S. Cornell 1952; M.A. Princeton 1958. Foreign nationals in the management of Brazilian and Mexican facilities. *Princeton.*

IZUMI TANIGUCHI, B.B.A. Houston 1952; M.B.A. 1954. Financing Japanese imports with especial relation to the role of foreign investment in the process. *Texas.*

WILLIAM P. TRAVIS, B.A. Harvard 1954; M.A. 1957. French investment and foreign trade structure. *Harvard.*

BILLY H. WILKINS, M.B.A. Texas College of Arts and Industries 1956; M.S. 1957. Effects on the economy of Venezuela of actions by the American petroleum industry and regulating agencies. *Texas.*

## Business Finance; Investment and Security Markets; Insurance

### *Degrees Conferred*

STEPHEN H. ARCHER, Ph.D. Minnesota 1958. An empirical test of guides to the selection of industrial common stocks for institutions.

EDWARD C. ATWOOD, JR., Ph.D. Princeton 1959. Theory and practice in the coffee futures market.

DAVID L. BICKELHAUPT, Ph.D. Pennsylvania 1959. Changes in financial structure during the transition of multiple-line insurance.

GEORGE W. BISHOP, JR., Ph.D. New York 1959. Charles H. Dow and the Dow theory.

M. J. BOOTE, Ph.D. McGill 1959. Certain taxation aspects of corporate finance in Canada.

DONALD O. BOWLIN, Ph.D. Illinois 1959. Factors affecting equity financing of American business corporations.

WARREN C. BRAY, Ph.D. New York 1958. A history of capital development in the American Tire and Rubber Company.

MOTHER MARTIN BYRNE, Ph.D. California (Los Angeles) 1959. A study of creditors' practices in the financing of religious institutions.

AVERY B. COHAN, Ph.D. Columbia 1959. The pricing of underwriters' services, 1935-52.

JACK W. COLEMAN, D.B.A. Indiana 1958. Efficiency in Air Force financial management: an analysis of the working capital fund concept.

DAVID K. EITEMAN, Ph.D. Northwestern 1959. Effect of regulation under alternate rate base types upon telephone and electric utility earnings.

CEDRIC V. FRICKE, Ph.D. Michigan 1959. Investment aspects of the variable annuity, 1958-1965.

GEORGE S. GOODELL, Ph.D. Northwestern 1959. A study of the role of trade credit in the financing of American industry.

RAGHUVIR S. GUPTA, Ph.D. Maryland 1958. Certain aspects of corporate management and finance in India.

EASO JOHN, Ph.D. Northwestern 1959. Investment experiences of twenty-five selected fire insurance companies, 1927-1955.

JOHN B. MINICK, Ph.D. Southern California 1959. Consumer credit insurance in Nebraska.

HERBERT D. MOHRING, Ph.D. Mass. Inst. Technology 1958. The life insurance industry: a study of price policy and its determinants.

JAMES M. MURPHY, D.B.A. Indiana 1959. The use of long-term debt by state supported institutions of higher education in the United States, 1947-1953.

INGOLF H. E. OTTO, Ph.D. George Washington 1959. Regulation of insurance in the United States by the federal government.

CHARLES F. POSTON, Ph.D. North Carolina 1959. The significance of restricted stock options in compensating the executives of 160 selected companies.

HAROLD W. SCOTT, Ph.D. New York 1959. The investment of trusteed pension funds.

HERBERT SIM, Ph.D. Syracuse 1958. The internal financing of corporations in the U.S., 1946-1954.

MORRIS L. STEVENS, Ph.D. Wisconsin 1959. The effect of changes in institutional and individual demand for corporate securities on the structure of the capital market, 1920-1955.

LEROY S. WEHRLE, Ph.D. Yale 1959. Life insurance portfolio selection: an empirical and theoretical investigation.

EDMUND H. WOOLRYCH, Ph.D. Syracuse 1958. Corporate pension funds and their effects on other savings institutions.

EDWARD D. ZINBARG, Ph.D. New York 1959. A study of industrial common stock price-earnings ratios and yields.

## *Theses in Preparation*

PETER S. ALBIN, B.A. Yale 1956. An empirical investigation of stock price determination. *Princeton.*

BERNARD BERNSTEIN, B.A. Iona 1954; M.A. Fordham 1956. Certain financial aspects of the automobile industry; 1946-57. A study of the industry's sources and uses of funds. *Fordham.*

JOSEPH B. BLACK, JR., B.S. Indiana 1947; M.B.A. 1956. Sources of growth in market values of common stocks of industrial companies. *Indiana.*

DAVID A. CLIMAN, B.S. McGill 1947; M.B.A. Pennsylvania 1952. Cash management in large non-financial corporations. *New York.*

EDWIN B. COX, B.A. Pennsylvania 1951; M.A. 1953. Trends in the distribution of stock ownership. *Pennsylvania.*

DONALD E. FARRAR, B.A. Harvard 1954. The investment decision under uncertainty. *Harvard.*

EDWARD R. HAIGLER, B.S. American 1945; M.B.A. 1956. Suggested statutory changes relating to transactions in securities. *American.*

JAMES HEIDELL, B.S. Harvard 1935; LL.B. 1938; M.S. Columbia 1948. The purchasing of tax exempt bonds by individuals in the 1946-1956 decade. *New York.*

MILTON W. HUDSON, B.S. Columbia 1951; M.A. New York 1954. Government security dealers: their role in the financial life of the nation. *New York.*

LAWRENCE D. JONES, JR., B.A. Ohio State 1953; M.A. 1954. Investment behavior of life insurance companies. *Harvard.*

AHMED KOOROS, B.A. George Pepperdine 1956; M.A. Southern California 1958. A contribution to the theory of comparative investment decisions. *Southern California.*

SANFORD L. MARGOSHES, B.A. Princeton 1948; M.B.A. Columbia 1950. Profitability and risk. *New York.*

SISTER MARY ELISSA MCGUIRE, B.A. College of Notre Dame of Maryland 1945; M.A. Johns Hopkins 1952. Bank stocks as an investment, 1951-57. *Fordham.*

CHARLES R. MINTON, B.S. and M.S. Kentucky. Management policies, performances, and practices of selected management investment companies of the open-end diversified type. *Kentucky.*

ARNOLD B. MOORE, M.B.A. Chicago 1957. A suggested method for analysis of security price time series. *Chicago.*

JOHN J. O'CONNOR, B.A. Fordham 1939; M.A. Columbia 1944. The financing of office buildings in N.Y. during the post World War II period. *New York.*

ALDEN C. OLSON, B.B.A. Minnesota 1949; M.A. 1951. Preferred stocks and valuation requirements for institutions. *Minnesota.*

HAROLD A. PETERSEN, B.A. DePauw 1955. Corporate finance, the investment decision and regional economic growth. *Brown.*

WILLIS P. ROKES, B.S. Utah 1949; LL.B. 1951; M.S. 1957. Automobile physical damage affiliates of sales finance companies and automobile manufacturers. *Ohio State.*

JOHN T. ROMANS, M.S. Tennessee 1957. Investment flows of life insurance companies in regional economic development. *Brown.*

LOUIS J. RUB, B.S. New York 1946; M.B.A. 1949. Agent-serviced government insured and guaranteed mortgages as investments for pension funds. *New York.*

JOSEPH E. SCHOBER, B.S. Illinois 1938; M.S. Harvard 1947. Budgeting for capital expansion. *New York.*

CASE M. SPRENKLE, B.A. Colorado 1956; M.A. Yale 1957. A study of expectations and leverage by an analysis of common stock prices. *Yale.*

RUSSELL G. S. STANFORD, B.S. Washington 1943; M.S. 1947. An analysis of speculation in financial markets. *Southern California.*

M. RICHARD SUSSMAN, B.S. Ohio 1950; M.B.A. Ohio State 1952. The stock dividend: a study of its concepts and of its uses in financial management. *Michigan.*

DAN USHER, B.A. McGill 1955; M.A. Chicago 1958. The debt equity ratio. *Chicago.*

GERT VON DER LINDE, Diplom Volkswirt Univ. Munich 1955. The organization, administration, and investment problems of pension funds. *California (Berkeley).*

CECIL E. WALTON, B.S. Oklahoma 1949; M.B.A. 1950. An inquiry into some aspects of corporate securities retirements from the sale of corporate funds. *Arkansas.*

FANG WEN WANG, B.A. National Chungking Univ. 1948; M.B.A. Northwestern 1951. Statistical analysis of industrial common stock price behavior. *North Carolina.*

WALTER WILLIAMS, B.B.A. Texas 1955; M.B.A. 1956. Actual cash value in property insurance: practical problems and economic implications. *Indiana.*

JOSEPH WISEMAN, B.B.A. City (New York) 1938; M.B.A. 1950. Financial aspects of depreciation methods. *New York.*

FRANK J. WRIGHT, B.S. Duquesne 1947; M.B.A. Pennsylvania 1949. Criteria employed to determine value of selected private transit companies transferred to public ownership with possible application to Pittsburgh Railways Company. *Pittsburgh.*

## Business Organization; Managerial Economics; Marketing; Accounting

### *Degrees Conferred*

MILTON ALEXANDER, Ph.D. New York 1959. The significance of ethnic groups in marketing new-type foods in greater New York.

WALTER A. BECKDAHL, Ph.D. Ohio State 1959. A theoretical approach to determining spare part support for any given organized operation.

GEORGE BENSON, Ph.D. New York 1959. The determination of normal retail newspaper advertising levels.

LUTHER D. BISHOP, Ph.D. Ohio State 1959. A study of the effects of automation in a selected company.

GEORGE E. BREEN, Ph.D. New York 1959. A program for the development of marketing research among medium and small size manufacturing firms in the industrial goods field.

ROBERT L. CLEWETT, Ph.D. Michigan 1959. The economics of automobile distribution.

EDWIN W. CROOKS, D.B.A. Indiana 1959. The history of the Halle Brothers Company.

BRUCE E. DESPELDER, Ph.D. Ohio State 1959. Ratios of staff to line personnel in the automotive parts manufacturing industry.

CARL T. EAKIN, D.B.A. Indiana 1959. A study of, and recommendations for, inventory policy and practice in the retail hardware trade.

WILLIAM F. EGLOFF, Ph.D. Northwestern 1958. Marketing strategies in the fiber glass panel industry.

FREDERIC N. FIRESTONE, Ph.D. Wisconsin 1958. Some aspects of "marginalism" in the management practices of "excellently-managed" companies.

GINO GIUSTI, Ph.D. Pittsburgh 1959. An economic study of research and development expenditures in American industries.

BOB R. HOLDREN, Ph.D. Yale 1959. The structure of a retail market and the market behavior of retail units.

ROBERT M. JENNINGS, D.B.A. Indiana 1959. George S. Olive and Company—a history of a public accounting firm.

RUEL C. KAHLER, Ph.D. Michigan 1959. Buying and store operations control in a food chain.

ROY A. KLAGES, Ph.D. Saint Louis 1959. An analysis and interpretation of the factors influencing the decentralization of the downtown department stores in St. Louis.

RICHARD I. LEVIN, Ph.D. North Carolina 1959. An analysis of effort-output relationship in hand motions.

VERNON L. LOOMIS, Ph.D. Ohio State 1959. An appraisal of sales quota usage.

MICHAEL Z. MASSEL, Ph.D. Northwestern 1958. Management programs for determining differences of opinion within and between policy makers, policy implementers and policy receivers.

JOHN F. MEE, Ph.D. Ohio State 1959. The history of twentieth century management thought.

JOHN A. MENGE, Ph.D. Mass. Inst. Technology 1959. Internal transfer pricing in vertically integrated firms.

EDWARD J. MORRISON, D.B.A. Indiana 1959. Applying management fundamentals to life insurance agencies.

DAVID W. ORTLIEB, D.B.A. Indiana 1959. Management philosophy and the corporate personality.

RAY M. POWELL, D.B.A. Indiana 1959. Top management view on tax depreciation allowances.

DONALD L. RICHARD, Ph.D. American 1959. The economic effects of the reducing charge methods of depreciation.

JOSEPH C. SCHABACKER, Ph.D. California (Los Angeles) 1959. A study of cash planning in small manufacturing companies.

LLOYD V. SEAWELL, D.B.A. Indiana 1958. An evaluation of selected industrial corporation annual reports for compliance with the Accounting Research Bulletins of the American Institute of Certified Public Accountants.

FREMONT SHULL, JR., Ph.D. Michigan State 1958. The advertising appropriation in the rubber tire industry: a study in decision-making.

ANDREW C. STEDRY, Ph.D. Carnegie Inst. Technology 1959. Budget control and cost behavior.

ALLAN J. TWARK, Ph.D. Illinois 1959. Sales effort and its effect on the quantity sold.

LEON P. ULLENSVANG, Ph.D. Northwestern 1959. The hotel-restaurant meat purveyor: an

analysis of the growth operation and future development of a specialized type of meat wholesaler.

JAMES S. WORLEY, Ph.D. Princeton 1958. Industrial research and development and the new competition—a study of the leading employers of research and development personnel by industry group and by size of firm.

## *Theses in Preparation*

JAROLD G. ABBOTT, B.A. William Jewell 1956. The expression of managerial authority in the family-owned firm. *Mass. Inst. Technology.*

ANKER V. ANDERSEN, B.B.A. Minnesota 1955; M.B.A. 1957. Accounting procedures and stability of income—the effect of cost allocation methods. *Minnesota.*

MORTON BACKER, B.S. Boston 1939; M.Litt. Pittsburgh 1952. A study of the usefulness of accounting costs for product pricing and capital formation decisions. *Pittsburgh.*

HAROLD D. BIRCKMAYER, B.A. Cornell 1952; M.B.A. 1956. Tax incentives for the petroleum and other mineral industries. *Cornell.*

RICHARD S. BOWER, B.A. Kenyon 1949; M.B.A. Columbia 1955. Capital spending in the clay construction product and refractory industries: a study of investment behavior. *Cornell.*

HENRY M. BRANER, B.S. New York 1952; M.B.A. Columbia 1954. An investigation of the principles and methods of the experimental method in marketing research. *Ohio State.*

LOUIS P. BUCKLIN, B.A. Dartmouth 1950; M.B.A. Harvard 1954. Application of the theory of the firm to the study of the structure of the distribution channels. *Northwestern.*

WILLIAM R. CAMPBELL, B.S. Ohio State 1950; M.B.A. 1951. Management of the real estate function in selected large industrial firms. *Indiana.*

NEIL C. CHURCHILL, B.S. California 1951; M.B.A. 1954. Production and cost-functions as related to management decisions based upon budget analysis. *Michigan.*

ALTON F. DOODY, B.A. Ohio Wesleyan 1956; M.B.A. Ohio State 1957. The Army-Air Force post exchange system—a marketing operation in the military service. *Ohio State.*

RONALD E. FRANK, B.S. Northwestern 1955; M.B.A. 1956. An analysis of short-run consumer brand purchasing patterns. *Chicago.*

ROBERT W. FRYE, B.S. Washington (St. Louis) 1956; M.B.A. 1957. An analysis of educational film marketing. *Indiana.*

NORMAN GEORGE, B.A. Ohio State; M.B.A. Pittsburgh 1954. Factors in foremen identification with management and their relationships with foremen effectiveness. *Ohio State.*

CHARLES H. HINDERSMAN, B.S. Pennsylvania 1947; M.B.A. Miami 1954. Changing balance of retail trade between the downtown and suburban portions of metropolitan areas. *Indiana.*

IRA HOROWITZ, B.A. Johns Hopkins 1955. An economic analysis of industrial research. *Mass. Inst. Technology.*

LEON R. KLEIN, B.B.A. Armstrong 1954; B.S. Ohio State 1955; M.B.A. 1956. The role, functions, and economic status of the rack jobber in the marketing process. *Ohio State.*

SAMUEL LAIMON, B.Comm. Saskatchewan 1949; M.B.A. Ontario 1951. Accounting for spoilage. *Chicago.*

MAX E. LUPUL, B.S. California 1954. Some effects of the introduction of frozen foods on the marketing channels of fresh and canned agricultural products, with special emphasis on the San Francisco Bay area. *California (Berkeley).*

HARVEY A. MAERTIN, B.S. Illinois Inst. Technology 1941; M.B.A. Chicago 1947. Some management problems, methods and criteria for predicting creatively in industrial research. *Northwestern.*

EUGENE R. MAGRUDER, B.B.A. Texas 1949; M.A. 1950. Management by objectives: a philosophy. *Ohio State.*

Morris L. Mayer, B.S. in B.A. Alabama 1949; M.S. New York 1950. Changes in variety store merchandising. *Ohio State.*

Tsri Ophir, B. A. Hebrew 1955; M.A. 1957; M.S. Mass. Inst. Technology 1957. Pricing internal transfers in decentralized companies. *Mass. Inst. Technology.*

B. P. Pashigian, B.A. Wayne 1954. The distribution of automobiles: an economic analysis of the franchise system. *Mass. Inst. Technology.*

Henri C. Pushker, B.S. Kent State 1955; M.B.A. 1957. A determination of the applicability of generally accepted accounting principles to nonprofit-seeking institutions. *Indiana.*

James W. Reynolds, B.S. Missouri 1948; M.S. 1950. Centralized accounting for food retailers. *Purdue.*

Leonard Rico, B.S. Rutgers 1952; M.A. Illinois 1955. Automation and the administration of personnel. *Mass. Inst. Technology.*

Thomas W. Sharkey, B.S. Ohio 1951; M.B.A. Indiana 1952. The development of a production game. *Indiana.*

Bruce Skaggs, B.A. George Washington 1949. Concepts and methods of establishing centralized stockage categories for commercial-type repair parts in the United States Army. *Ohio State.*

George R. Slater, B.S. Purdue 1946; M.S. 1956. Function of an industrial economist in a diversified industry. *Purdue.*

David A. Storey, B.S. Massachusetts 1954; M.S. Purdue 1958. Distribution and comsumption of baking products. *Purdue.*

Dale Taylor, B.A. Brigham Young 1951; M.A. 1953. Allocation of consolidated taxes. *Northwestern.*

James D. Taylor, B.S. Iowa 1950; M.A. 1954. An analysis of changes in sales volume within central business districts and non-central business district areas in central cities within standard metropolitan areas in the United States from 1948-1954. *Iowa.*

Ralph L. Thomas, M.L. Pittsburgh 1955. The use of corporate funds for educational purposes. *Pittsburgh.*

Kenneth P. Uhl, B.S. Iowa 1953; M.A. 1958. Some aspects of stockholder purchasing bias for "their" corporations' products. *Iowa.*

Karl Vogt, B.S. Holy Cross 1952; M.A. Syracuse 1957. The erosion of managerial prerogatives in the postwar era. *Syracuse.*

William H. Waldorf, B.A. Pennsylvania 1949; M.A. Chicago 1951. The market demand for food, with emphasis on services. *Chicago.*

Martin R. Warshaw, B.A. Columbia 1947; M.B.A. Michigan 1957. Manufacturer-wholesale relations and their influence on patterns of distribution in selected consumer goods industries. *Michigan.*

Gilbert R. Whitaker, Jr., B.A. Rice Inst. 1953; M.S. Wisconsin 1958. An investigation of the influence of budgetary planning upon the behavior of the business firm. *Wisconsin.*

## Industrial Organization; Government and Business; Industry Studies

### *Degrees Conferred*

Earl D. Bennett, Ph.D. Michigan 1959. Small business management.

Stanley E. Boyle, Ph.D. Wisconsin 1959. The artificial rubber industry in the United States.

Allan J. Braff, Ph.D. Wisconsin 1959. Wage and price decisions in the basic steel industries, 1945-1956.

M. Eleanor Craig, Ph.D. Duke 1959. Recent history of the North Carolina furniture manufacturing industry with special attention to locational factors.

Charles Dean, Ph.D. Columbia 1959. Industrial development and monopoly behavior.

Harold Demsetz, Ph.D. Northwestern 1959. Product innovation and imitation, with a case study of the frozen orange concentrate industry.

Elizabeth Y. Deran, Ph.D. California (Berkeley) 1959. Western mobile home industry.

Brian Dixon, Ph.D. Michigan 1959. Implications of selected price discrimination cases on management price policy.

Howard C. Giles, Ph.D. Purdue 1959. A case study of the Indiana Restaurant Association.

Anthony J. Glennon, Ph.D. Fordham 1959. The trade practice conference procedure of the Federal Trade Commission, 1935-1956.

William E. Gustafson, Ph.D. Harvard 1959. A locational study of the printing and publishing industry.

William E. Hurley, D.B.A. Indiana 1959. Titan of Pennsylvania—a business history of the Titan Metal Manufacturing Company.

John M. Lishan, Ph.D. Harvard 1959. The Sherman Act.

Kenneth H. McCartney, Ph.D. Minnesota 1959. Government enterprise: a study of the Inland Waterways Corporation.

Paul T. McElhiney, Ph.D. California (Los Angeles) 1959. The freeways of metropolitan Los Angeles—an evaluation in terms of their objectives.

Jack McEnroe, Ph.D. Syracuse 1959. Investment decision and the electric power industry.

Norman O. Miller, D.B.A. Indiana 1958. Industry growth and corporate profitability.

David C. Motter, Ph.D. Vanderbilt 1958. Governmental controls over the iron and steel industry during World War II: their development, implementation, and economic effect.

George C. Nichols, Ph.D. Virginia 1959. The impact of the St. Lawrence Seaway upon the economy of Virginia.

George Novak, Ph.D. Columbia 1959. Long-range planning of railroad freight traffic in USSR.

Khattar, P. Rahbany, Ph.D. Wisconsin 1958. Licensing of water power projects by the Federal Power Commission.

Philip J. Reinertsen, Ph.D. Chicago 1958. The pulp and paper industries in Sweden and Canada.

John W. Riegel, Ph.D. Harvard 1959. Research and product development in the railway equipment industry.

George Saunders, Ph.D. Syracuse 1959. Economics of the air conditioning industry.

Gene R. Simonson, Ph.D. Washington 1959. Economics of the aircraft industry.

Bruno Stein, Ph.D. New York 1959. Labor participation in stabilization agencies: the Korean War period as a case study.

Perry Teitelbaum, Ph.D. Chicago 1959. Nuclear energy and the U.S. fuel economy, 1955-1980.

William C. Tuthill, Ph.D. Michigan 1959. Rate of return of private electric utilities.

Wilfred Watson, Ph.D. Texas 1959. Determination of training needs of the first level of supervision in member companies of the Southern Gas Association.

Rudolph A. White, Ph.D. Alabama 1958. A comparative analysis of manpower mobilization in Great Britain and the United States during World War II.

## *Theses in Preparation*

Shirley M. Almon, B.A. Goucher 1956; M.A. Radcliffe 1958. Business concentration and economic growth. *Harvard.*

William H. L. Anderson, B.A. Williams 1955. Econometric study of manufacturing investment. *Harvard.*

Richard W. Beeson, B.A. Ohio Wesleyan 1955; M.A. Northwestern 1959. The "meeting competition in good faith" section of the Robinson-Patman Act: aid or barrier to competition? *Northwestern.*

Charles H. Bradford, B.S. Utah 1952; M.S. 1953. Imputed individual road revenues and road costs in the State of Utah with an analysis of problems and considerations for rational road investment. *Harvard.*

James E. Brown, B.S. Richmond 1954; M.A. Michigan State 1958. An examination of the problems of public utility valuations looking toward suggested solution. *Florida.*

James P. Cairns, B.A. Toronto 1949; M.A. Columbia 1950. Integration in the food retailing industry. *Johns Hopkins.*

William D. Carmichael, B.A. Yale 1950. Some problems in the strategy and tactics of price control. *Princeton.*

Alvin Cohen, B.A. Columbia 1953; M.B.A. George Washington 1955. The role of the Chilean government in the development of its electricity. *Florida.*

Robert F. Croll, B.S. Northwestern 1954; M.B.A. Michigan 1955. The influence of federal and state legislation on retail competition in the automobile industry: a study in business-government relations. *Indiana.*

J. Gaylord Cummins, B.A. Johns Hopkins 1957. Mergers and concentration in the paper industry. *Johns Hopkins.*

Hersckel Cutler, B.S. Boston 1957. M.B.A. Pennsylvania 1958. The influence of transportation rates and costs in the location of American manufacturing and distribution facilities. *Syracuse.*

Donald C. Darnton, B.A. William and Mary 1953; M.A. Michigan 1956. The probable impact of the St. Lawrence Seaway on the port of Baltimore. *Michigan.*

Phoebus J. Dhrymes, B.A. Texas 1957. Differential rates of productivity changes: a study of the service industries. *Mass. Inst. Technology.*

James M. Folsom, B.B.A. Georgia 1953. Application of the Celler Anti-Merger Act. *Vanderbilt.*

Donald F. Forster, B.A. Toronto 1956; M.A. Harvard 1958. Origins and development of Canadian combines policy. *Harvard.*

Edward Greenberg, B.S. New York 1957. Decision making in the automobile industry. *Wisconsin.*

Marshall M. Hall, B.S. Columbia 1957. Investments in innovations and research. *Wisconsin.*

Joseph M. Hennessey, B.S. Boston 1951; M.A. 1953. New England manufactures 1947-54—a postwar analysis of employment, output and productivity. *Boston.*

Jack Hickman, B.S. U.S. Military Academy 1935; M.S. Missouri 1958. Air cargo potentials. *Florida.*

Ma Tin Hlyne, B.S. Univ. Rangoon-Burma 1943; M.B.A. New York 1950. The accounting and financial systems of the Tennessee Valley Authority. *New York.*

Kenneth Holcomb, B.S. Arkansas 1947; M.A. 1954. An analysis of problems involved in piggyback service offered by the railroads with particular reference to the economic significance of the services. *Arkansas.*

Charles M. Hummel, B.S. Virginia Polytechnic Inst. 1942; M.S. Pennsylvania 1949. Development of a quantitative criterion for the guidance of highway investment decisions in Virginia. *Virginia.*

Solomon Jacobson, B.S.S. City 1930; LL.B. Columbia 1931; M.A. Brooklyn 1938. The fourth estate: a study of the American Newspaper Guild. *New School.*

HENRY K. KRAUSKOPF, B.B.A. Cincinnati 1940. Small business and federal purchasing. *Columbia.*

MILLARD F. LONG, B.A. Harvard 1955; M.A. Chicago 1957. The British nationalized coal industry. *Chicago.*

GEORGE E. MCCALLUM, B.A. California 1954. A case for transport regulation reconsidered. *California (Berkeley).*

PAUL T. MCELHINEY, B.A. Washington 1942; M.A. 1955. The freeways of metropolitan Los Angeles—an evaluation in terms of their objectives. *California (Los Angeles).*

CHARLES O. MEIBURG, B.S. Clemson 1953; M.A. Virginia 1958. The free public road and government highway policy decisions. *Virginia.*

JORA R. MINASIAN, B.S. DePaul 1953; M.B.A. 1954. A study in the economics of research and development in the chemical and drug industries. *Chicago.*

CHARLES T. MOORE, B.S. Indiana 1952; M.B.A. 1953. The economic effects of water transportation on the Ohio River. *Indiana.*

ABDUL WAHAB MUTAR, B.A. Texas 1953; M.A. 1954. Public utilities in Iraq and the role of government. *Texas.*

GORDON NOVOTNIE, B.A. Pennsylvania State 1940; M.L. Pittsburgh 1952. Economic aspects of stabilization in the steel industry. *Pittsburgh.*

JAMES R. OTT, JR., B.S. Hendrix 1947; M.A. Vanderbilt 1950. Employment trends in the textile industry in the period since World War II. *Vanderbilt.*

PHILIP C. PACKARD, B.A. Washington 1955. Economics of the newsprint paper industry. *California (Berkeley).*

CHARLES F. PHILLIPS, JR., B.A. New Hampshire 1956. Competition in the synthetic rubber industry. *Harvard.*

LAURIE S. ROBERTSON, B.A. Nebraska 1943; M.A. 1948. Financial aspects of the railroad crisis. *Michigan.*

WARREN ROSE, B.S. Maryland 1949; M.A. Northwestern 1953. Economic significance of air coach travel. *North Carolina.*

NORMAN SCHNEIDER, B.A. Brooklyn 1953. Impact of product differentiation. *California (Berkeley).*

DONALD SOLAR, B.A. Wisconsin 1951. Policy on local service airlines. *Columbia.*

EARL A. SPILLER, JR., B.A. Ohio Wesleyan 1956. Significance and influence of accounting data adjusted for price level changes on the problems and policies of public utilities. *Michigan.*

KONRAD STUDNICKI-GIZBERT, B.S. London 1951; M.S. 1957. Economics of Canadian air transport industry. *McGill.*

JOHN G. WATTS, B.A. Minnesota; M.A. Pennsylvania. Investment in the Broadway theatre. *Columbia.*

EDWARD ZANE, B.B.A. Alaska 1951; M.B.A. Boston 1954. Piggyback transportation: its economic effects on inter-carrier and interregional competition. *Massachusetts.*

## Land Economics; Agricultural Economics; Economic Geography; Housing

### *Degrees Conferred*

RAYMOND L. ANDERSON, Ph.D. Wisconsin 1959. Problems of private land use for recreation in Wisconsin.

RICHARD A. ANDREWS, Ph.D. Minnesota 1959. A study of the sweet corn industry in the Midwest farm economy.

RICHARD D. APLIN, Ph.D. Cornell 1959. Comparative costs of country handling of milk-bulk assembly vs can assembly.

CHARLES F. BORTFELD, Ph.D. Minnesota 1958. Production alternatives in response to price changes for a 320-acre wheat-beef farm [illegible] central Kansas.

JULIAN BRADSHER, Ph.D. Californ[illegible] 1959. Agrarian reform in Mexico since 1934.

MICHAEL F. BREWER, Ph.D. Cal[illegible]keley) 1959. Water pricing and allocation with particular reference to Californ[illegible]gation districts.

DALE H. CARLEY, Ph.D. Ohio State 1959. The effect of price on the supply response of milk in two Ohio markets.

EDWIN G. C[illegible] Indiana 195[illegible] the demand for coal for power generation in the Tennesse[illegible] the impac[illegible]nging demand patterns on a supplying coal field.

IRVING F. [illegible]vis, JR., Ph.D. Cal[illegible] (Berkeley) 1959. An economic analysis of saving in agriculture, with special ref[illegible]nce to California.

[illegible]TON C. DENNIS, Ph.D California (Berkeley) 1959. Interregional competition in the frozen strawberry indust[illegible]

JEANNE E. DOST, Ph.D Radcliffe 1959. An interregional analysis of the three major wheat producing regions.

AKHILESH DUBEY, Ph.D. Ohio State 1958. The effects of the St. Lawrence Seaway on Ohio wheat marketing.

LUDWIG M. EISGRUBER, Ph.D. Purdue 1959. The effect of laying flock size on the economics of egg production in northeastern Indiana.

SHUH-CHING FAN, Ph.D. Cornell 1959. The causes of variability in yields, costs and returns in apple production in western New York, 1956 and 1957.

FARHAD GHAHRAMAN, Ph.D. Minnesota 1958. The right of use and economics of irrigation water in Iran.

WALTER E. GUNNING, Ph.D. New York 1959. A comparison of real estate valuation in the United States and Great Britain.

ROBERT L. GUSTAFSON, Ph.D. Chicago 1958. Carryover levels for grains: a method for determining amounts that are optimal under specified conditions.

NADIM G. HAJJAR, Ph.D. California (Berkeley) 1959. Intraregional trade in the Arab Near East, with emphasis on the products of agriculture.

FREDERICK HANER, Ph.D. Pittsburgh 1959. The economic future of the domestic petroleum industry: a forecast through 1970.

OSMOND L. HARLINE, Ph.D. Indiana 1958. Economics of the Indiana coal mining industry.

VINCENT J. HUBIN, Ph.D. New York 1959. The nature and measurement of realty income.

RUFUS B. HUGHES, Ph.D. Chicago 1959. Low incomes in Southern agriculture.

T. M. JOSHI, Ph.D. Pennsylvania 1959. Agricultural land revenue in the Bombay State.

MONTE JUILLERAT, Ph.D. Purdue 1959. The pricing structure for soybeans at country elevators and processors in Indiana.

ARCADIUS KAHAN, Ph.D. Rutgers 1959. The collective farm economy of the Soviet Union, 1933-1936, with special emphasis on output, income and government policy.

RAY S. KELLEY, JR., Ph.D. Vanderbilt 1958. Some natural resources of New England.

MYRON P. KELSEY, Ph.D. Purdue 1959. Economic effects of field renting on resource use on central Indiana farms.

ROBERT A. KOCH, Ph.D. Purdue 1959. Interregional competition in the tomato processing industry.

JANIS LABSVIRS, Ph.D. Indiana 1959. A case study in the Sovietization of the Baltic states: collectivization of Latvian agriculture, 1944-1956.

ROBIN C. LINSTROMBERG, Ph.D. Oregon 1959. Theory and practice of benefit-cost analysis.

DAVID LUAN, Ph.D. Texas 1959. Statistical quality control in cotton marketing.

BERNARD C. MELTZER, Ph.D. Pennsylvania 1959. Price behaviour of residential real estate.

JOHN R. MOORE, Ph.D. Wisconsin 1959. Market structure and competitive behavior in the dairy industry—the present state of knowledge.

JOHN T. MOORE, D.B.A. Indiana 1959. Fuel and power resources for Arkansas industrial expansion.

KENNETH C. NOBE, Ph.D. Cornell 1959. An agricultural regions concept for New York State.

DAVID R. PAPERA, Ph.D. Stanford 1958. Rise and decline of the California apple industry.

VERNON W. PHERSON, Ph.D. Purdue 1959. The competitive potential of the U.S. cotton industry.

IGNATZ J. PIKL, JR., Ph.D. Vanderbilt 1958. Economic problems of pine pulpwood production in the South and in the Hiwassee region.

HANS PILHOFER, Ph.D. Minnesota 1958. The interrelationship of farm mechanization and organization in decision making.

JAMES T. RALPH, Ph.D. Stanford 1958. The broiler chicken industry—an economic study.

ALFRED A. SCHMID, Ph.D. Wisconsin 1959. Water allocation and development in Wisconsin.

OLIVER G. SEIDMAN, Ph.D. New York 1959. A financial history of the dairy industry.

RICHARD L. SIMMONS, Ph.D. California (Berkeley) 1959. Optimum adjustments of the dairy industry of the western region to economic conditions of 1975.

WALLACE F. SMITH, Ph.D. Washington 1958. An outline theory of the housing market, with special reference to low-income housing and urban renewal.

ROBERT D. STEVENS, Ph.D. Cornell 1959. Capital formation and agriculture in some Lebanese villages.

AKIRA UCHIDA, Ph.D. Purdue 1959. Theoretical models of interregional competition in the poultry meat industry.

CHARLES P. WILSON, Ph.D. California (Berkeley) 1958. An economic and statistical analysis of beef cattle marketing and prices.

### *Theses in Preparation*

GERALD E. ACKERMAN, B.S. Cornell 1954; M.S. Toronto 1955. Changes in productivity of farm resources. *Purdue.*

HAMEED AL-QAYSI, B.A. Texas 1952; M.A. 1953. Iraq petroleum concessions in the context of world major petroleum concessions. *Columbia.*

RICHARD J. AMICK, B.S. Purdue 1954; M.S. 1956. Economics of size in the hog enterprise in Georgia. *Purdue.*

JOHN M. AMOS, B.S. Kansas State 1956; M.S. 1957. The effects of industrial developments on grain, feed and farm supply agencies in Ohio. *Ohio State.*

RALPH L. ANDREANE, B.A. Drury 1952; M.A. Washington (St. Louis) 1955. Crude oil production and the structure of the American petroleum industry. *Northwestern.*

GONZALO ARROYO, B.S. Catholic Univ. of Chile 1956; Lic. Phil. Immaculate-Conception College, Montreal, 1958. Efficiency of alternative tenure systems. *Iowa (Ames).*

JARVIS BABCOCK, B.A. Oberlin 1953. Demand for beef herd stock. *Iowa (Ames).*

RANDOLPH BARKER, B.S. Cornell 1953. The response of milk production to price—a regional analysis. *Iowa (Ames).*

PAUL T. BLAIR, B.S. Mississippi State 1952; M.S. 1953. Market organization and practices for potatoes in the Hastings area of Florida. *Florida.*

HENRY C. BRAMER, B.S. Pittsburgh 1948; M.S. 1953. An analysis of the economic aspects of the water polution abatement case in the Ohio River Valley. *Pittsburgh.*

CURTIS H. BRASCHLER, B.S. Missouri 1950; M.S. 1958. An investigation of the effects of varying rates of inflation and deflation on agricultural income. *Purdue.*

BRUCE L. BROOKS, B.S. Idaho 1943; M.S. 1950. An examination and evaluation of the Pacific Northwest wheat market relative to future trading. *Purdue.*

ROGER L. BURFORD, B.B.A. Mississippi 1956; M.A. 1957. The effects of federal cotton acreage restrictions upon rural to urban and South to non-South migration, and the economic consequences of such migration upon the South. *Indiana.*

DAVID L. CALL, B.S. Cornell 1954; M.S. 1958. Evaluation of factors affecting inter-regional competition in processed vegetables. *Cornell.*

EDWARD E. CARSON, B.S. Purdue 1953; M.S. 1956. Analysis of the effect of price and income programs on the beef industry. *Purdue.*

T. H. CHOU, LL.B. National Southwest Associated Univ., Yunnan, China, 1945; M.S. Cornell 1956. Dynamic linear programming for conservation in western Iowa. *Iowa (Ames).*

ELMER G. CLOSE, B.S. Florida 1948; M.S.A. 1952. Marketing of Florida grown hydroponic tomatoes. *Florida.*

SALVATORE COMITINI, B.A. Alabama 1951; M.A. 1955. An account study of Japanese fisheries exploitation and development. *Washington.*

MARCEL DANEAU, B.S. Laval 1954; M.S. Massachusetts 1957. Canada's Atlantic fishing industry. *Massachusetts.*

ROBERT C. DAVENPORT, B.S. American 1949. An economic appraisal of the federal urban renewal program. *Maryland.*

JACK R. DAVIDSON, B.S. Wyoming 1953; M.S. Montana State 1955. The effects of resource availability, prices, and contractual arrangements on resource use for market milk dairies in California. *California (Berkeley).*

OTTO A. DAVIS, B.A. Wofford 1956; M.A. Virginia 1957. The economics of municipal zoning. *Virginia.*

RICHARD H. DAY, B.S. Iowa (Ames) 1955. The analysis of production response in Southern agriculture. *Harvard.*

NORBERT A. DOROW, B.S. Iowa (Ames) 1948; M.S. Minnesota 1957. Economic analysis of the alternative systems of harvesting, storing and feeding forage crops. *Minnesota.*

JOHN A. EDWARDS, B.S. Wisconsin 1950; M.A. Nebraska 1952. The influence of weather on yields of spring wheat. *Chicago.*

EBER W. ELDRIDGE, B.S. Iowa (Ames) 1942; M.S. Michigan State 1957. Relation of occupational plans of Iowa farm boys to agricultural adjustment. *Iowa (Ames).*

EUGENE H. FOX, B.S. Northern St. Teachers 1956. An economic study of farming in southeast United States, with emphasis on low-income farming. *Alabama.*

CECIL E. FULLER, B.S. Ohio State 1954; M.S. 1955. Analysis of the economic importance of the surplus and deficit areas of feed grains in Ohio. *Ohio State.*

WAYNE FULLER, B.S. Iowa (Ames) 1955. Inclusion of lagged variables in the analysis of demand for farm products. *Iowa (Ames).*

MAMMOOTIL V. GEORGE, B.A. Univ. of Travancore, 1950; M.A. Univ. Madras 1952. The effects of mergers on the growth of the dairy industry. *Wisconsin.*

ANTONIO GILES, B.S. Escuela Nacional de Agric. "LaMolina," Lima, Peru, 1956; M.S. Utah State 1958. Economic analysis of Iowa's ground water reserves. *Iowa (Ames).*

HENRY A. GREEN, B.S. Rutgers 1956; M.S. 1958. An analysis of optimum combinations of land and human resources in a low-income area of southern Indiana. *Purdue.*

HARRY GREENBAUM, B.S. Texas A. & M. 1955; M.S. Ohio State 1956. An economic evaluation of Israeli Kibbutzim. *Ohio State.*

JURGEN H. GREIF, Matura, Georg-Herwegh School, Berlin, 1953; M.B.A. Chicago 1956. Petroleum in South America—an analytical examination of the development and consequences of public and private exploitation of oil resources in the postwar period. *Chicago.*

FRANCIS W. GROVES, B.S. Wisconsin 1951. Hauling and transportation cost functions for Wisconsin milk. *Wisconsin.*

CURTIS C. HARRIS, JR., B.S. Florida 1956. Production payments in agriculture. *Harvard.*

JOHN R. HILDEBRAND, B.A. California 1949; M.A. George Washington 1951. Geographical differentials in the earning power of input categories on Kansas farms. *Chicago.*

PAUL H. HOEPNER, B.S. Cornell 1955; M.S. 1956. An analysis of risk and uncertainty in dairy and hog production. *Minnesota.*

ANDREAS HOLMSEN, M.S. Royal Norwegian Agr. College 1955. An analysis of the variability in different factors affecting profitability in milk production within and between two regions in New York State. *Cornell.*

EUGENE C. HOLSHOUSER, B.S. Florida State 1950; M.S. 1953. The effect of the Lexington, Kentucky northern belt line or land values and land use: a case study. *Kentucky.*

ROBERT W. HOOD, B.S. Cornell 1955; M.S. Purdue 1957. Selection of optimum production and marketing activities for dairy processing. *Purdue.*

DALE M. HOOVER, B.S. Iowa (Ames) 1952; M.S. 1954; M.A. Chicago 1959. Agricultural land prices in the United States from 1910 to 1950. *Chicago.*

GORHAM HUSSEY, B.S. Maine 1954; M.B.A. Harvard 1958. Regional marketing development. *Purdue.*

LOREN IHNEN, B.S. Illinois 1953; M.S. 1954. Programming farm adjustments. *Iowa (Ames).*

JACK T. ISHIDA, B.S. Hawaii 1942; M.A. 1947. Analysis of factors affecting operating efficiency of selected Indiana supply and service cooperatives. *Purdue.*

ELIYAHU KANOVSKY, B.A. Yeshiva 1952. The economy of the Israeli Kibbutz. *Columbia.*

RONALD D. KRENZ, B.S. North Dakota Agric. College 1955. Farm size and costs in relation to farm machinery technology. *Iowa (Ames).*

LEO F. KRISTJANSON, B.A. Manitoba 1954; M.A. 1958. A history of farm organizations in Wisconsin. *Wisconsin.*

HUO-SHENG LEE, Graduate National Inst. Pol. Sci., Nanking, China, 1946; M.B.A. New York 1950. An economic appraisal of American investment in international petroleum industry. *New York.*

JAMES H. LEWIS, B.S. Ohio State 1953; M.S. 1954. An analysis of pricing hogs and the changing marketing structure in the wholesaling and retailing of pork and pork products. *Ohio State.*

JUNG-CHAO LIU, B.A. National Taiwan Univ. 1950; M.A. Washington 1955. An econometric study of the rice and sugar market in Taiwan. *Michigan.*

ALLYN O. LOCKNER, B.A. Huron 1954. A proposal for the taxation of molybdenum, uranium and vanadium mining in Colorado. *Colorado.*

DOUGLAS J. MCCONNELL, B.A. Texas 1956; M.A. 1957. Problems of resource allocation in farm management. *Wisconsin.*

RUSSELL F. MCDONALD, B.S. Ohio State 1950; M.S. 1958. Analysis of the costs and returns from sorting fleece wools for market in the producers warehouse and the fleece wools pricing structure. *Ohio State.*

VERNON C. MCKEE, B.S. Kansas State 1956; M.S. 1957. Economic implication of contract farming. *Iowa (Ames).*

RUSSELL E. MOFFETT, B.S. Michigan State 1955. Water resource investment, with particular attention to uncertainty. *California (Berkeley).*

OSCAR K. MOORE, B.S.A. Florida 1938; M.S. Maryland 1940. The Brazilian coffee economy. *Florida.*

WILLIAM C. MOTES, B.S. Kansas State 1954; M.S. 1958. A spatial equilibrium study of location factors in the livestock-meat industry. *Iowa (Ames).*

JAMES A. MUNGER, B.A. Yankton 1952; M.S. South Dakota State 1955. Land tenure adjustments in an agricultural-forestry transition area. *Wisconsin.*

FRED L. OLSEN, B.S. Wisconsin State 1950; M.S. Minnesota 1955. An economic analysis of factors affecting the supply of grade A milk in the upper Midwest. *Minnesota.*

RONALD H. POLLOCK, B.S. Ohio State 1948; M.S. 1949. An analysis of changes in consumer milk purchases in two Ohio metropolitan areas. *Ohio State*

LEONIDAS POLOPOLUS, B.S. California (Davis) 1955. Market structure and industry organization in the production and processing of beet sugar: a study of federal regulation and control. *California (Berkeley).*

LYNN RADER, B.S. Colorado State 1949. Economic analysis of range and pasture improvement practices in beef cattle ranch management in the Sierra Nevada region of California. *California (Berkeley).*

ROBERT M. REESER, B.S. Ohio State 1948; M.S. 1956. Formulation of a desirable long-range land use program for Ohio. *Ohio State.*

WILLIAM G. RHOADS, B.S. Mass. Inst. Technology 1951. Exhaustible resource valuation, allocation and public policy. *Mass. Inst. Technology.*

GEORGE E. SCHUH, B.S. Purdue 1952; M.S. Michigan State 1954; M.A. Chicago 1959. The supply of labor in American agriculture. *Chicago.*

RAM R. SINGH, B.S. Agr. College, Kaupur, U.P. India, 1944. Organization of the resources of contiguous groups of small farms in a central Ohio country for maximum economic return. *Ohio State.*

MELVIN W. SMITH, B.S. Alabama Polytechnic Inst. 1951; M.S. 1958. An analysis of consumer preferences and buying habits in purchasing fresh fruits and vegetables. *Ohio State.*

NORTON E. SMITH, B.S. Illinois 1951; M.S. Purdue 1955. A least cost hog procurement system for Indiana packers. *Purdue.*

JAMES C. SNYDER, B.S. Ontario 1953; M.S. Purdue 1956. Application of operations-research techniques to meat packing. *Purdue.*

N. PANDANDA SUBAIYA, B.S. Karnatak Univ. India, 1956. M.S. Minnesota 1958. Farm management under Indian social conditions. *Minnesota.*

KENNETH H. THOMAS, B.S. Cornell 1950; M.S. 1956. Evaluation and refinement of the Minnesota farm possibility techniques as a guide to farm planning. *Minnesota.*

ROBERT W. THOMAS, JR., B.A. Iowa 1948; M.A. New Mexico 1951. Economic analysis of Iowa's surface water. *Iowa (Ames).*

JAMES F. THOMPSON, B.S. Murray State Teachers 1949; M.S. Kentucky 1951. Part time farming and resource productivity. *Chicago.*

MARVIN W. TRAUTWEIN, B.S. Illinois 1956; M.S. Iowa (Ames) 1958. Effects of new industry on the agricultural economy of eastern Iowa. *Iowa (Ames).*

YIEN-I TU, B.S. National Taiwan Univ. 1953; M.S. Iowa (Ames) 1958. Product acceptability in relation to the demand for meat. *Iowa (Ames).*

LILY VERMA, B.A. Univ. Rangoon (Burma) 1953. Resource use and productivity in Thailand agriculture. *California (Berkeley).*

RAYMOND D. VLASIN, B.S. Nebraska 1953; M.A. 1957. The impact of highway land acquisition on farm property owners and operators. *Wisconsin.*

LARKIN B. WARNER, B.A. Ohio Wesleyan 1956; M.A. Indiana 1958. The economics of the movement of Ohio coal. *Indiana.*

EDWIN R. WESTCOTT, M.S. Ohio State 1950. Optimum combination of resources in dairying. *Ohio State.*

THOMAS WISE, B.A. Cambridge 1948; M.A. 1951. Methods of using surplus rural labour in Pakistan. *McGill.*

## Labor Economics

### *Degrees Conferred*

WALLACE N. ATHERTON, Ph.D. California (Berkeley) 1959. Some aspects of trade union policies and behavior: a theoretical analysis.

ROBERT I. DESROCHERS, Ph.D. Fordham 1959. The cost of living escalator clause in the General Motors-UAW 1948 and 1950 contracts.

WARREN EASON, Ph.D. Columbia 1959. Soviet manpower.

ELBERT T. EGGERS, D.B.A. Indiana 1958. Employment of the physically handicapped in selected industrial plants in metropolitan Atlanta.

WALTER L. EISENBERG, Ph.D. Columbia 1959. Government policy in sitdown strikes.

HARRY GREENFIELD, Ph.D. Columbia 1959. The theory and practice of sliding scale wage agreements in the United States.

FRED HARTENSTEIN, Ph.D. Pittsburgh 1959. The federal government treatment of secondary boycotts.

JOHN D. HOLMES, Ph.D. Illinois 1959. An economic analysis of the interrelationships between unions and technology, 1947-57.

PAUL G. KEAT, Ph.D. Chicago 1959. Changes in occupational wage structure, 1900-1956.

ROBERT R. KESELL, Ph.D. Mass Inst. Technology 1959. The role of the international union representative in the American labor movement.

ROBERT E. L. KNIGHT, Ph.D. California (Berkeley) 1958. A history of industrial relations in the San Francisco Bay area, 1900-1918.

JOHN J. KORBEL, Ph.D. Harvard 1959. A decision unit model for the labor force.

JOHN W. LEONARD, Ph.D. Cornell 1958. Industrial, regional, and county patterns of union organization in the post World War II period: a test of generalizations.

MELVIN LURIE, Ph.D. Chicago 1958. The affect of unionization on wages in the transit industry.

DONALD J. MCCLURG, Ph.D. California (Berkeley) 1959. Labor organization in the coal mines of Colorado, 1878-1933.

BEN MILLER, Ph.D. New York 1959. An analysis of employee attitudes toward methods changes in the white collar field—a study of six companies.

CYNTHIA T. MORRIS, Ph.D. Yale 1959. Industry-wide collective bargaining and nationalization.

FRED C. MUNSON, Ph.D. Mass. Inst. Technology 1959. Collective bargaining in the lithographic industry.

RAYMOND MUNTS, Ph.D. Wisconsin 1959. Bargaining for health: union experience with health insurance and medical care in the U.S.

BARBARA W. NEWELL, Ph.D. Wisconsin 1959. Metropolitan unionism—a case study of Chicago in the Nineteen Thirties.

PHILIP G. OLSON, Ph.D. Purdue 1959. Socio-economic factors affecting labor mobility in an Indiana rural community.

LELAND E. OTT, Ph.D. Purdue 1959. Labor utilization in independent Indiana supermarkets.

THOMAS ROBERTS, Ph.D. Pennsylvania 1959. History and analysis of labor-management relations in the Philadelphia transit industry.

Charles P. Sawaya, Ph.D. Indiana 1959. The employment effect of minimum wage regulation in the Southern pine lumber industry.

Joseph Schuster, Ph.D Georgetown 1959. Union management cooperation in job-skill training programs in the contract construction industry in the United States, 1932-1957.

Norman J. Simler, Ph.D. Minnesota 1959. The impact of unionism on wage-income ratios in the manufacturing sector of the economy.

Lucille D. Swaim, Ph.D. Radcliffe 1959. Some problems of an orphan union.

Arthur N, Turner, Ph.D. Cornell 1958. Impersonality and group membership—a case study of an automobile assembly line.

Bernard Udis, Ph.D. Princeton 1959. Technology, worker mobility and local wage differentials.

Roger W. Walker, Ph.D. Cornell 1959. An evaluation of a pre-retirement planning program: the TVA experience.

William D. Wood, Ph.D. Princeton 1959. An analysis of office unionism in Canadian manufacturing industries.

## *Theses in Preparation*

Theodore M. Alfred, B.A. Doane 1949; M.B.A. Northwestern 1951. Application of labor market analysis to the internal staffing function in large firms. *Mass Inst. Technology.*

Abraham H. Belitsky, B.A. Wisconsin 1952; M.A. Syracuse 1953. Hiring practices in the building trades. *Harvard.*

William L. Blachman, B.S. Wisconsin 1955; M.S. 1957. A case study in industrial conflict: the Kohler strike. *Wisconsin.*

Thomas G. Carpenter, B.S. Memphis State 1949; M.A. Baylor 1950. The economic aspects of the use of foreign labor in Florida agriculture. *Florida.*

Harvey Clermont, O.F.M., B.A. Capuchin Seminary of Mary Immaculate 1950; M.A. Catholic 1956. Organizing the insurance worker. *Catholic.*

John P. Cullity, B.S. St. Peter's 1952. The growth of government employment 1900-1955. *Columbia.*

Richard L. Danfield, B.S. Wisconsin 1951; M.S. 1956. The "bargaining fee" as the answer to the union security problem. *Wisconsin.*

Ernest M. DeCicco, B.S. New Hampshire 1949; M.A. 1951. The influence of market structure and productivity on union bargining power and inflation. *Boston.*

Nicholas DeWitt, B.A. Harvard 1952; M.A. 1953. The economics of professional manpower in the USSR. *Harvard.*

Lawrence Donnelly, M.B.A. Xavier 1957. Comparative analysis of the wage structures for different occupations and different occupations and different labor market areas. *Cincinnati.*

Robert Evans, Jr., B.S. Mass. Inst. Technology 1954. The economics of slavery. *Chicago.*

Thomas A. Finegan, B.A. Claremont 1951; M.A. Chicago 1953. Hours of work in the United States—a cross sectional analysis. *Chicago.*

Mary A. Frisbie, B.S. Temple 1942; M.A. New York 1944. Data processing equipment, related office machines, and office employment 1932-1957. *New York.*

Donald Garnel, B.A. Queens 1955. The impact of labor unions on market structure. *California (Berkeley).*

Howard M. Gitelman, B.A. Pittsburgh 1954; M.S. Wisconsin 1957. A partial theory of the labor movement. *Wisconsin.*

Harry L. Hall, B.S. Columbia 1939; M.A. 1943. Wage differentials in theory and practice. *Southern California.*

ARTHUR M. KRUGER, B.A. Toronto 1956. Labour-management relations in the Canadian basic steel industry. *Mass. Inst. Technology.*

JOHN J. MACKIN, B.A. Carroll 1940; M.A. Catholic 1951. National labor policy and the union shop. *Catholic.*

GARTH L. MANGUM, B.S. Brigham Young 1956. Wildcat strikes and union pressure tactics in American industry. *Harvard.*

PHILIP MARCUS, B.A. Bucknell 1952; M.A. Rutgers 1955. Seasonal farm labor in New Jersey. *Rutgers.*

CHARLES F. MARTIN, B.A. Wayne 1950; M.A. Mississippi 1957. Unionism in the federal departmental service, 1949-59. *Louisiana State.*

JOAN M. MCCREA, B.A. Indiana; M.A. California (Los Angeles) 1958. Wage determination in Los Angeles retail trade. *California (Los Angeles).*

MARTIN P. OETTINGER, B.A. City (New York) 1952; M.A. Brown 1956. Economic policy and the level and structure of wages in the Netherlands since 1945. *Harvard.*

JAN PARKER, B.A. Connecticut College for Women 1954; M.A. Connecticut 1956. Age as a factor in earnings differentials. *Brown.*

SANTO PULLARA, B.S. Syracuse 1952; M.B.A. 1953; LL.B. 1956. Factors affecting growth of small business retirment plans. *Syracuse.*

CHARLES E. ROCKWOOD, B.S. Illinois 1954; M.B.A. Western Reserve 1957. A practical basis for the granting of sector by sector wage adjustments. *Indiana.*

PETER SCHRAN, Diplom Volkswirt, Free Univ. Berlin 1954. Problems of the Chinese work force. *California (Berkeley).*

SAMUEL N. SEIDMAN, B.A. Brooklyn 1955. A history and study of the Philippine labor movement. *New York.*

NORMAN J. SIMLER, B.S. Georgetown 1949; M.A. 1953. The impact of unionism on wage-income ratios in the manufacturing sector of the economy. *Minnesota.*

KEITH SLOANE, B. Ec. Univ. Queensland, Australia, 1956. "Wage drift" under a government wage policy. *Duke.*

CARL D. SNYDER, B.S. Heidelberg 1944; M.A. Miami (Ohio) 1948. Union organization of white collar workers in the automobile industry.

VLADIMIR STOIKOV, B.S. Illinois 1953; M.S. Wisconsin 1955. The allocation of scientists. *Johns Hopkins.*

ISKANDER TEDJASUKMANA, M.S. Cornell 1958. Development of labor policy and labor legislation in Indonesia, *Cornell.*

SISTER MARY ELOISE THOMAS, BA. Mundelein 1947; M.A. Notre Dame 1955. A study of the causes and consequences of the economic status of the migratory farm worker in Illinois, Indiana, Michigan, and Wisconsin, 1941-1958. *Notre Dame.*

WILLIAM D. WAGONER, B.B.A. North Texas State 1953; M.A. Texas 1957. The non-free worker in post Civil War American history. *Texas.*

## Population; Welfare Programs; Standards of Living

### *Degrees Conferred*

LEON APPLEBAUM, Ph.D. Wisconsin 1959. A history of voluntary health insurance in Wisconsin.

BARBARA R. BERMAN, Ph.D. Radcliffe 1959. The projection of regional consumer expenditures.

GERALD A. P. CARROTHERS, Ph.D. Mass. Inst. Technology 1959. Forecasting the population of open areas.

JEAN R. PEARMAN, Ph.D. Minnesota 1959. A survey of economic security programs in Michigan.

ELLA J. POLINSKY, Ph.D. American 1959. Some implication of the employment patterns of women under social security.

SOM PRAKASH, Ph.D. *Virginia 1959. Demographic changes and economic growth in India: a study of some basic problems in Indian economy since Independence.*

## *Theses in Preparation*

ALAN B. BATCHELDER, B.A. Ohio Wesleyan 1953; M.A. Harvard 1955. Negro migration and technological innovation. *Harvard.*

ALFRED J. BURNS, B.S. Louisiana 1954; M.S. 1956. An analysis of consumer food purchasing problems. *Purdue.*

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FLOYD A. FARRAR, B.A. Fisk 1949; M.A. Rutgers 1950. Population movements and industrialization in Canada, 1921-1951. *Pennsylvania.*

THOMAS M. JOHNSON, B.A. Chicago 1948; J.D. 1951. Characteristics of the uninsured unemployed in the post-war period. *Columbia.*

ELEANOR M. SNYDER, B.A. Connecticut College for Women 1936; M.A. Columbia 1938. Economics of low family incomes. *Columbia.*

## VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

### *Vacancies*

*Accounting:* Senior-level position at midwest university. Desired qualifications are: doctoral degree, C.P.A. certificate, teaching experience at the university level, and publications. Salary and rank commensurate with qualifications. P220

*Economics and Tax Research:* The California State Board of Equalization currently has a vacancy for Chief, Intercounty Equalization Division. This administrative post requires broad and extensive experience in real property valuation, economics, tax research work or in teaching of economics at the collegiate level. Starting salary $12,000 with annual increments to $13,800. Apply before October 2, to the California State Personnel Board, Department G, 801 Capitol Avenue, Sacramento 14, California.

*Economist:* To do research and consultant work with the Department of the Treasury, San Juan, Puerto Rico. Position will be on Civil Service basis. Ph.D. degree, or advanced work leading to it, required; specialization in public finance and/or money and banking desirable. No previous experience required. Annual salary $7,200 plus travel expenses to and from Puerto Rico. Persons interested please write to the Director, Office of Economic and Financial Research, Department of the Treasury, P.O. Box 4515, San Juan, Puerto Rico.

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## *Economists Available for Positions*

*Business administration:* Man, 54; M.A., Yale, M.S. Columbia; honorary doctorate. Broad business experience as well as teaching; excellent references. Desires position teaching investment, finance, and international relations. Available in fall, 1959. E837

*Economic theory, history of economic thought, economic fluctuations, money and banking, international economics and trade, economic problems of underdeveloped countries (especially Middle and Southeast Asia):* Man, 28; B.A., M.A., currently writing Ph.D. dissertation. Teaching and administrative experience. Interested in teaching and/or research. Available in fall, 1959. E840

*Principles, money and banking, economic thought, business trends, real estate, labor relations, research, business law, insurance:* Man, 38, married; Ph.D. Eleven years of successful college teaching experience; 4 years in academic administration; now department head, economics and business; has had experience in real estate and insurance. Will consider university teaching or heading a department if position represents an advancement. E842

*Economic principles, comparative economic systems, sociology, criminology:* Man, 34; attended Universities of Maryland and Wisconsin; M.A., Ph.D. almost completed. Three years of full-time and 2 of part-time university teaching; 9 months as civilian commodity-industry analyst, Department of the Army; at present contributing to a study (to be published soon) on the merger movement in selected industries which process wholesale and retail food. Desires a position in either college teaching or economic research. E844

*Economic theory, labor economics, labor-management relations, comparative economics, economic thought, business and government:* Man, 41, married; Ph.D., University of Pennsylvania; Phi Beta Kappa. Listed on arbitration panels of Federal Mediation Service and American Arbitration Association; experienced teacher; author of book published by a leading university press and journal articles. Private labor arbitration and administrative experience in federal government. Currently full professor in liberal arts college. Desires change in June or September, 1960, especially to Rocky Mountain states, southwest, or Pacific Coast areas. E845

*Economics:* Man; Dr. rer. pol. from German university (*summa cum laude*): Twelve years of university teaching; banking experience; publications. Seeks teaching position. E846

*Economic principles, statistics, labor, accounting, public administration:* Man, 34, white; A.B., Ohio University; M.A., Maxwell Graduate School, Syracuse University. Six years of accounting and 4 years of sales experience. Desires position at small university or liberal arts college in southern or southwestern section of country wherein scholastic endeavor is foremost. Negro institution would be acceptable. Salary secondary to chance of working under guidance of select staff. Available immediately. Please write John J. Oross, 197 East Street, Wadsworth, Ohio.

*Agricultural economics, economic development, economic theory, population problems:* Man, 37, married; Ph.D., University of Chicago. Seven years of teaching and research experience; 5 years of business experience. Contributor of articles to national and regional economic journals. Desires academic position with opportunity for research. Currently Fulbright Professor in Latin America, conducting lectures in Spanish. Available in January, 1960. E847

*Economic theory, finance, labor economics:* Man, in forties; Ph.D., large eastern university. Five years of full professorship (3 at large state university); experienced professional and industrial research and writing. Enjoys space-age mental challenge and teaching. Listed in *Who's Who in America, Who's Who in Commerce and Industry*, etc. E848

*Economics:* Man, in early forties; M.A. and Ph.D. in economics. Years of teaching and research experience; now on the faculty of a medium-sized university. Desires a teaching position in the southwest with above-average responsibilities. E849

# The American Economic Review

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## CHOOSING AMONG ALTERNATIVE PUBLIC INVESTMENTS IN THE WATER RESOURCE FIELD

*By* PETER O. STEINER*

Should the federal government have permitted the Idaho Power Company to develop Hells Canyon or should it instead have developed a public project? If the latter, should it have built one high dam, two intermediate-size dams, or three smaller ones? Questions like these, involving the amount and form of public investments, require criteria for evaluating the investments and operational guides for applying the criteria. In the present paper attention will be focused on the problem of choosing among alternative public expenditures with particular reference to water resource development.[1]

As a classical problem in efficient resource allocation, there is a solution that is both simple and well known. In an efficiency-oriented mold where there are no barriers to the flows of funds or resources, where "benefits" and "costs" are correctly determinable, it is evident that

* The author, professor of economics at the University of Wisconsin, gratefully acknowledges the assistance of the Social Science Research Council whose faculty research fellowship made this research possible. The Water Resources Planning and Development Project of the Graduate School of Public Administration, Harvard University, provided stimulus, assistance and a forum for discussing the ideas with a wide assortment of experts. Too many people have read and commented on earlier drafts of this paper to be individually named, but my thanks to them all. Robert Dorfman, Otto Eckstein, Stephen Marglin and Roger Miller have been particularly helpful.

[1] While the focus is on water resources, I believe the analysis can be readily extended to other aspects of the applied theory of public expenditure. Water resource development is particularly suitable for analysis for several reasons: (1) it involves the critical border between public and private expenditures, which are often truly alternatives; (2) it presents enormously complicated practical problems of choice among alternatives that can serve to illustrate merits (and limitations) of the analysis; and (3) it has received a remarkable wave of attention in recent years from professional economists. See, most recently: O. Eckstein [3], J. V. Krutilla and O. Eckstein [5], R. N. McKean [6]. Margolis' able review article of these three books [7] notes with exceptional clarity certain of the fundamental difficulties that the solutions offered in these contributions encounter.

The parent discussion of the federal government [4] is known, and hereafter cited, as the "Green Book."

appropriate policy is to build every project for which benefits exceed costs and to develop every project to the point where marginal benefits equal marginal costs.[2]

There are several reasons why this solution is not satisfactory.

1. The pure efficiency criterion is not dominant in determining the volume of public expenditures. Other sensible dimensions of public welfare such as cyclical stability, economic growth, income distribution, and the size of government are (and should be) significant determinants of the volume of public expenditures. For this reason there may be effective budget constraints on the expenditure possibilities. Efficiency has a role but it is within limits established by these constraints. Such constraints imply opportunity costs:

(a) When budgetary constraints leave the marginal benefit-cost ratio different in the public and private sectors, there is an opportunity cost of transferring dollars from one sector to another.[3] Evidently the size of this opportunity cost will vary according to the way in which funds are transferred, and the level of resource use in the sector from which transferred.

(b) So far as budgets are constraining, otherwise meritorious projects will not be undertaken. There is thus an opportunity cost of foregone public alternatives. Specifically this will imply development of projects to a point short of the level where marginal benefits equal marginal costs.[4]

2. Because of the pre-emptive right of government in certain areas, a positive decision with respect to some public project may preclude (I shall subsequently use the word *displace*) an alternative private development. The significance of such displacement is that it involves loss of those public benefits that would arise through private endeavor. But the opportunity cost of such displacement is not usually the whole of the benefits from private development, since displaced funds may be employed elsewhere. It is only the "superior attractiveness" (which I will call *supramarginality*) of the specific displaced project over *its* marginal alternative that is a proper opportunity cost of displacement. Because of pre-emption, these private opportunities may be exception-

[2] Eckstein [3, pp. 47-73] provides a nice derivation from classical welfare economics.

[3] Krutilla and Eckstein [5, Ch. 4] provide a "special case" solution designed to recognize this. I will refer to modifications that recognize one (but not all) of the crucial complications as special cases, in contrast to the more general case developed in this paper.

[4] This special case is recognized by Eckstein [3], where he suggests using a marginal benefit-cost cut-off ratio of $1 + v$, where $v$ is so chosen that the list of all projects so designed for which total benefits exceed costs just exhausts the scarce budget. For similar reasons the Green Book [4] rejects maximizing benefits minus costs as a criterion since it favors large projects over small ones. McKean [6] also addresses this problem. His procedure is to choose the rate of discount in such a way that building all projects for which marginal benefits equal marginal costs will just exhaust the budget.

ally attractive. The extent to which private marginal alternatives have relevance will depend upon the level of employment of resources.[5]

3. A specific public program will consist in some selection of individual projects (e.g., dams of specified heights as specified locations) from among a larger number of possible projects. Not only are the benefits and costs of particular projects interdependent (and thus the value of a specific dam depends upon what other dams are built) but some projects will be mutually incompatible. This incompatibility may be of two kinds: it may be purely technological (it is not possible to build two dams on the same site, or to use a given increment of a specified dam for two conflicting purposes at the same time); it may be purely economic (a specified community requiring X units of electric power may have a choice among any of a series of hydro sites and also a series of possible steam-generating plants; but if every such project is capable of producing the required X units, any two such projects will be economically incompatible). The significance of this is to complicate enormously the choice among alternative projects.

4. Discreteness of individual projects is essential for an operational solution to the problem. Not only is discreteness sometimes required for technological reasons, but estimation of the magnitudes of benefits and costs on the basis of potentially available data requires comparison of discrete alternatives.

All of these considerations taken together make choice of the "best possible program," given the restraints, a complex one which no simple marginal equivalence can handle. It is the primary purpose of this paper to provide an *operational* framework for choosing efficient programs (and thus also program elements, or projects) subject to appropriate budget restraints, incompatibility restraints, and discreteness restraints while recognizing (or approximating) the full range of opportunity costs of any action.

The next sections develop a model which will suggest how to assign a numerical value to every potential project so that it may be directly compared with every other potential project; and thus the program (combination of projects) may be chosen that will maximize the appropriate welfare criterion subject to all appropriate restraints. The essential features of this model are: (1) its recognition of the general equilibrium character of the consequences of a specified public investment decision that result from both the source(s) of funds and the displacements (if any) that occur; a central conviction of the model

[5] Stephen Marglin has called my attention to the fact that displacements may be negative as well as positive—that a public project may *create* supramarginal private investment opportunities. That this possibility (of what are often called secondary benefits) can be handled within the framework of the model developed below is neglected in the subsequent exposition.

is that second- and even third-round effects may not be negligible; *(2) its framing of decision rules in such a form that data for their application* are likely to be available and that can be applied by modern techniques of numerical analysis; (3) its generality, which embraces a whole series of potentially relevant special cases.

This model by focusing on the choice problem neglects other important problems. Specifically, it takes as given (a) the identification of the feasible components of a program,[6] and (b) the determination of the net benefits that accrue from using these components singly or in specified combinations. These "givens" represent difficult and important elements in the over-all problem of water resource development, and there is no intention of minimizing their importance.

An initial oversimplified example may serve to illustrate the central difference between the present approach and conventional benefit-cost analysis. Suppose a choice must be made between two public projects, A and B, for which the relevant basic data are as follows:

| | *Project A* | *Project B* |
|---|---|---|
| Net benefits (net of all costs, appropriately discounted) | 100 | 100 |
| Cost to government | 70 | 70 |
| Source of government funds: | | |
| from existing budget | 50 | 70 |
| from increased taxes | 20 | 0 |
| Displaced private project: | yes | no |
| Net benefits | 40 | 0 |
| Private investment | 30 | 0 |

Notwithstanding the identical benefits and costs, the two projects are not identical in their effect. Project A if undertaken will use $20 less of the scarce public budget, but will leave $20 less in taxpayers' hands; additionally, Project A will displace a supramarginal private investment. Project A will be preferred if, but only if, the net benefits produced by the additional $20 within the public budget exceed the combined net benefits foregone by the transfer of funds through taxation and by foregoing of the supramarginal investment opportunity.

## I. *The Model*

### A. *The Function to Be Maximized*

Recognizing the general equilibrium nature of a public investment decision, the objective function is the present value of the difference

[6] Feasible, as used here, does not mean compatible. If, say, a dam can be built at three different sites, at five different heights at each site there will be 15 feasible component projects. The choice technique developed can easily handle incompatibility restraints.

between total benefits and total costs *in all sectors of the economy.*[7] While measurement of benefits and costs is not discussed in this paper, what is meant in principle is the market values (actual or imputed) of the stream of goods and services produced and of the resources used. Present values are the time streams of anticipated benefits and costs discounted by an appropriate rate of discount. This objective function comprehends the indirect as well as the direct effects of a public expenditure.

The role of the rate of discount in this analysis is to permit the evaluation at a point of time of streams of benefits and costs that occur over time; it is a metric for comparing unlike time profiles. It is the opportunity cost of deferred consumption (some would call it "social time preference"), and should not be confused with a number of other interest rates that may be appropriate in other connections.[8] It will be designated as $\pi$.

In choosing to maximize a concept embodying net benefits, it is evident that attention is being directed toward an efficiency objective. Are we making this the overriding criterion for decision? Were this an unconstrained maximum, the answer would be yes; for a constrained maximum, the answer is no. The size of the budgets that are restraining leaves economic efficiency in a relatively secondary role. Within the limits imposed, efficiency seems the sensible criterion.

### B. *Sectors of the Economy*

While the objective function comprehends the general equilibrium effects in the entire economy, it is evident that it is necessary to consider only those sectors on which a public expenditure (of the kind herein considered) will have an impact. Our sectors may, therefore, comprehend less than the entire economy. If, as seems likely, a specific

[7] The use of *net* benefits may be questioned. Why should we be concerned with costs at all —why not maximize the discounted value of *gross* benefits subject to a whole series of resource restraints, one for each resource used? In principle this procedure seems superior, but it would require inclusion of the benefits of deferred use of scarce resources in the value of the discounted benefit stream. Use of net benefits assumes that the market-determined factor prices (which lead to costs) attach the appropriate reservation values to resources not currently used, and thus saves a totally impossible burden of estimation. Every restraint used implies a shadow "factor price" for the restrained resource; we use such restraints (and hence such shadow prices) only for the key constraining budget or budgets.

[8] Among them: the government borrowing rate; the private borrowing rate; the opportunity cost of transferring funds from the private sector to the public sector; the internal rates of return of marginal projects in any sector; a risk or uncertainty premium; the rate of discount used in the private capital market to evaluate unequal time streams. As we have need for any of these, perfectly sensible, measures they will be introduced. But they are not necessarily equal to the opportunity cost of deferred consumption of net benefits. Some of these (e.g., interest actually paid) are included in the costs subject to discounting. Under some institutional circumstances some or all of these rates may coincide, but there is no necessary reason why they must.

governmental decision in the water resources field will not affect the size of the defense budget, that budget need not be included. In a truly general equilibrium sense, of course, everything affects everything else. But the imposed constraints, in the form of budgets, may in fact provide significant insulation against many kinds of effects. For the present, consider four sectors (two public and two private) as defined in the following paragraphs. Some expansion of this number may be necessary, as is noted subsequently.

*The Public Sectors (S1 and S3).* Sector 1 is defined to include the set of projects among which direct choice is to be made. It might include all water resources projects everywhere in the nation under the jurisdiction of the agencies selecting projects and allocating budgeted funds.[9] It might be defined more broadly to include all public works projects, or it might be defined much more narrowly. What is controlling is (a) the jurisdiction of the budget that is controlling, and (b) the range of alternatives that are to be specifically considered for inclusion. The more broadly it is defined, the greater is the scope that is given to the "efficiency" criterion in choosing outlets for public expenditure.[10] Specific projects in Sector 1 are designated by the subscript $ij$.[11]

But while the appropriate budget comprehends all projects in this sector, it is not so limited. Not all the possible public projects for which benefits exceed costs are explicitly considered. Since both project design and benefit and cost estimation are expensive activities it is neither feasible nor sensible to develop plans for them if they have a low probability of being built because of the limited budget. Sector 3 is viewed as the reservoir of such inchoate projects. If, because of a lack of meritorious projects in Sector 1, or because of the lumpiness of discrete projects in Sector 1 there are extra funds, funds become available for expenditure in Sector 3. Consideration of Sector 3 thus serves two purposes: first, it requires that the yield of any project from Sector 1 included in the final program exceed the yield of any project not specifically considered, and second, it provides recognition of the fact that unutilized funds have some productive value.

[9] Whether state and municipal projects are included here or treated in a private sector depends upon how decisions are in fact to be made. If an integrated intergovernmental agency is allocating pooled funds over pooled opportunities, the entire pool is in Sector 1; if state and municipal funds are independently administered, they are not included in Sector 1. Hereafter we assume the latter.

[10] The sensible definition must avoid triviality or unusability. If the budget is defined overnarrowly—e.g., to apply to a specific dam at a specific location—the consideration of the value of that dam vis-à-vis other possible projects is lost. If defined too broadly—e.g., to cover all federal expenditures—it must become nonoperational. But whether it should cover specific river basins, or comprehend all water projects I leave to others.

[11] The double subscript is designed to recognize that a project involves use of a certain facility ($i = 1, 2, \ldots i^*$) for a certain purpose ($j = 1, 2, \ldots j^*$). The coding of projects plays an important role in recognizing incompatibilities, as will be discussed below.

Sector 3 thus plays a role analogous to "slack activities" in linear programming and results in making the total expenditure from the restrained budget in the two public sectors *definitionally* equal to the amount of that budget. In what follows we assume, for simplicity, that the yield (to be defined) per dollar in Sector 3 is an estimable magnitude that can be represented by a constant, $a_3$.

*The Private Sectors (S2 and S4).* Sector 2 consists of the relevant private projects that are alternative to the projects included in Sector 1 and that will thus be displaced if the public projects are undertaken. The *j*th project is the private alternative to the one or more public projects for providing the *j*th service.[12] If there is more than one private alternative for each public project, the one included in Sector 2 is the one (or the set of compatible ones) that will actually be undertaken. This may be a matter that the government can specify (via licensing, for example) or it may be the one that is most attractive to private investors. It must be made clear that the projects in this sector are actual alternatives, not idealized "private alternatives." As such, the quantity and/or quality of the private service may differ from that comprehended in the public alternative.[13] If there is no alternative that would be undertaken, it is convenient to imagine a dummy alternative whose costs and benefits are both zero. By this definition, Sector 2 consists of a set of $j^*$ elements.

Evidently we are assuming that public authorities have first choice: by building a specific project ($ij$) they preclude the *j*th private project; by failing to provide the *j*th service by one of the $ij$ public projects they (in effect) have left it to be provided by the *j*th private project.

Sector 4 consists of what may be called the pool of marginal private investment opportunities. Need for this sector is due to the fact that the real opportunity cost of displacing a private project is the supramarginality of that project, not its total net benefits. For simplicity it is assumed that this sector consists of a homogeneous stock of investment

[12] We here assume that it is the *j*th public *service* that displaces a private project providing a substitute source of that service. The assumption may be wrong. Facilities as well as services can displace. For example: a specific public flood-control dam not only may displace a private flood-control project but, by pre-empting a location, may displace (say) a private power plant as well. The appropriate procedure is to regard the projects in Sector 2 as the private projects actually displaced by the $ij$ public projects—for whatever reason. This causes no difficulties unless there are internal incompatibilities *within* Sector 2.

Such incompatibilities, while unlikely because of the definition of projects in Sector 2 as actual (rather than fictional) alternatives, can occur. If they do, they must be recognized by additional incompatibility constraints, and they complicate somewhat the techniques of solution discussed below. In no case will they prevent solution.

[13] For example, a series of public projects for providing respectively 90, 150, and 200 units of hydro power may all potentially displace the same 100-unit private steam-generating plant.

opportunities whose yield per dollar can be represented by a constant, $a_4$.[14]

If there is no alternative investment, that is, if funds displaced remain wholly idle, we regard a dummy alternative for which $a_4 = 0$. In this way it is appropriate to regard the sum of the private investment in Sectors 2 and 4 as being a constant, unless because of the adoption of some project or program there is a transfer of funds from the private to the public sector (for example, via taxation). Put differently, if the public development occurs wholly within the preassigned budget limit, we assume it can affect the productivity of private investment (by displacing supramarginal investment opportunities) but not the volume of that investment.

If adoption of a specific public program causes a net increase in the size of the restraining budget, it will imply a transfer of funds into the public sectors, and the opportunities thus foregone must also be considered.

### C. *Forms of the Objective Function*

The function to be maximized (subject to subsequently enumerated restraints) can be written as follows:

$$N = \sum_{S1} x_{ij}G_{ij} + \sum_{S2} x_jG_j + \sum_{S3} x_pG_p + \sum_{S4} x_pG_p \tag{1}$$

where the $G$'s are the "gains," i.e., the present values of the benefits over all costs, and the $x$'s represent the level of specific projects and $p$ is a running subscript. For our discrete case, each project has a level of either zero (it is not undertaken) or one (it is undertaken). In this form, the objective function does not help solve anything. But as is shown in the appendix, it is equivalent to find the maximum (subject to the same restraints) of the following function:

$$Z = \sum_{S1} x_{ij}y_{ij} \tag{2}$$

where

$$y_{ij} = (G_{ij} - a_3k_{ij}) - (G_j - a_4l_j) - a_2m_{ij} \tag{3}$$

[14] The value of $a_4$, as will subsequently be shown, depends upon the internal rate of return of marginal investment opportunities that are the alternatives to private investment in specific, displaceable alternatives to public projects. The homogeneity of these is in fact doubtful—public utilities, municipal governments, and large private corporations surely have different "cut-off" rates of return. For this reason it would probably be appropriate to use a few broad subclasses of "marginal private investments" with separate values of $a_4$ for each. This creates no difficulties in the analysis, but would complicate the exposition considerably.

and where:

$a_3$ and $a_4$ have the meanings previously assigned.
$a_2$ is opportunity cost per dollar of funds transferred from the private sectors to the public sector.
$k_{ij}$ is the drain of the $ij$th project on the limiting budget.
$l_j$ is the capital cost of the $j$th private project.
$m_{ij}$ is the number of dollars transferred to the public sector as a result of the adoption of the $ij$th public project.
$\bar{k}_{ij} = k_{ij} + m_{ij}$.

Each $y_{ij}$ is a number. Equation (2) tells us that if we choose a program of projects in such a way as to maximize the sum of the $y_{ij}$, we will have maximized the basic objective function specified in equation (1).

The several terms in equation (3) have been arranged to facilitate interpretation. The first parenthesis consists of difference between the present value of net benefits of the $ij$th project and the present value that the scarce funds it takes from the limited budget would produce if applied outside of Sector 1.[15] It is thus the gain of the specific project over other public use of the limited public funds. The main function of this term is to permit direct comparison of alternative public projects of very different size, a problem that has worried many people and led to some foolish solutions.[16]

The second parenthesis represents the supramarginality of the private project displaced. It is only the supramarginality of any displacement that is truly an opportunity cost of that displacement.

The final term measures the opportunity cost of transferring $m$ dollars from the private sector to the public sector *in addition* to the previous budget. There is some disagreement about how frequent such transfers are. Probably for most specific projects considered, funds come entirely from within the previously assigned budget limits, but it is not impossible that in specific cases authorization of specific projects is

[15] Notice that we do not compare the gain of this project with that of another project in Sector 1 which may ultimately not be built for lack of funds. *That* opportunity cost comes in selection of the group of projects that maximize equation (2) subject to the budget restraints. We here compare the considered project with alternatives that are not specifically considered. This has a tremendous advantage over the procedure described in footnote 4; we do not have to predict before we start which projects will not be built—we do not have to assume the answer to our problem in order to arrive at it. It is perhaps possible that some iterative procedure could be used in that (other) approach, but I have not been able to find one that converged on the correct solution. The relevance of this in a practical sense is to the problem of suboptimization. I find no solution unless one can take subbudgets as being predetermined and acceptable.

[16] For example, attempting to maximize the ratio of benefits to costs.

accompanied by willingness to provide some increase in taxes and thus an increase in the total federal budget.

In sum, the *y's* represent gains net of three separate kinds of opportunity costs: (1) the cost of using scarce public funds, (2) the cost of displacing a supramarginal private project, and (3) the cost of the net transfer of funds from the private to the public sector that is specifically attributable to the adoption of the project. It should be noted that the *y's* are relative, not absolute, measures of net benefits over all opportunity cost.[17]

D. *Restraints*

Were it not for incompatibilities among projects and for budget limitations, choosing all projects for which $y$ is positive would assure a maximum. Given a budget restraint but neglecting incompatibilities, it would be sufficient to rank projects according to the size of the *y's* and move down the list until the budget was exhausted. But the combined existence of both budget restraints and incompatibilities are central *practical* features of the problem of choosing an optimal expenditure program. We hope to specify the restraints in such a way as to be consistent with techniques of solution of the choice problem.

*Budget restraint.*

(a) $$\sum_{S1} x_{ij}k_{ij} \leq K$$

here $K$ is the restrained budget.

What is the appropriate definition of $K$? Should it be the total budget r capital expenditure, total expenditure, total expenditure not reimbursible, or (as Eckstein [3] suggests) the present value of total penditures? There are interesting issues here, which space precludes scussing at this time. In terms of the model, any one can be implemented. Indeed, only slight modification is required to substitute a ries of budget restraints which differentiate according to type and timg of expenditures.

*Discreteness.*

(b) $$x_{ij}, x_j = 0, 1; \qquad \text{all } ij, j.$$

Assuming discreteness is essentially a matter of definition. Suppose imagine a dam at a given location which can, alternatively, be built

Equation (2) is equivalent to equation (1) for finding a maximum, but it is not identi- A number of constant terms, that affect the value of the function but not the location of naximum, have been dropped. See appendix.

at three different heights. This is most conveniently handled by defining the dam as three discrete (and mutually incompatible) projects, one corresponding to each of the feasible heights. Of course the height of a dam may be truly continuous, at least within limits; the procedure suggested is to choose (either by sampling techniques or by judgment) discrete levels for analysis.[18]

*Incompatibility Restraints.*

(c) $$\sum_i x_{ij} + x_j = 1; \qquad \text{all } j.$$

(d) $$\sum_j x_{ij} \leq 1; \qquad \text{all } i.$$

The first of these restraints recognizes that, of all the included public projects that could provide the $j$th service, not more than one can be chosen. Considering also the private alternative (which may be a dummy alternative), one and only one is always chosen.

The second restraint recognizes that the same facility (e.g., dam site) can be used only once. This does not mean it can be used to provide only a single function. If, for example, a given dam ($i$) can be used alternatively (a) for flood control, (b) for hydro-power, and (c) for some lesser amount of hydro-power plus some flood control, we envisage three $j$'s, one corresponding to each of the uses.

Evidently the coding of projects (defining $i$ and $j$) is critical. The fundamental purpose of this coding is to recognize the incompatibilities. In the absence of complementarities of uses and of benefits, this coding occasions few problems. Complementarities do exist, and they complicate matters. For example, if two compatible projects used together provide benefits different from the sum of the benefits received if they are used alone (because they are operated in series and outflows from one become inflows into the other), we must multiply the number of projects. For example, if two projects (1, 2) can be used in combination in three different ways ($a$, $b$, $c$) as well as separately, we must identify five different projects (1; 2; 1 + 2,$a$; 1 + 2,$b$; 1 + 2,$c$), which are mutually incompatible. In general all problems of coding, including the problems of complementarity, are solved by increasing the number of "projects" defined. Since the selection among projects is to be made by

[18] Benefit and cost functions, if they are presented continuously, usually result from fitting a function to discrete observations in any case. For many other cases data are estimable only for discrete levels. It is of course quite possible that the optimal height may be missed by this procedure, but if assumed discrete observation points are chosen so that the continuous function is monotonic between them, an iterative procedure is available with which to move from an approximate solution to a final optimal solution.

mechanical means, the extent to which feasibility is impaired depends upon the power (and cost) of computational techniques.

## II. *Problems of Solution*

*Any* attack on the water resource problem involves estimation of the benefits and costs of individual projects. This formidable task presumably provides the $G$'s of the present model, and is taken as given. The significant additional data required in this model are the values for the $a$'s, the opportunity costs not reflected in factor prices.

Each of the parameters, $a_3$, $a_4$, $a_2$, involves the public valuation of the net benefits of a dollar of expenditure outside of Sector 1 (the subscripts referring to the sector in which spent). To understand the factors determining the magnitudes of these parameters, consider the following simplified problem:

Suppose that for an initial expenditure of $v$ dollars, a net benefit stream of $B$ dollars per year for $T$ years is generated. Let $D(i)$ be the present value of $B$ dollars per year for $T$ years, discounted at the rate $i$.

$$D(i) = B \int_0^T e^{-iT} dt = B \frac{(1 - e^{-iT})}{i} .$$

Let $G(i) = D(i) - v$, the excess of present value over initial expenditure. Our parameters, $a$, are of the form:

$$a = \frac{G(\pi)}{v} = \frac{D(\pi)}{v} - 1$$

where $\pi$ is the public rate of discount. It is probable, in view of scarce budgets, that the $G(\pi)$, and thus $a$, are positive. For any project it is possible to find an internal rate of return, $r$, which will make $G(r)$ exactly zero, i.e.:

$$D(r) - v = 0,$$

$$D(r) = v$$

whence,

$$a = \frac{D(\pi)}{D(r)} - 1 = \frac{r}{\pi} \frac{(1 - e^{-\pi T})}{(1 - e^{-rT})} - 1$$

and thus the value of $a$ depends upon $\pi$, $r$, and $T$. Table 1 shows values of $a$ for selected values of $\pi$, $r$ and $T$:

TABLE 1—VALUES OF $a$

| $r$ | $T=25$ | | | $T=50$ | | | $T=100$ | | |
|---|---|---|---|---|---|---|---|---|---|
| | $\pi=.02$ | $\pi=.04$ | $\pi=.06$ | $\pi=.02$ | $\pi=.04$ | $\pi=.06$ | $\pi=.02$ | $\pi=.04$ | $\pi=.06$ |
| .02 | 0.0 | −.20 | −.34 | 0.0 | −.33 | −.50 | 0.0 | −.43 | −.62 |
| .04 | .24 | 0.0 | −.18 | .46 | 0.0 | −.27 | .76 | 0.0 | −.32 |
| .06 | .52 | .22 | 0.0 | 1.0 | .36 | 0.0 | 1.6 | .48 | 0.0 |
| .08 | .82 | .46 | .20 | 1.6 | .76 | .29 | 2.5 | .96 | .33 |
| .10 | 1.1 | .72 | .41 | 2.2 | 1.1 | .60 | 3.3 | 1.5 | .67 |
| .15 | 2.0 | 1.4 | .99 | 3.7 | 2.2 | 1.4 | 5.5 | 2.7 | 1.5 |

It is evident that when $\pi = r$, $a = 0$. This situation would necessarily pertain in, say, a purely competitive model in which (if there is more than one sector) funds would flow between sectors until the yield of the marginal projects in each sector was the same, and, further, equal to $\pi$, the rate of discount. That this need not be the case is due to the imposition of budgets that impede such free flow of funds, and further to the pre-emptive reservation of certain types of projects to the public sector. If such impediments exist, attention to differences in marginal alternatives must be recognized. The values of $a$ serve this purpose.

### A. *The Relevance and Size of* $a_3$

The constant $a_3$ represents the opportunity cost of using one dollar of the restrained budget within the list of projects in Sector 1 rather than in Sector 3. It is thus the "shadow price" of the scarce budget. Its relevance occurs within the maximization procedure because of the discreteness and unequal size of individual projects. Suppose, for example, that within an over-all budget limit of $500, program A (consisting of one group of projects) produces net gains of $100 for an expenditure of $500, while program B provides net gains of $90 for an expenditure of $400. Which is the preferred program? Evidently the answer depends upon the use to which the saved $100 might be put. In this case, if $a_3 = .1$, the two programs are identical in merit.[19]

The appropriate size of $a_3$ is fundamentally an empirical question to which I do not have the answer. It cannot be negative: if the internal rate of return (call it $r_3$) of projects in Sector 3 is less than $\pi$, the projects have no merit whatever. Beyond this, $a_3$ is probably relatively small; one would expect the strongly supramarginal projects to be in

[19] Why should the two programs be unequal—why is not program B expanded to exhaust the budget? It may not be possible because of the discreteness of the included projects to find an increment that will (a) increase the gains and not violate the restraints, budgetary or other, and (b) will have a marginal benefit greater than projects in Sector 3.

cluded in Sector 1. Evidently the size of $a_3$ is crucially affected by the ratio $r_3/\pi$ which in turn will depend upon how binding is the budgetary limit.

B. *The Relevance and Size of* $a_4$

This constant, while formally symmetrical with $a_3$, is both more important and has more conceptual difficulties. Its purpose is to permit comparison of a specific private project displaced with its (marginal) private alternative. Difficulties are of two sorts:

First, supposing there is a homogeneous class of private investment opportunities that are marginal as privately measured, we require an evaluation of these private investments discounted at the public rate, $\pi$. This causes difficulty because two projects whose present value is equal if evaluated at $r_4$ (as we will designate the internal rate of return of marginal private projects) may be unequal if evaluated at $\pi$. Indeed, project A may be superior to project B if evaluated at $r_4$ and inferior if evaluated at $\pi$, because of differences in the time periods over which benefits are received, or in the time profiles, or both. The practical solution would appear to require making some estimate of the average time paths and time durations for classes of marginal projects, although something more sophisticated than simple averaging is probably desirable [1].

The second difficulty is that marginal private investment opportunities are probably not homogeneous in view of the variety of organizations that we include in the private sector. For example, large private manufacturing firms require a higher rate of return than do privately owned public utilities, and municipal governments would doubtless apply still a third rate. At the limit this would involve the tedious task of identifying the specific marginal alternative to every displacement; more likely it will be possible to simplify by considering a few subclasses of alternatives to public expenditure and estimating separate values of $r_4$ (and thus $a_4$) for each.[20] In any case, given high marginal cut-off rates of return in at least some private sectors, the numerical value of $a_4$ may be substantially greater than zero, as reference to Table 1 will suggest.[21]

[20] In effect this means replacing Sectors 2 and 4 by several subsectors in the analysis. This causes no difficulty, but expands the number of terms in the *generalized* valuation metric. For a specific project with a specific private alternative the problem remains the same, but one must choose the "right" value of $a_4$. The algebraic extension of the argument to subsectors is omitted, but it is straightforward.

[21] To appreciate the real significance of this fact, recall that the constant occurs in the expression $(G_j - a_4 I_j)$ whose two terms are dimensionally equivalent. The higher the value of $a_4$, the less is the supramarginality of a given $G_j$. For example, the cost of displacing a steel mill (say by flooding its proposed site) is only the difference between the value of that site and some other site to which the steel investment will probably be directed. Obviously

### C. *The Relevance and Size of $a_2$*

The opportunity costs of transferring funds from the private to the public sectors will depend ultimately upon which groups in the private sector furnish the funds. Krutilla and Eckstein [5, Ch. 4] consider the effect of two types of tax reduction: the first, reducing income taxes in a manner most advantageous to low-income families, and also reducing sales taxes; the second, reducing income taxes with special emphasis on upper-income brackets, and also reducing corporate income taxes. In each case they find that the "appropriate" weighted average "interest rate" is between 5 and 6 per cent. This interest rate is analogous to the internal rates of return with which we have been dealing and may be designated $r_2$. Reference to Table 1 will show the magnitude of $a_2$ for various values of $T$ and $\pi$.[22]

How important the transfer of funds is depends upon the budgetary facts of life. Given an economy with a persistent tendency toward inflation, and given the large size of defense and related expenditures, the size of the public works budget would seem to be confined within rather narrow limits by over-all political and economic considerations. So far as budgets are rigid, no attention need be paid to the cost of transfer of funds. The term $a_2 m_{ij}$ is included in the metric chiefly to permit flexibility in its application. At present the $m_{ij}$ seem likely to be small; so far as they are, the magnitude of the constant $a_2$ loses importance.

## III. *An Application of the Model*

A simplified application may serve to clarify the model and to illuminate the role of the various opportunity costs in the special cases. Table 2 presents data for a public decision between two alternative public projects and the private alternative thereto.[23] In treating this example, we assume that investment funds are the limiting resource, that the

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if displaced projects are marginal they can be ignored; that they may well not be marginal is due to the governmental rights of pre-emption. But if supramarginal, only the supramarginality is significant. This fact has plagued the attempts to recognize displacement through limiting benefits to "alternative cost," and it has similarly given the use of secondary benefits (which are negative displacements) such a shady reputation.

[22] It should be noted that I am giving the transfer of funds a very different role than that assumed by Krutilla and Eckstein [5, Ch. 4]. They assume that the alternative to every (small) expenditure is a tax cut (of the appropriate form) of equal amount within a successful govennmental stabilization policy. Thus this computed interest rate is the *sole* opportunity cost they consider. In my view this not only neglects displacements but also neglects the fact that the real alternative may be another public project. To the extent that this is so, specific investment decisions do not imply transfers of funds from private to public sectors. It is, however, possible that *part* of the funds required will represent an addition to the federal budget, and for this part the type of weighed average interest rate that they compute is relevant.

TABLE 2—SIMPLIFIED CASE
(millions of dollars)

| Public Project S1 | Code $ij$ | $\pi = .025$ | | $\pi = .055$ | |
|---|---|---|---|---|---|
| | | Present Value of Net Benefits $G_{ij}$ | Investment $\bar{k}_{ij}$ | $G_{ij}$ | $\bar{k}_{ij}$ |
| High Dam | 11 | 1000 | 400 | 250 | 430 |
| 2 Lower Dams | 21 | 800 | 200 | 300 | 220 |
| Private Alternative S2 | $j$ | $G_j$ | $l_j$ | $G_j$ | $l_j$ |
| 3 Low Dams | 1 | 550 | 200 | 150 | 200 |

Private interest rate = 5%.

budget is at least $430 million, and that choice is limited to the specific alternatives listed. The problem is then simply to maximize.

$$\sum_{S1} x_{ij}y_{ij} \text{ subject to } x_{11},\ x_{21},\ x_1 = 0,\ 1 \text{ and } x_{11} + x_{21} + x_1 = 1.$$

We analyze three cases according to the source of the investment funds. Case 1 in which they come entirely from the existing public budget $(k_{ij} = \bar{k}_{ij})$; Case 2 in which they come entirely from the private sector $(m_{ij} = \bar{k}_{ij})$; and Case 3 in which half come from each source $(m_{ij} = k_{ij} = .5\bar{k}_{ij})$. Each case assumes a discount rate, $\pi$, equal in turn to .025 and .055.

Consider Case 1 for $\pi = .025$. We can immediately write:

$$y_{11} = 1000 - 400a_3 - (550 - 200a_4)$$
$$y_{21} = 800 - 200a_3 - (550 - 200a_4)$$

We will prefer project 11 to project 21 if $y_{11} > y_{21}$. We will prefer it to the private alternative if $y_{11} > 0$. These conditions and the condition that $a_3 \geq 0$ immediately yield the condition for choosing $x_{11} = 1$ as:

$$0 \leq a_3 < \min. \begin{Bmatrix} 1.0 \\ 1.12 + .5a_4 \end{Bmatrix}$$

[*] The data are based upon the Krutilla and Eckstein [5, Ch. 5] analysis of the Hells Canyon alternatives. Since a detailed explanation of the derivation of these rounded data from the underlying data requires more space than the example justifies, this had best be regarded as a quasihypothetical example. Annual benefits and costs are discounted at $\pi$ over 100 years in each case. An expanded development of this case is in mimeographed form.

TABLE 3—DECISION RULES FOR CHOICE OF PROJECT

| | Public: High Dam | Public: 2 Lower Dams | Private: 3 Low Dams |
|---|---|---|---|
| $\pi = .025$ Case 1 | $0 \leq a_3 \leq \min \begin{Bmatrix} 1.0 \\ 1.12 + .5a_4 \end{Bmatrix}$ | $1.0 < a_3 \leq 1.25 + a_4$ | $\max \begin{Bmatrix} 1.12 + .5a_4 \\ 1.25 + a_4 \end{Bmatrix} < a_3$ |
| Case 2 | $a_3 \leq \begin{Bmatrix} 1.0 \\ 1.12 + .5a_4 \end{Bmatrix}$ | $1.0 < a_3 \leq 1.25 + a_4$ | $\max \begin{Bmatrix} 1.12 + .5a_4 \\ 1.25 + a_4 \end{Bmatrix} < a_3$ |
| Case 3 | $0 \leq a_3 \leq \min \begin{Bmatrix} 2 - a_2 \\ 2.25 - a_2 + a_4 \end{Bmatrix}$ | $2 - a_2 < a_3 \leq 2.5 + 2a_4 - a_2$ | $\max \begin{Bmatrix} 2.25 - a_2 + a_4 \\ 2.50 - a_2 + 2a_4 \end{Bmatrix} < a_3$ |
| $\pi = .055$ Case 1 | — | $0 \leq a_3 \leq .75 + 1.1a_4$ | $\max \begin{Bmatrix} .75 + 1.1a_4 \\ .23 + .47a_4 \end{Bmatrix} < a_3$ |
| Case 2 | $a_3 \leq \min \begin{Bmatrix} -.24 \\ .23 + .47a_4 \end{Bmatrix}$ | $-.24 < a_3 \leq .75 + 1.1a_4$ | $\max \begin{Bmatrix} .75 + 1.1a_4 \\ .23 + .47a_4 \end{Bmatrix} < a_3$ |
| Case 3 | $0 \leq a_3 \leq \min \begin{Bmatrix} -.48 - a_2 \\ .47 + .93a_4 - a_2 \end{Bmatrix}$ | $-.48 - a_2 < a_3 \leq 1.37 + 1.82a_4 - a_2$ | $\max \begin{Bmatrix} .47 + .93a_4 - a_2 \\ 1.37 + 1.82a_4 - a_2 \end{Bmatrix} < a_3$ |

Similarly we will choose project 21 (i.e., let $x_{21} = 1$) if $y_{21} > y_{11}$ and $y_{21} > 0$, which yield the condition:

$$1.0 < a_3 < 1.25 + a_4.$$

Finally, we will choose the private alternative if both $y_{11}$ and $y_{22}$ are negative which yield the condition for $x_1 = 1$:

$$a_3 > \max. \begin{Bmatrix} 1.12 + .5a_4 \\ 1.25 + a_4 \end{Bmatrix}$$

For a specific value of $a_4$ (which depends, given $\pi$, on $r_4$, the marginal private rate of return) the decision rule becomes a simple function of $a_3$. E.g., for $r_4 = .05$, $a_4 = .85$, the rule is (we arbitrarily assign the borderline cases):

$$\begin{aligned} \text{choose } x_{11} &= 1 \quad \textit{if} \quad 0 \leq a_3 \leq 1.0 \\ x_{21} &= 1 \quad \textit{if} \quad 1.0 < a_3 \leq 2.1 \\ x_1 &= 1 \quad \textit{if} \quad 2.1 < a_3 \end{aligned}$$

Analysis of the other cases is equally straightforward. Table 3 summarizes the decision rules in terms of the values of the $a$'s. Table 4 gives them for the assumed values $r_4 = .05$ and $r_2 = .055$.[24]

[24] These values appear to be the ones Krutilla and Eckstein imply. Since they make the assumptions of our Case 2, the decision appears to be a unique function of the rate of discount, and since they further argue that the correct rate of discount is $\pi = r_3$, their solution is to have the public build the two lower dams.

TABLE 4—DECISION RULES IF $r_4 = .05$, $r_2 = .055$

| | Public: High Dam | Public: 2 Lower Dams | Private: 3 Low Dams |
|---|---|---|---|
| $\pi = .025 \begin{Bmatrix} a_4 = .85 \\ a_2 = .93 \end{Bmatrix}$ Case 1 | $0 \leq a_3 \leq 1.0$ | $1.0 \leq a_3 \leq 2.1$ | $2.1 < a_3$ |
| Case 2 | all $a_3$ | — | — |
| Case 3 | $0 \leq a_3 \leq 1.07$ | $1.07 < a_3 \leq 3.27$ | $3.27 < a_3$ |
| $\pi = .055 \begin{Bmatrix} a_4 = -.09 \\ a_2 = 0 \end{Bmatrix}$ Case 1 | — | $0 \leq a_3 \leq .65$ | $.65 < a_3$ |
| Case 2 | — | all $a_3$ | — |
| Case 3 | — | $0 \leq a_3 \leq 1.99$ | $1.99 < a_3$ |

The main point worth noting is that the decision in general does not rest solely upon the value of the public discount rate ($\pi$), since the value of $\pi$ affects not only the benefits but also the several values of the $a$'s.

This illustration is of course extraordinarily oversimplified in that it involves a choice between only two alternatives, each of which has the same private alternative, in that the budget restraint is not binding, and in other ways. More complicated problems can be most effectively handled by machines.

## IV. *Methods of Solution*

Choosing the levels of the choice variables (the $x_{ij}$) in such a way as to maximize equation (2) subject to the restraints can, in principle, be done in at least two ways. The most general is as a problem in linear programming; indeed, since the $x_{ij}$ are all either zero or one, it is the simplest form of linear programming problem—the so-called transportation problem.[25] In essence, the standard (simplex) method of solution is iterative, proceeding from an arbitrary feasible solution to an optimal one.

An alternative algorithmic procedure based upon enumeration of feasible combinations is available and may prove less expensive for

[25] There are a number of expositions of the method. See, for one, [2, Ch. 5]. Markowitz and Manne [8] developed a technique for solution that is appropriate for discrete values of the choice variables even if not restricted to zero and one. More recent (unpublished) work by R. E. Gomory ("An Algorithm for Integer Solutions to Linear Programs," Princeton-I.B.M. Math. Res. Project Tech. Rept. No. 1, Nov. 17, 1958) has developed a solution to the discrete programming problem that is better suited to machine computation.

problems of moderate size. Because of the particularly simple form of the objective function and of the restraints, this procedure is easily adapted to machine computation. The procedure is as follows:

*Step* 1: Identify the combinations that are feasible with respect to the incompatibility restraints.

Consider a case in which there are $N$ projects identified. $N$ is by definition the number of $_{ij}$'s identified, and cannot be greater than $i^*$ times $j^*$ (where * indicates the largest value assigned), and is most likely smaller, because not all facilities can be used for all purposes; e.g., a dam site below a city cannot be used to provide flood control protection to the city. Since the problem is to assign each such project a level of either zero or one, there are $2^N$ total combinations of projects —a truly fearsome number for even moderate sizes of $N$ (e.g., if $N = 30$, $2^N$ exceeds 1 billion). Fortunately, not all must be considered, for any program containing more than the smaller of $i^*$ or $j^*$ projects will necessarily violate one of the incompatibility restraints on the $x_{ij}$. Thus, letting $n = \min(i^*, j^*)$ it is necessary to consider only

$$\sum_{\alpha=0}^{n} \binom{N}{\alpha} \text{ combinations,}$$

which is a very much smaller number (e.g., if $N = 30, n = 4$, the number is about 32,000; if $N = 30, n = 5$, the number is about 174,000).[26]

Many of these combinations violate the incompatibility restraints ($c$) and ($d$) which in effect require that programs be taken from an $(i^* + 1) \times j^*$ matrix in such a way that not more than one project be included from any row or column. Those programs that do not violate this condition become a set of addresses for future reference.

*Step* 2: Compute for each feasible combination identified in Step 1 the $\sum_{ij} x_{ij}k_{ij}$ and eliminate all that violate the budget restraint.

Since the combinations remaining after the first step consist of a

[26] This maximum number of combinations that the machine must consider in the first step of the analysis can be evaluated directly using binomial tables, or can be approximated for all but very small values of $N$ using areas under the normal curve:

$$\sum_{\alpha=0}^{n} \binom{N}{\alpha} = 2^N \sum_{\alpha=0}^{n} \binom{N}{\alpha} \left(\frac{1}{2}\right)^N \approx 2^N \int_{-\infty}^{t} g(t)dt$$

where $g(t)$ is a normal curve of zero mean and unit standard deviation, and $t$, the normal deviate.

$$t = \frac{2n + 1 - N}{\sqrt{N}}$$

series of programs having some elements with $x_{ij} = 1$, and the rest equal to zero, all that is required is summation of the $k_{ij}$ of the nonzero components of each program. Those programs that do not violate this restraint are retained as addresses for the third step.

*Step* 3: For the addresses identified in Step 2, the $\sum_{ij} x_{ij} y_{ij}$ is computed, and the program with the largest value is the best of the feasible programs.

Once again the computation involved is a simple sum of the values of $y_{ij}$ for each of the projects with nonzero values of $x_{ij}$ in each remaining program.[27] Instead of simply choosing the maximum, it is of course possible to rank programs in decreasing order and thus leave the way open to evaluating the differential merit of alternative programs and permitting the introduction of such other (nonefficiency) considerations as may be desired. (For example, the second-best program may be very nearly as good as the best and yet provide a politically better geographical distribution of expenditures.)

The choice between the alternative methods of solution will depend upon the relative cost of computation. Either is in principle capable of solving the problem in a straightforward manner.

## V. *Conclusion*

The basic equation (3) of the present model,

$$y_{ij} = (G_{ij} - a_3 k_{ij}) - (G_j - a_4 l_j) - a_5 m_{ij}$$

represents a *general* form of a sensible objective function. It encompasses therefore a variety of special cases which rest implicitly upon certain limiting assumptions. A number of these special cases have been advocated as the correct solution; the present section attempts to place them in context. Special cases can be generated in a number of ways by choice of limiting assumptions concerning the sources of funds, the existence of supramarginal private alternatives, and the level of employment.

[27] The three-step procedure here suggested is designed to economize computational time. In principle it would be possible to combine the last two, or even all three steps. For example, it would be possible to compute both $\Sigma x_{ij} k_{ij}$ and $\Sigma x_{ij} y_{ij}$ for all the addresses found in step one, thus combining steps two and three. But to do so would be to calculate the $\Sigma x_{ij} y_{ij}$ for many programs which are of no interest since they violate the budget restraint, and therefore to multiply the number of machine operations required.

An alternative procedure is available if it is desired to leave the budget limit unspecified until the last step.

If federal budgets are regarded as fully flexible, then all marginal funds come from the private sector and the term $a_3k_{ij}$ vanishes. If private alternatives either do not exist or are merely marginal, the term $(G_j - a_4l_j)$ also vanishes, and all that remains is $G_{ij} - a_2m_{ij}$.[28]

Given the heavy defense and related demands upon the federal budget, given full employment of resources and heavy taxes, a different view is that public works budgets are virtually rigid in which case the term $a_2m_{ij}$ vanishes. Given also supramarginal private alternatives, this suggests that the relevant terms are $(G_{ij} - a_3k_{ij}) - (G_j - a_4l_j)$. These are reasonably realistic assumptions for the bulk of the postwar period.

In an economy characterized by heavy underutilization of resources, one might expect few or no marginally attractive investment opportunities, and the value of $a_4$ would become zero (since $r_4 = 0$). Similarly, one might visualize a mechanism for transferring funds from private to public sectors that tapped idle resources; thus the value of $a_2$ would approach zero, and a larger public works program would be justifiable on efficiency as well as on stability grounds, since some of the indirect opportunity costs would vanish.

That the nature of the choices made will vary according to the general budgetary and economic situation is of course not an exclusive feature of the present model—transfer costs or internal rates of return also reflect these considerations. The chief advantages of the model are: (1) its flexibility in allowing for changes in the relevance and magnitude of the *different* classes of opportunity costs, (2) its flexibility in defining the appropriate restraint or restraints, (3) its handling of the displacement problem, and (4) its ability to handle choice among incompatible alternatives.[29]

## Mathematical Appendix

The purpose of this appendix is to show the derivation of equation (2) (p. 900 above), which maximizes over Sector 1 only, from equation (1), which concerns the maximum over all sectors, and to make clear the role played by the particular assumptions and definitions.

[28] If, in addition, one follows [5] and regards the appropriate $\pi = r_2$, $a_2$ is zero and the criterion is simply to maximize $G_{ij}$ evaluated at this "transfer-cost" interest rate.

[29] With respect to (1) I believe that most of the case studies of 1950-58 made in [5] and [6] require re-evaluation in terms of the assumptions appropriate to the period. With respect to (2), both [3] and [5] assume that the appropriate restraint is the present value of total cost; [6] assumes capital cost is the scarce resource. With respect to (3) the problem is usually assumed away by supposing all private alternatives are marginal, or by assuming $a_4 = 0$. With respect to (4), it is standard practice to ignore incompatibilities—apparently the assumption is made that choice is limited to mutually compatible alternatives. But this ducks a major part of the problem: choosing the efficient compatible sets.

*Definitions*—Let:

Sectors, $S1$, $S2$, $S3$, $S4$ be defined as in text;

$p$ be a general running subscript ($p=ij$ in $S1$; $p=j$ in $S2$);
$\pi$ be a discount rate;
$G_p$ be the present value of the $p$th project over capital and operating costs, evaluated using $\pi$;
$x_p$ be the level of the $p$th project;
$\bar{k}_{ij}$ be cost in units of the scarce budget, $\bar{k}_{ij}=k_{ij}+m_{ij}$, where $k_{ij}$ is the drain on the existing budget, $m_{ij}$ is the net increase in the budget which adoption of the $ij$th project will cause; $m_{ij}$ represents the amount of funds transferred from the private sector (by taxation or borrowing);
$K$ be the existing budget restraint for $S1$ and $S3$;
$l_p$ be the capital cost of the $p$th private project;
$A$ be the amount of private investment if no funds transferred;

$\alpha \sum_{S1} x_{ij}m_{ij}$ be the change in private investment owing to the transfer of $\sum_{S1} x_{ij}m_{ij}$ dollars from private to public sectors;

$$\left.\begin{aligned} a_3 &= \frac{G_p}{k_p} \text{ for } S3 \\ a_4 &= \frac{G_p}{l_p} \text{ for } S4 \end{aligned}\right\} \text{ assumed constant, } \qquad a_3 \geq 0;$$

$$a_2 = \alpha a_4.$$

*Objective function:*

$$N = \sum_{S1} x_{ij}G_{ij} + \sum_{S2} x_jG_j + \sum_{S3} x_pG_p + \sum_{S4} x_pG_p. \tag{1}$$

*Restraints:*

$$\sum_{S1} x_{ij}k_{ij} \leq K \text{ (budget restraint).}$$

$$x_p = 0,\ 1, \text{ all } p \text{ (discreteness restraint).}$$

$$\sum_i x_{ij} + x_j = 1 \text{ (incompatibility of } j\text{'s).}$$

$$\sum_j x_{ij} \leq 1 \text{ (incompatibility of } i\text{'s).}$$

Since the direct choice variables are the $x_{ij}$, it is convenient to reformulate the objective function in a form relying only on these variables.

From the definitions, we note that:

(i) $$\sum_{S1} x_{ij}k_{ij} + \sum_{S3} x_p k_p = K;$$

(ii) $$\sum_{S2} x_j l_j + \sum_{S4} x_p l_p = A - \alpha \sum_{S1} x_{ij} m_{ij};$$

(iii) $$G_p = a_3 k_p, \text{ for } S3;$$

(iv) $$G_p = a_4 l_p, \text{ for } S4.$$

Thus, we can write:

$$\sum_{S3} x_p G_p = a_3 \sum_{S3} x_p k_p = a_3 \left(K - \sum_{S1} x_{ij} k_{ij}\right),$$

$$\sum_{S4} x_p G_p = a_4 \sum_{S4} x_p l_p = a_4 \left(A - \alpha \sum_{S1} x_{ij} m_{ij} - \sum_{S2} x_j l_j\right).$$

Substituting, in eq. (1):

$$N = \sum_{S1} x_{ij} G_{ij} + a_3 K - a_3 \sum_{S1} x_{ij} k_{ij} + \sum_{S2} x_j G_j + a_4 A - a_4 \alpha \sum_{S1} x_{ij} m_{ij} - a_4 \sum_{S2} x_j l_j.$$

(1.1) $$N = \sum_{S1} (G_{ij} - a_3 k_{ij} - a_2 m_{ij}) x_{ij} + \sum_{S2} (G_j - a_4 l_j) x_j + a_3 K + a_4 A.$$

Let $w_{ij} = G_{ij} - a_3 k_{ij} - a_2 m_{ij}$,

$w_j = G_j - a_4 l_j$,

$y_{ij} = w_{ij} - w_j$.

We can rewrite eq. (1.1):

$$N = \sum_{S1} w_{ij} x_{ij} + \sum_{S2} w_j x_j + a_3 K + a_4 A.$$

From the restraints we know that $x_j = 1 - \sum_i x_{ij}$, and thus:

$$N = \sum_{S1} w_{ij} x_{ij} + \sum_{S2} w_j - \sum_{S2} w_j \sum_i x_{ij} + a_3 K + a_4 A$$

$$= \sum_{S1} (w_{ij} - w_j) x_{ij} + \sum_{S2} w_j + a_3 K + a_4 A.$$

Since the last three terms are constant, they will not affect the maximum, and we can maximize instead:

(2) $$Z = \sum_{S1} y_{ij} x_{ij}$$

where, to expand,

(3) $$y_{ij} = (G_{ij} - a_3 k_{ij}) - (G_j - a_4 l_j) - a_2 m_{ij}.$$

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# RELATIVE INCOME SHARES IN FACT AND THEORY

*By* IRVING B. KRAVIS*

Despite the prominence of functional income distribution in the history of economic thought and the recent evidences of a revival of interest in the subject,[1] the theory of distribution remains in a parlous state. This is particularly true with respect to the aid it can give in explaining historical changes in relative shares. Part of the reason for the lack of more progress has been the tendency to deal with the problem in purely deductive terms; and when empirical factors have been taken into account, there has been a tendency to explain away evidences of a rising labor share and to regard division of income between labor and capital as having remained constant.[2]

Our examination of U. S. data for the past half century has led to the conclusion that the notion of long-run constancy in relative shares is false—at least for this country and period. There has actually been a shift in the distribution of national income from property to labor, and it is this which has to be explained. The size of the shift depends upon the particular assumptions used in allocating entrepreneurial income between its property and labor components, but there appears to be little doubt about the existence of the movement. Indeed, when the various biases in the statistical and accounting framework are taken into account, the net effect is to strengthen the impression of a notable change in relative shares. Labor's gain in terms of personal income is, as would be expected, much greater than in terms of national income.

The main purpose of this paper is to set forth the evidence supporting these conclusions. The more interesting and challenging task of explain-

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[1] On the theoretical side, the most important single work has been that of S. Weintraub [37], which attempts to break new ground and which contains references to the recent theoretical literature. For citations of the recent empirical work see R. M. Solow [24].

[2] The oversimplification involved in this view has been stressed in an international comparison of income shares by Simon Kuznets [20] which came to hand after this paper was completed.

ing them requires a fuller understanding of the processes of economic change than economists possess at present. Some of the elements in the story can be identified but we can only speculate on their precise role. One important factor has been the change in the relative supplies of labor and capital. The number of man-hours worked has not expanded as fast as population despite the great rise in real hourly earnings, while reproducible capital (in constant prices) has nearly doubled in relation to man-hours. The greater responsiveness of the supply of capital to the demands of a growing economy has led to price-induced substitution with existing techniques and probably also to capital-using innovations. The opportunity for factor substitution in this historical rather than in a static sense has been of great practical importance as a built-in stabilizer of relative shares. Aside from relative factor supplies, shifts in the structure of industry, particularly those involving a diminution in the relative importance of agriculture, have probably operated to favor the labor share. Rising prices, by adversely affecting the incomes of rentiers, may also have contributed to the increase in the relative share of labor.

## I. *The Empirical Evidence on Income Shares*

The study of the trends in the functional distribution of income is handicapped by the fact—often commented upon [19, pp. 81-90] [20, p. 25]—that the nature of the components of income for which we have data has not been determined by the requirements of the economists but by the legal and institutional arrangements of our society. The greatest differences between the classical concepts of factor shares and the concepts used in national accounting relate to rent and profit. The "rent" of national accounting is the "rental income of persons" and does not represent a scarcity return either on the indestructible resources of nature or on specific factors temporarily fixed in quantity. It does not even include the net income on all leased property, but only the portion thereof received by persons; the net income on real estate owned by businesses is not counted as rent but as part of corporate profits or unincorporated income. Corporate profits also include explicit or imputed interest received by firms, and the income of unincorporated enterprises contains not only rent and interest but also the return for the labor of proprietors.

### A. *A Threefold Division of Income*

Given this accounting framework, a threefold division of income into employee compensation, entrepreneurial (unincorporated) income, and property income (rent, interest and corporate profit) is perhaps most relevant to the study of functional shares. In these terms, we can see

TABLE 1—DISTRIBUTIVE SHARES IN NATIONAL INCOME, 1900–57

(Averages of percentage shares for individual years)

| Period | Distributive Shares | | | | | | Property Share, Various Concepts | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | Interest, Rent and Corporate Profits | Total Property Share | | | |
| | Employee Compensation (1) | Entrepreneurial Income (2) | Corporate Profits (3) | Interest (4) | Rent (5) | Total (6) | (7) | Asset Basis (8) | Labor Basis (9) | Proportional Basis (10) | Economy-wide Basis (11) |
| 1900–09 | 55.0 | 23.6 | 6.8 | 5.5 | 9.1 | 100 | 21.4 | 36.8 | 23.0 | 30.6 | 28.0 |
| 1905–14 | 55.2 | 22.9 | 6.9 | 5.8 | 9.1 | 100 | 21.8 | 38.0 | 24.4 | 30.7 | 28.3 |
| 1910–19 | 53.2 | 24.2 | 9.7 | 5.2 | 7.7 | 100 | 22.6 | 38.0 | 30.6 | 31.9 | 29.8 |
| 1915–24 | 57.2 | 21.0 | 8.9 | 5.3 | 7.6 | 100 | 21.8 | 34.6 | 23.9 | 29.8 | 27.6 |
| 1920–29 | 60.5 | 17.6 | 8.2 | 6.2 | 7.6 | 100 | 22.0 | 32.3 | 21.9 | 28.4 | 28.6 |
| 1925–34 | 63.0 | 15.8 | 6.4 | 8.1 | 6.6 | 100 | 21.1 | 27.9 | 17.2 | 26.8 | 25.1 |
| 1930–39 | 66.8 | 15.0 | 4.9 | 8.2 | 5.0 | 100 | 18.1 | 23.9 | 12.7 | 23.4 | 21.3 |
| | | | | | | | | | | | |
| 1929–38 | 66.6 | 15.5 | 4.3 | 8.9 | 4.6 | 100 | 17.8 | 23.5 | 11.2 | 23.2 | 21.1 |
| 1934–43 | 65.1 | 16.5 | 9.1 | 6.0 | 3.3 | 100 | 18.4 | 24.3 | 17.0 | 24.2 | 22.0 |
| 1939–48 | 64.6 | 17.2 | 11.9 | 3.1 | 3.3 | 100 | 18.3 | 25.8 | 21.5 | 24.3 | 22.1 |
| 1944–53 | 65.6 | 16.4 | 12.6 | 2.1 | 3.4 | 100 | 18.1 | 24.7 | 21.5 | 23.8 | 21.6 |
| 1949–57 | 67.1 | 13.9 | 12.8 | 2.7 | 3.4 | 100 | 18.9 | 26.0 | 20.2 | 23.8 | 22.0 |

*Sources:* 1900–09 to 1930–39: D. G. Johnson [11, p. 178]. Johnson's estimates were based on data of the Commerce Department [31], S. Kuznets [19], W. I. King [14] and R. F. Martin [21]. 1929–39 to 1949–57: derived from Commerce Department data [34].

from the average shares for overlapping decades presented in Table 1 that employee compensation rose from 55 per cent of national income at the beginning of the century to 67 per cent in the 1930's and remained at approximately that level in the ensuing decades. The bulk of the increase in the share of employee compensation in its period of expansion (between 1900-09 and 1930-39)[8] came at the expense of entrepreneurial income which declined from 24 to 15 per cent, but it should not be inferred that managerial employees were the main beneficiaries of the rise in employee compensation. On the contrary, the evidence we have indicates that there was only a small gain in the percentage share of corporate officers during the period when employee compensation rose most, and there has actually been a diminution in the executive share in recent decades (see Table 2).

TABLE 2—SHARE IN NATIONAL INCOME OF COMPENSATION OF EMPLOYEES
(per cent)

| | All Employees | Corporate Officers | Other Employees |
|---|---|---|---|
| 1919–24 | 60.3 | 3.6 | 56.7 |
| 1928–37 | 65.4 | 4.3 | 61.1 |
| 1929–38 | 66.6 | 4.3 | 62.3 |
| 1934–43 | 65.1 | 3.6 | 61.5 |
| 1939–48 | 64.6 | 2.9 | 61.7 |
| 1944–53 | 65.6 | 2.8 | 62.8 |

*Sources:* As in Table 1 except for shares of corporate officers in 1919–24 and 1928–37 which are based on S. Kuznets [19, pp. 216, 853].

Perhaps in view of the perquisites of corporate officers that escape taxation and would be difficult to evaluate for income accounting purposes even if all the facts were known, these figures must be seasoned with several grains of salt. However, the understatement in recent years compared with earlier ones would have to be substantial to offset the impression of a declining relative share. Even, for example, if we add 50 per cent to the average income of corporate officers for the 1944-53 decade, the share of corporate officers in national income does not quite come up to their 1929-38 share. It is still possible, however, that the in-

[8] For this and certain other series, the 'thirties represent a dividing decade with one type of behavior characteristic of the period before and another of the period after. Since the underlying sources are also different before and after the 'thirties, this raises the question whether the variation in behavior results merely from differences in statistical treatment. Fortunately we are able to present figures from both the earlier (largely Kuznets) and later (Department of Commerce) sources for the 'thirties, and the close similarity in the two sets of figures offers some reassurance about the comparability of the two sets of data. Some of the conceptual differences, which have become quantitatively significant in the 'forties and 'fifties, are discussed in the text subsequently.

come share of corporate managerial employees below the officer category has been rising.

It seems likely that the upward trend in wages or total employee compensation and the decline in entrepreneurial income have their beginnings back in the 19th century. E. C. Budd has estimated that the share of wages in private income rose from about 43 per cent in 1869-70 to around 48 per cent in 1909-10 [1].

The long-run rise in the employee share and decline in the share of business income are closely related to two other important and interrelated trends in the nation's economic structure—the change from an agricultural to an industrial country and the shift out of self-employment into wage and salary employment. The proportion of persons engaged in agriculture declined from more than half of the work force in 1870 to less than 40 per cent at the turn of the century, to 10 per cent and less in the 1950's, while the proportion of employees rose from 58 per cent in 1870 to 64 per cent at the turn of the century and to around 85 per cent in the 1950's.[4]

The share of property income (interest, rent and corporate profit) hovered around a level of 21 or 22 per cent from 1900-09 to 1925-34, and around 18 or 19 per cent since 1930-39. This relative stability of the property share has, however, been the product of offsetting shifts in its components, particularly interest and corporate profits. In eight out of the ten decade-to-decade changes in Table 1, the interest and corporate profits shares move in opposite directions, describing about one and one-half long cycles during the half century or more. There was a peak in the profits share and a trough in the interest share in the prosperous first world war decade, a profits trough and interest peak in the depressed 'thirties, and high profits and low interest shares in the prosperous 'forties and 'fifties. This seesaw relationship between a return based upon a fixed claim and one representing a residual share contrasts with the decline in the rent share from 9.1 per cent in 1900-09 to around 3⅓ per cent in 1934-43 and the ensuing periods. In a sense, this reduction in the rent share plays an important role in producing the relative stability of the property share. Compared to most former decades, the recent levels of the profit share tend to be higher by a larger number of percentage points than the interest share is lower, and, were it not for the fact that the rent share has only been one-half or one-third of its former levels, the property share would have risen.

A large part of the rent share—over 61 per cent in 1950[5]—represents

[4] 1870 and 1900 based on Budd [1, App. A]; 1950's based on Commerce data [33, Tables 25 and 28].

[5] [32, p. 86]. Rented nonfarm dwellings accounted for 21 per cent of net rental income and owner-occupied nonfarm dwelling for 40 per cent. Farm realty, including both residences and farming property, was responsible for 13 per cent.

net rental income from residential property, and it might be thought that the decline in the rent share could be traced to a reduction in the relative importance of housing services in total consumption as the level of aggregate consumption per capita has increased. Indeed, it is possible that the relative importance of buildings in general might have diminished in an economy expanding in per capita income and wealth.

If we approach this question from the consumption side, we find conflicting evidence concerning the trend in the proportion of housing "rent" to total consumption. While Kuznets' data indicate that housing remained around 14 per cent of total consumption from 1899-1908 to 1929-38, Dewhurst's estimates suggest a decline of 36 per cent in the housing share in consumption during a period in which the rent share in income dropped by 64 per cent.[6] Evidence from survey sources, although far from satisfactory, tends to support the Kuznets rather than the Dewhurst findings on this point.

Nor did the relative importance of residential real estate in income-producing assets diminish during the period when the rent share declined so precipitously. If anything, the share of nonfarm residences in total private tangible assets (other than consumer durables) was slightly higher after 1929 than before, but this was offset or more than offset by a fairly pronounced decline in the share of other structures [8, p. 306].

There is some evidence, however, that rent-yielding assets (land and buildings) did diminish in relative importance in the investment holdings of individuals. Goldsmith's balance sheets of national wealth indicate that the share of such assets in the total earning assets held by households diminished from 45 per cent in 1900 to 40 per cent in 1949.[7] The implication is that there was a trend toward the replacement of amateur real estate lessors (including owners occupying their own dwellings) by lessors making a regular business of owning and leasing real estate. This would, of course, cause a shift in the reporting of the income yielded by real property from the category of rent to the category of business income. However, the decline in the relative importance of rent-yielding assets held by households is not great enough to account

[6] The decline in the income share from 1905-14 to 1934-43 is calculated from Table 1. The decline in the consumption share is calculated by taking Dewhurst's estimate of 19.1 per cent for 1909 as representative of the decade 1905-14 and extrapolating Dewhurst's 1929 figure to 1934-43 on the basis of Commerce Department data. The consumption data and their sources are cited in Kravis [16, pp. 340, 342].

[7] Based on R. Goldsmith [7, pp. 42-43, 56-57]. All the tangible and intangible assets listed by Goldsmith except consumer durables, currency, U. S. government securities, commercial bank deposits and "other intangible assets" were considered as earning assets for this purpose.

for more than a small part of the contraction in the rent share. On the other hand, it is not beyond the realm of possibility that the trend toward business ownership and leasing of property may be reflected more fully in the income estimates than in the wealth data.[8] The difference would have to be substantial to help us very much, and, on the whole, it seems unlikely that changed institutional arrangements relating to real property ownership and management are primarily responsible for the diminished importance of rents in national income. The role of more purely economic factors—viz., supply and demand relationships—will be discussed subsequently in connection with the more general question of the change in the property share as a whole.[9]

### B. *The Division of Income between Property and Labor*

Although the threefold division into labor, entrepreneurial and property shares is as far as the usual accounting records of the economy can carry us, it is necessary to attempt to divide entrepreneurial income into its labor and property components if we are to probe some questions that arise: Has the increase in the share of labor been attributable mainly to the shift from self-employment in the proprietorship form to employment under the corporate form of business organization? What has happened to the share of income representing returns to the current efforts of persons engaged in economic activity (i.e., what we shall call the "labor" share) as compared to the share representing the return on past accumulations of wealth (i.e., what we shall call the "property" share)?

Of course, if all or virtually all of entrepreneurial income could be considered as a reward for the labor of the entrepreneur, the answer to these questions would be very simple. In that case, our findings would be that the relative shares of labor and property in the national income have remained almost constant for more than half a century with only a slight shift in favor of labor in the years around 1930.

The difficulty with this view is that it implicitly assumes that the returns upon the assets of unincorporated enterprises have been zero or negligible. These assets form a substantial, albeit declining, share of total private wealth. In 1956, unincorporated businesses and farms accounted for 23 per cent of the total tangible assets of the private sector (excluding consumer durable goods); in 1900, they accounted

[8] Goldsmith's wealth balance sheets, for example, do not show any ownership of residential property by unincorporated businesses.

[9] The rent figures probably are the least reliable of the various share estimates [32, p. 86]. However, there is no obvious reason for believing that a downward bias of increasing severity has crept into the estimates; and in view of the size of the reduction it seems improbable that the change could be explained on this basis.

for 41 per cent.[10] Therefore, the assumption about the rate of return earned by these assets is critical to an evaluation of the property share in entrepreneurial income.

One possible assumption is that the rate of yield upon entrepreneurial property has been the same as that upon other property. The entrepreneurial property share in national income can then be calculated by applying to the share of other property (i.e., the combined share of interest, rent and corporate profits) the ratio of the value of entrepreneurial to other property. When the entrepreneurial property shares calculated in this fashion are added to the other property shares (interest, rent, and corporate profits) to obtain what may be called the total property share the results are as shown in column 8 of Table 1. This will be referred to as the "asset basis" for estimating the property share in entrepreneurial income.

The asset basis regards the return to entrepreneurial labor as the residual component of entrepreneurial income. It seems just as logical —and has the sanction of at least occasional practice—to calculate the labor component directly and to regard the property portion as the residual return. This can be done by assuming that the annual value of the labor of a proprietor was equal to the annual earnings of a hired worker. The figures produced by this approach, the "labor basis" for splitting up entrepreneurial income, are presented in column 9 of Table 1.[11]

The difficulty with both the asset and labor bases is that they tend to concentrate the effects of fluctuations in the entrepreneurial share upon one or the other component. It may be more realistic to argue that when the entrepreneur commits his labor and capital to an enterprise, he is taking the risk that he will get much less than the market rates of return on both the labor and capital in the hope, of course, that he will get much more. Since the two types of input are jointly committed,

[10] The percentages were calculated from R. Goldsmith's national balance sheets [7, pp. 42-56]. The percentages for the years for which Goldsmith presented balance sheets are as follows:

| | | | | | |
|---|---|---|---|---|---|
| 1900 | 41 | 1929 | 28 | 1945 | 29 |
| 1912 | 43 | 1933 | 24 | 1949 | 27 |
| 1922 | 35 | 1939 | 24 | 1956 | 23 |

The 1956 figure is based on data kindly supplied by Goldsmith.

[11] Since the labor earnings of proprietors were assumed to be the same as employees, the share of national income accounted for by proprietors' labor earnings could be obtained by multiplying the wage and salary share by the ratio of proprietors to employees. The numbers of proprietors and employees for 1929-57 were obtained from Commerce data [33], for 1919-29 from Kuznets [19, pp. 334, 340], for 1909-19 from Creamer [3, pp. 129-30], and for 1900 and 1910 from Budd [1, App. A]. For 1909-19 the number of proprietors was extrapolated from Kuznets 1919 figures [19, p. 340] by an index based on estimates by King [14, p. 62].

there is a case for allowing both to share in the ups and downs of entrepreneurial income. This suggests that entrepreneurial income be divided into the labor and property components in constant proportions. Using a rough generalization of estimates of the labor and property shares in entrepreneurial income prepared by D. G. Johnson [11, p. 726], we have taken 65 per cent as labor and 35 per cent as property income.[12] The total property shares on the basis of these proportions are given in column 10 of Table 1.

Any such constant division of entrepreneurial income between property and labor is subject to the objection that it ignores the forces at work in the economy shifting the balance between labor and property incomes—the very tendencies we are studying. An allocation basis which meets this criticism and still avoids concentrating the effects of the vicissitudes of entrepreneurial fortune on one share or the other is to divide the income of noncorporate business for each period in accordance with the current relationship between labor and property income in the entire economy excluding the entrepreneurial sector. We have labeled this the "economy-wide" basis (see column 11 of Table 1).

The addition of entrepreneurial property returns to the other property shares produces total property shares that are substantially smaller in recent decades than at the beginning of the century except when the labor basis is used, in which case the decline is modest. On all four approaches there was a sharp drop in the total property share between 1910-19 and 1930-39 when the share of entrepreneurial income fell from 24 to 15 per cent. The decline was the most precipitous in terms of the labor basis, since this method has the effect of assigning to labor almost the whole of entrepreneurial income during the decades affected by the great depression when business earnings were low. With economic recovery, however, the labor basis results in a sharp rise in

[12] Johnson calculated the capital component of farm operators' income by applying current rents to farm land, and current interest rates to the value of farm inventories [10, p. 726]. He divided the income of nonfarm proprietorships on the basis of (1) the relative importance of property and labor in income originating in corporate manufacturing and (2) the relationship between wages paid by noncorporate enterprises and the net income of such businesses [11, pp. 176-77]. Johnson's reasoning with respect to nonfarm enterprises began with the idea that the property share which was 25 per cent of corporate income, was less, perhaps only 20 or 15 per cent, of noncorporate income. Since virtually all property income in unincorporated enterprises is part of proprietors' income and since in proprietorships wages and entrepreneurial income have been about equal, a 20 per cent property share for the enterprise means a 40 per cent property share in entrepreneurial income. Johnson felt warranted in taking 20 per cent as the property share for the enterprise, but he settled on a 35 rather than 40 per cent property share in entrepreneurial income in order to allow for the fact that some property used by entrepreneurs is not owned by them. Cf. also G. J. Schuller [23] whose estimates with respect to the broad division between labor, entrepreneurial and property shares suggest conclusions similar to those reached above.

the total property share, whereas the other approaches show only very slight or no increases.

Our methods in connection with all four techniques are, of course, susceptible of substantial refinement, and even with the same basic method many alternative assumptions with respect to the returns to labor or capital are possible. In particular, it would be preferable to value the services of labor and capital in accordance with the returns prevailing in each sector of the economy rather than in the economy as a whole. For example, the labor performed by entrepreneurs in agriculture and trade, who even in recent years have accounted for about two-thirds of all proprietors, should be valued at annual earnings of employees in those industries rather than (as we have done) at annual earnings of employees in the economy as a whole. Similarly, in place of the economy-wide approach, it would have been desirable to allocate entrepreneurial income in each sector between property and labor on the basis of the division in the corporate portion of the sector.

While our methods of estimation have influenced the results, the share figures are in an even more fundamental sense the product of the particular accounting framework under which they were produced. While there is on the whole widespread agreement upon the methods of social accounting, some issues which may affect the income share estimates remain controversial.[13] In addition, even some of the conventional procedures that are followed may have won agreement more on the ground of statistical convenience than conceptual adequacy. We are faced with the question, therefore, whether the trend in shares that we have observed—particularly the decline in the property share—would persist if the issues concerning accounting methods had been resolved in another way. We will consider the effect upon shares of (1) the shift of certain activities from households to the market, (2) the omission from the national accounts of the returns on certain types of property (viz., property owned by government and durable goods other than residences owned by consumers), (3) the exclusion from the estimates of interest on government debt and the inclusion of the compensation of government employees, (4) the Department of Commerce practice of using historical rather than replacement-cost depreciation, and (5) the effect of changes in tax laws and regulations governing depreciation allowances.

1. The effect of the monetization of household activities is difficult to quantify. However, its possible impact is somewhat offset by the fact that not all of household production can be taken as representing serv-

[13] For a recent discussion see *Studies in Income and Wealth*, Vol. 22, Princeton 1958, especially the papers by G. Jaszi, R. T. Bowman and R. A. Easterlin, and E. C. Budd.

ice income; at least a small share must be regarded as income derived from property.

2. Rough calculations make it appear unlikely that the inclusion of income from government property and consumer durables would obliterate the decline in the property share. If we use R. Goldsmith's national wealth estimates[14] and assume that the return on these types of property was equal to that on the types whose earnings are included in the national accounts, the share of property in the national income declines from 31.1 per cent in 1900-09 to 26.2 per cent in 1949-57 instead of from 28 to 22 per cent as the economy-wide basis estimates of Table 1 show. Since approximately 15 to 20 per cent of the tangible assets owned by government consisted of stocks of monetary metals and roughly another 25 to 50 per cent consisted of public land, and since

TABLE 3—GOVERNMENT INTEREST AS PERCENTAGE OF NATIONAL INCOME[a]

| | |
|---|---|
| Kuznets' estimates: | |
| 1919–28 | 1.7 |
| 1929–38 | 2.4 |
| Department of Commerce estimates: | |
| 1929–38 | 1.9 |
| 1934–43 | 1.6 |
| 1939–48 | 1.7 |
| 1944–53 | 1.9 |
| 1949–57 | 1.7 |

[a] National income is inclusive of net interest payments by government.
*Sources:* S. Kuznets [19, pp. 137, 811]; Department of Commerce [34, pp. 4, 6].

our assumption implies that the return on government property was equal to that of private property, we may well have overstated the extent to which the inclusion of income from government property would modify the observed change in income shares. A similar calculation, in which only consumer durables are added, produces a property share of 29.4 per cent in 1900-09 and 23.8 per cent in 1949-57. Thus it is clear that the share of property in the national income declined notably even if generous allowance is made for income from types of property omitted from conventional national income accounting.

3. Whether net interest paid by government should be treated as a factor return or as a transfer is a moot question. The interest shares in Table 1 exclude such payments, thus following the Department of Commerce in regarding them as transfers.[15] Judging from the estimates

[14] [7, pp. 14-15, 42-56]. Data for 1950 and 1955, also prepared by Goldsmith [29, p. 123], were averaged and taken as representative of 1949-57.

[15] Johnson adjusted the original Kuznets interest series so as to exclude net interest payments by government, so the figures in Table 1 are consistent in this respect throughout the entire period.

of government interest from 1919-28 onwards in Table 3, their inclusion would tend to raise the level of the interest and property shares by 1 or 2 percentage points but would have no effect upon the trend.

However, an alternative adjustment, and possibly a preferable one, is to exclude income originating in the government altogether. For the period since 1929, this is easily done by subtracting the compensation of government employees from employee compensation and national income. For the years prior to 1929, when Johnson's version of Kuznets' estimates are used, the same adjustments are made though the justification for so simple a procedure is more dubious. The results, shown in Table 4, do little to alter the conclusion that the share of employee

TABLE 4—LABOR SHARES, EXCLUDING COMPENSATION OF GOVERNMENT EMPLOYEES FROM EMPLOYEE COMPENSATION AND NATIONAL INCOME

| | Employee Compensation | Labor Share[a] | | Employee Compensation | Labor Share[a] |
|---|---|---|---|---|---|
| 1900–09 | 53.3 | 70.7 | 1929–38 | 62.9 | 75.5 |
| 1905–14 | 53.5 | 70.3 | 1934–43 | 60.5 | 73.4 |
| 1910–19 | 51.5 | 68.6 | 1939–48 | 59.5 | 72.4 |
| 1915–24 | 54.6 | 70.1 | 1944–53 | 61.0 | 72.6 |
| 1920–29 | 58.4 | 71.7 | 1949–57 | 63.2 | 72.2 |
| 1925–34 | 61.6 | 73.9 | | | |
| 1930–39 | 63.6 | 76.2 | | | |

[a] Economy-wide basis.

*Sources:* As in Table 1 except for compensation of government employees 1900–09 to 1930–39 which was derived by extrapolation of Department of Commerce figures by use of data of S. Kuznets [19, p. 811], W. King [14, p. 122] and S. Fabricant [6, App. D].

compensation has risen over the period as a whole, but they do throw into question the secular trend in the labor share (economy-wide basis) particularly since 1929. If this were the final adjustment that had to be made, our conclusion would be that much of the increase in the labor share observed in the data for the half century was attributable to the growth in relative importance of government employment and to the circumstance that the current method of accounting for government product leaves no room for returns to any factor but labor.

4. The Department of Commerce, upon whose data we rely for the years since 1929, estimates business and national income by deducting from gross product capital consumption allowances which, except for agriculture, are based upon original rather than replacement cost. From the standpoint of measuring relative shares in income, there may be some preference for calculating property and national income on the basis of replacement cost. In a period of rising prices, it can be argued, original cost depreciation tends to overstate the property share because

it leads to the inclusion in income of what is really a diminution in the real value of capital assets. Contrariwise, in a period of falling prices original cost depreciation tends to understate the property share because it results in the overvaluation of the actual consumption of capital assets in current production.

When replacement cost depreciation is used instead of original cost,

TABLE 5—TOTAL PROPERTY SHARE (ECONOMY-WIDE BASIS) AS AFFECTED BY METHOD OF DEPRECIATION

(Average of percentage shares for individual years)

| | Depreciation at Original Cost | | Depreciation at Reproduction Cost | |
|---|---|---|---|---|
| | Including Government | Excluding Government | Including Government | Excluding Government |
| 1929–38 | 21.1 | 24.5 | 20.7 | 23.5 |
| 1934–43 | 22.0 | 26.6 | 21.3 | 24.8 |
| 1939–48 | 22.1 | 27.6 | 20.8 | 24.6 |
| 1944–53 | 21.6 | 27.4 | 19.5 | 22.8 |
| 1949–57 | 22.0 | 27.8 | 19.3 | 22.1 |

*Sources:* Original cost depreciation shares from Table 1. Replacement cost depreciation shares calculated by adding to Commerce depreciation allowances [34, p. 6] an estimate of the difference between replacement and original cost depreciation. Kuznets' estimates of replacement cost depreciation, adjusted by him to exclude depreciation on government capital, were used [17, Table A-4, Col. 6]. Kuznets' data were available only through 1953; the estimate for 1949–57 in above table is therefore based upon the present author's rough extrapolations. The excess of replacement over original cost depreciation was allocated between corporate and noncorporate enterprise on the basis of the relative amounts of original cost depreciation. Original cost depreciation is given separately for the corporate and noncorporate sectors beginning with 1946 in Department of Commerce estimates [35, pp. 216–17]. Corporate depreciation was extrapolated back to 1929 on the basis of estimates for manufacturing depreciation by D. C. Wooden and R. C. Wasson [39], and noncorporate depreciation was taken as the residual.

the property share in 1949-57 is slightly lower than in 1929-38 whether the compensation of government employees is included or not (see Table 5). The economy-wide property share in privately produced income declines from 29.3 per cent in 1900-09 to 22.1 per cent in 1949-57.[16]

[16] It is difficult to evaluate the effect of the method of depreciation upon the pre-1929 estimates in Table 1. The figures for these earlier years are fundamentally backward extrapolations of Commerce Department data on the basis of shares in aggregate payments. To the extent that the resultant estimates are closer to those that would have been obtained by the application of replacement cost depreciation, our conclusion would be that there has been a fairly consistent downward trend in the property share in national income dating from the second decade of the current century. If, on the other hand, the pre-1929 estimates are closer to those that would have been obtained by the use of original cost depreciation, adustment to a replacement cost basis would, in view of the rising price trends in the first quarter of a century, make for an even sharper decline in the property share than our figures show and also might well indicate that the beginning of the drop goes all the way back to the start of the century.

TABLE 6—LABOR SHARES IN NATIONAL INCOME SINCE 1929, CYCLICAL PEAKS

| | Compensation of Employees | | Economy-Wide Labor Share — Nonfarm Depreciation at | | | |
|---|---|---|---|---|---|---|
| | | | Original Cost | | Reproduction Cost | |
| | Including Government | Excluding Government | Including Government | Excluding Government | Including Government | Excluding Government |
| 1929 | 58.2 | 55.7 | 69.9 | 66.8 | 70.5 | 68.3 |
| 1937 | 65.1 | 61.2 | 78.7 | 74.6 | 79.5 | 76.7 |
| 1944 | 66.4 | 58.6 | 79.2 | 72.0 | 80.0 | 74.4 |
| 1948 | 63.1 | 59.4 | 76.9 | 71.6 | 79.4 | 77.0 |
| 1953 | 68.3 | 64.4 | 78.8 | 73.6 | 81.1 | 78.2 |
| 1957 | 69.7 | 66.3 | 79.3 | 73.4 | 83.7 | 81.2 |

*Sources:* As in Tables 4 and 5.

5. On the other hand, changes in tax laws and regulations permitting accelerated depreciation in the second world war, after the Korean episode and again in the Revenue Code of 1954, led to an overstatement of depreciation and a corresponding understatement of profits in Department of Commerce statistics for recent years relative to earlier ones. The only available estimate of the magnitudes involved indicates that the profit share in 1957 would be one or two percentage points higher if depreciation were computed on the prewar basis [35, p. 15]. It is doubtful that this correction would completely eliminate the downward movement in the property share (see Table 6).

Taking all factors into account, we conclude that the decline in the property share in national income over the first half of the 20th century has been substantial, and that it cannot be explained away either by our methods of estimation or by the vagaries of the social accounting system. This is true, however, only of the general direction of the change between the beginning and end of the half century or more with which we are concerned; the magnitude of the change and its continuity during the period do depend upon the techniques employed to separate the labor and property components of entrepreneurial income.

For the period since 1929 in particular, the measurement of long-term trends in relative shares is greatly complicated, first by the effects of the great depression in producing extraordinarily low property and high labor shares, and secondly by the great wartime increase in government payrolls which accentuated the impact upon relative shares of the accounting conventions employed in estimating income originating in government. An attempt is made in Table 6 to evade at least the first of these difficulties by studying the income shares in successive cyclical peak years for indications of trend. The impression of an upward trend

TABLE 7—SHARES OF CORPORATE PROFITS IN NATIONAL INCOME
(per cent)

| | Taxes | Dividends | Undistributed Profits[a] | Total[a] | |
|---|---|---|---|---|---|
| | | | | Before Tax | After Tax |
| 1919–28 | 1.7 | 5.3 | 1.4 | 8.4 | 6.7 |
| 1924–33 | 1.2 | 6.4 | −1.7 | 5.9 | 4.7 |
| 1929–38 | 1.2 | 6.9 | −3.8 | 4.3 | 3.1 |
| | | | | | |
| 1929–38 | 1.5 | 6.0 | −3.2 | 4.3 | 2.8 |
| 1934–43 | 3.8 | 4.9 | −0.4 | 9.1 | 5.3 |
| 1939–48 | 5.9 | 3.5 | 2.5 | 11.9 | 6.0 |
| 1944–53 | 6.3 | 3.1 | 3.2 | 12.6 | 6.3 |
| 1949–57 | 6.5 | 3.3 | 3.0 | 12.8 | 6.3 |

[a] Includes inventory valuation adjustment.

*Sources:* 1919–38: S. Kuznets' shares of corporate dividends and corporate saving [19, p. 217] in national income adjusted so as to include the federal corporate income and excess profits taxes as shown by G. J. Schuller [23, p. 312]. The shares of corporate profits in the above estimates, it should be noted, differ somewhat from those in Table 1, mainly because different sources and methods were used in deriving the figures.

1929–57: Commerce Department data [34].

in the labor share is strengthened if allowances are made for the special circumstances of 1937 and 1944, the former being depressed relative to the other years notwithstanding its position as a cyclical peak in the National Bureau chronology[17] and the latter being a war year in which government payrolls were double what they had been two years earlier and one and a half times what they were to be two years later. It must be added, however, that averages for overlapping cycles, alternating peak-to-peak and trough-to-trough cycles, show no trend even though the final peak-to-peak cycle (1953-57) has a higher labor share (on a reproduction cost basis) than any other since 1929. Weighing this negative finding against the evidence in Tables 5 and 6, the safest conclusion appears to be that the property share since 1929, even on a reproduction cost basis, has been characterized by near secular stability with some evidence of a slight tendency to drift downwards.

Before turning to the possible reasons for the decline in the property share over the period as a whole, we will take some account of the manner in which these and related shifts have affected the recipients of labor and property incomes. In this connection, particular significance must be attached to the growing role of corporate profits in the composition of property incomes. Unlike the other property shares, allocations to taxes and undistributed profits may create a substantial gap between profits earned by corporations and income actually paid

[17] Over 14 per cent of the labor force was unemployed in 1937 compared to 4 per cent or less in other years shown in Table 6 [36, p. 21].

out by them to income receivers in the form of dividends. The record for the period since the first world war is shown in Table 7.

Well over half of the relative rise in corporate profits from the 'twenties and the 'thirties has been accounted for by the increase in tax liability. The share of dividends in the national income has actually decreased. The implication is that property income has declined relative to labor income in terms of income actually paid to income recipients (personal income) even more than it has in terms of income earned (national income).

The data in Table 8, showing the distributive shares in personal income for overlapping decades, confirm this. Between the 'thirties and the 'fifties, the share of rent, interest and dividends combined declined by 8 percentage points, of which 6 were gained by labor incomes and 2 by transfer receipts. The total property share also shows up with a much less favorable trend on the personal income basis than on the national income basis, at least for the period since the 'thirties. Whereas the figures in Table 1 relating to national income show the total property share holding its own or even gaining since 1929, those of Table 8 relating to personal income indicate that this is true only when the labor basis is used. On the other bases the total property share declines from the 24 to 26 per cent range in 1929-38 to the 14 to 17 per cent range in 1949-57. For the period preceding 1929, transfers and corporate taxes, which constitute the most important differences between national and personal income, were of smaller quantitative significance, and the shares in personal income are not far different in magnitude and direction of change from the shares in national income. Thus, if we avoid the assumption that the vicissitudes of entrepreneurial fortunes are absorbed entirely by one factor or the other, the total property share in personal income declines from 28 or 30 per cent in 1909-18 to 14 or 17 per cent in 1949-57. Service income (i.e., income from employment and the labor share of entrepreneurial income), the other major share, thus gained 6 percentage points in the national income and more than twice as many points in personal income. The impact of the shifts were the greater since the groups relying primarily upon labor incomes probably benefited more from the rise in transfer receipts than did those to whom property incomes were important.

In appraising these results we must, once again, take account of the conceptual underpinnings. The accounting problems we considered in connection with shares in national income either are of limited quantitative significance for personal as for national income, or, as in the case of the valuation basis used for depreciation, irrelevant for the personal income concept. However, since our interest in shares in personal income is more directly related to the fortunes of particular groups of

TABLE 8—DISTRIBUTIVE SHARES IN PERSONAL INCOME, 1909–57
(per cent)

| | National Bureau Data[a] | | | | | Commerce Department Data[a] | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1909–18 | 1914–23 | 1919–28 | 1924–33 | 1929–38 | 1929–38 | 1934–43 | 1939–48 | 1944–53 | 1949–57 |
| 1. Labor income[b] | 51.9 | 56.3 | 61.4 | 64.1 | 64.4 | 61.5 | 63.8 | 65.6 | 66.4 | 67.7 |
| 2. Entrepreneurial income | 27.4 | 24.3 | 19.6 | 15.9 | 14.6 | 14.7 | 16.8 | 18.2 | 17.4 | 14.7 |
| a. Farm | (11.8) | (10.6) | ( 8.3) | (6.4) | (5.9) | (5.9) | ( 6.7) | ( 7.3) | ( 6.8) | (4.9) |
| b. Business and professions | (15.6) | (13.7) | (11.3) | (9.5) | (8.7) | (8.8) | (10.1) | (10.9) | (10.6) | (9.8) |
| 3. Rent | 10.7 | 8.9 | 7.3 | 6.3 | 5.0 | 4.3 | 3.3 | 3.5 | 3.6 | 3.6 |
| 4. Dividends | 6.0 | 5.7 | 5.6 | 6.3 | 6.0 | 5.7 | 4.9 | 3.7 | 3.3 | 3.5 |
| 5. Interest | 4.0 | 4.8 | 6.1 | 7.3 | 7.2 | 10.1 | 7.6 | 5.0 | 4.3 | 4.8 |
| 6. Transfers | —[c] | —[c] | —[c] | —[c] | 2.9 | 3.7 | 3.6 | 3.9 | 5.1 | 5.6 |
| 7. Total | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |
| 8. Rent, dividends and interest | 20.7 | 19.4 | 19.0 | 19.9 | 18.2 | 20.1 | 15.8 | 12.2 | 11.2 | 11.9 |
| Total labor (service) and total property shares[d] with entrepreneurial income allocated on: | | | | | | | | | | |
| A. *Asset basis* | | | | | | | | | | |
| 9. Service | 64.6 | 69.0 | 71.5 | 72.3 | 73.1 | 69.8 | 75.5 | 78.9 | 79.4 | 78.0 |
| 10. Property | 35.4 | 31.0 | 28.5 | 27.7 | 24.0 | 26.5 | 20.9 | 17.1 | 15.5 | 16.4 |
| B. *Labor basis* | | | | | | | | | | |
| 11. Service | 70.2 | 78.1 | 80.9 | 84.1 | 84.3 | 83.7 | 82.1 | 80.3 | 80.0 | 80.1 |
| 12. Property | 29.8 | 21.9 | 19.1 | 15.9 | 12.8 | 12.6 | 14.3 | 15.8 | 14.9 | 14.3 |
| C. *Proportional basis* | | | | | | | | | | |
| 13. Service | 69.7 | 72.1 | 74.1 | 74.4 | 73.9 | 71.1 | 74.7 | 77.5 | 77.6 | 77.4 |
| 14. Property | 30.3 | 27.9 | 25.9 | 25.5 | 23.3 | 25.2 | 21.7 | 18.5 | 17.3 | 17.0 |
| D. *Economy-wide basis* | | | | | | | | | | |
| 15. Service | 71.5 | 74.4 | 76.4 | 76.3 | 75.7 | 72.6 | 77.2 | 81.0 | 81.2 | 80.2 |
| 16. Property | 28.5 | 25.6 | 23.6 | 23.7 | 21.4 | 23.7 | 19.2 | 15.1 | 13.7 | 14.2 |

[a] Figures are averages of percentage shares for individual years.
[b] For 1909–18 to 1924–33 includes government transfer payments. From 1929 on, personal contributions for social insurance were deducted from labor income.
[c] Included in labor income.
[d] Transfer shares as in line 6. See text for explanation of bases for allocating entrepreneurial income.
*Sources*: 1909–38: Derived from D. Creamer [3, pp. 116–23].

income recipients, we must consider the possibility that differential price movements have offset the welfare effects of the changes.

Indeed, Johnson has argued that labor's share in money income has risen to a greater degree than would be the case if income were valued in real terms.[18] His argument centers around the national accounting practice of valuing goods produced and consumed on farms at farm prices. The shift of labor from farm to nonfarm jobs represented by the decline in the agricultural portion of the labor force from 33 per cent in 1900-09 to 10 per cent in 1949-57 meant that the money incomes of workers as reflected in the national accounts increased more than their real incomes. The gap between farm and nonfarm labor money incomes was sufficiently great in 1900-09 (the farm average was $144 per annum and the nonfarm average was $536) so that this shift away from agriculture could have been responsible for a rise in the service share in national income (calculated on the economy-wide basis) from 72 to 75.8 per cent even if no other factors were operative.[19] (This compares with the actual increase from 72 per cent in 1900-09 to 78 per cent in 1949-57 shown in column 11 of Table 1.) The fact that almost two-thirds of the observed increase in the service share could be attributable to the shift out of agriculture underlines the extent to which this increase might be a money rather than a real phenomenon. In order to deflate the increment in labor income resulting from the movement out of agriculture, we take the purchasing power of a dollar of nonfarm income as the equivalent of 70 per cent of that of a dollar of farm income. This represents a rough estimate intended to err on the side of exaggerating rather than minimizing the purchasing power difference; it is based on data in the 1941 BLS-BHNHE survey [27].[20] If the property total is kept constant in dollar amount and the incomes of

[18] [11, p. 180]. We follow Johnson here in discussing shares in national rather than personal income as a matter of convenience, though it is the latter that is relevant in this context.

[19] The figures on farm and nonfarm incomes and labor force are from Johnson [11, pp. 180-81]. The present calculations follow those of Johnson in assuming that (a) the absolute dollar return to property remains unchanged while the dollar total for labor rises due to the shift from farm to nonfarm work, and (b) the rate of return on agricultural property is the same as on nonagricultural property.

[20] Food, housing, fuel, light and refrigeration received in kind by rural families and single consumers during 1941 averaged $497 while the total value of consumption (including both money expenditures and receipts in kind) averaged $1,344 (p. 75). Receipts in kind were valued at retail prices in the rural community (p. 16) rather than as in the national accounts at prices received by farmers. If it is nevertheless assumed that the prices of consumption in kind were twice as great in the nonfarm areas while the prices of goods bought for money were the same, the nonfarm cost of the bundle of goods valued at $1,344 on the farm would be $1,841. Thus the purchasing power of a nonfarm dollar would be 73 per cent of that of a farm dollar. For other calculations regarding the same point see Kuznets [18, pp. 60-62] and N. Koffsky [15, p. 170].

the new workers on nonfarm jobs are deflated to 70 per cent, the service share rises from 72 per cent in 1900-09 to 74.4 per cent in 1949-57. Thus, starting with a service share of 72 per cent, the shift out of agriculture was sufficient to raise the share to 75.8 in money terms and to 74.4 per cent in real terms. On this basis, a little more than one-third of the increase in the service share flowing from the shift out of agriculture represents a rise in money income only; the balance represents an increase in both money and real income.

This kind of calculation, however, understates the extent of the increase in the real service share. The reason is that owners of property as well as providers of labor services have moved off the farms and into nonfarm areas. Indeed during the period when the share of agriculture in the labor force declined from 33 to 10 per cent, its share in the national wealth also declined substantially—from 28 to 11 per cent. Furthermore, in view of the commercialization of agriculture, it is reasonable to suppose that agricultural property owners are to a larger extent than formerly residents of nonfarm areas. Thus, if we are to calculate changes in real shares, we must know what changes have occurred in the purchasing power of a dollar's income to recipients of property incomes as well as to recipients of service incomes. A rather crude calculation based on the shift of two-thirds of the 1900-09 agricultural property income from farm to nonfarm recipients raises the deflated service share in the 1949-57 national income from 74.4 to 75.5 per cent.[21]

It seems unlikely, therefore, that the shift in the composition of personal income from property owners to service recipients has been solely or even mainly a monetary or accounting phenomenon.

## II. *Reasons for the Decline in the Property Share*

The more important factors accounting for the differences in the behavior of the shares in national income and the shares in personal income are fairly apparent. Government policies, reflected in the rise of corporate income taxes and transfer payments, have played a major role in bringing about a sharper contraction in the share of property returns in personal income than in national income. High corporate profits in recent decades have resulted in corporate saving in the form of undistributed profits amounting to 2 or 3 per cent of the national income and have thus further widened the gap between the property share

[21] A two-thirds shift was taken to represent both the decline in the relative importance of agriculture as a source of property income (the decline in the farm share in the national wealth was 60 per cent—from 28 to 11 per cent), and the fact that more recipients of farm property income are urban dwellers than in the past. Here as in the previous calculations involving labor, we are interested in the effect of the shift out of agriculture into nonagriculture and thus we assume that the total quantities of capital and labor are fixed.

in earned income and the property share in income actually paid to income recipients. A shift from the generally sustained prosperity of the 'forties and 'fifties to less favorable conditions might, of course, change this picture drastically.

The forces producing secular changes in the type-structure of national income are more difficult to explain. It is necessary to examine the other secular changes in the American economy, particularly the differential rates of growth in various dimensions of the economy. In Table 9 the changes in population, labor, capital, income, and prices are set forth in terms of averages for overlapping decades. Among the important changes between the opening and the closing decades are the following:

TABLE 9—MACRO-VARIABLES RELATED TO INCOME SHARES, 1900–09 TO 1949–57

| | Population (mil) (1) | Labor Force (mil) (2) | Man-Hours (bil) (3) | National Income ($ Bil) 1929 Prices (4) | Tangible Reproducible Assets ($ Bil) 1929 Prices (5) | Total Wealth ($ Bil) 1929 Prices (6) | National Output Price Index 1929=100 (7) |
|---|---|---|---|---|---|---|---|
| 1900–09 | 83.4 | 33.2 | 99.6 | 39.4 | 125.2 | 191.7 | 52.7 |
| 1905–14 | 91.9 | 37.4 | 107.1 | 45.8 | 149.3 | 224.7 | 58.6 |
| 1910–19 | 100.0 | 40.3 | 109.8 | 55.8 | 174.2 | 257.1 | 73.1 |
| 1915–24 | 107.4 | 42.4 | 111.2 | 63.8 | 199.5 | 291.9 | 95.3 |
| 1920–29 | 115.2 | 45.3 | 114.0 | 71.5 | 234.8 | 343.0 | 104.0 |
| 1925–34 | 121.3 | 48.5 | 118.2 | 70.5 | 263.6 | 384.6 | 92.3 |
| 1930–39 | 127.7 | 51.1 | 120.4 | 71.1 | 268.3 | 385.2 | 82.8 |
| 1929–38 | 126.1 | 52.2 | 122.7 | 72.1 | 268.4 | 387.3 | 83.9 |
| 1934–43 | 130.9 | 56.3 | 130.5 | 96.0 | 275.5 | 384.8 | 87.4 |
| 1939–48 | 137.9 | 61.1 | 140.2 | 129.9 | 293.7 | 399.5 | 111.9 |
| 1944–53 | 148.2 | 64.5 | 140.3 | 149.9 | 325.6 | 437.4 | 145.9 |
| 1949–57 | 159.9 | 67.4 | 140.5 | 167.7 | 364.9 | 489.2 | 167.5 |

*Sources:*

Col. (1): 1900–39 S. Kuznets [17, App. E, Table 37]; 1929–57 Census data [29, p. 5].

Col. (2): 1900–39 S. Kuznets [17, App. E, Table 39]; 1929–57 Census data [28, p. 21].

Col. (3): Col. (2) times 52 weeks times average weekly hours interpolated from F. Dewhurst and assoc. [5, p. 1073] as follows:

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 1900–09 | 57.7 | 1915–24 | 50.4 | 1930–39 | 45.3 | 1939–48 | 44.1 |
| 1905–14 | 55.1 | 1920–29 | 48.4 | 1929–38 | 45.2 | 1944–53 | 41.8 |
| 1910–19 | 52.4 | 1925–34 | 46.9 | 1934–43 | 44.6 | 1949–57 | 40.1 |

Col. (4): 1900–39: R. Goldsmith [7, p. 429, col. 3]; 1929–57: Dept. of Commerce estimates of national income deflated by Commerce implicit price indexes for components of gross national product with weights of .8 for consumption, .1 for gross domestic investment and .1 for government purchases.

Col. (5) and (6): Excluding consumers durables 1900–48: Goldsmith [7, pp. 20–21]; 1949–57: Goldsmith, letter of 11/11/58 (preliminary estimates).

Col. (7): 1900–39: Implicit index derived from Goldsmith's estimates of national income in constant and current prices; 1929–57: See note to col. (4).

1. The population doubled, but owing chiefly to a one-third reduc- tion in the hours of work, total annual man-hours increased only b something more than 50 per cent.

2. Net accretions to wealth were substantial. Reproducible tangibl assets (excluding durable consumers goods) in real terms were nearl three times larger in 1949-57 than in 1900-09.

3. Real income was 4¼ times larger in the aggregate, more tha twice as large on a per capita or per worker basis, and three time greater per man-hour.

4. The price level (in terms of implicit national product deflators was three times higher at the end of the period than at the beginning

Because of differences in the rates of growth, capital expanded rela tive to labor but declined relative to income. As Table 10 indicates, rea reproducible tangible assets per man-hour in 1949-57 were almos double the 1900-09 level, but, owing to the tripling of product per man hour, the capital-output ratio (with capital represented by reproducibl tangible assets) at the close of the half century was only two-thirds th beginning level.

The increases in the quantity of labor and the amount of capital playe a modest role in accounting for the great growth in real product. Th effect of the increased quantities per se may be approximated by assum ing that the average return to labor and capital, each measured by divid ing its quantity into its aggregate income, remained unchanged afte 1900-09. In this event the rise in the quantities of labor and capital b 1949-57 would have produced an increment in real output equivalen only to one-fourth of the increase that actually occurred.[22] Althoug other methods of estimation produce somewhat different results,[23] ther is little doubt that national output rose by much more than input. Th greater growth in output must have been attributable to technical prog ress and/or economies of scale. Even if, as G. J. Stigler suspects [26] th economies of scale were of overwhelming importance in accounting fo the growth in productivity, it seems probable that the historical develop ment of economies of scale was itself dependent upon the discovery an utilization of new techniques. In attempting to explain the changes i

[22] This amounts to asssuming that the prices of capital and labor remained constant ar only the quantities increased. Real output would have risen from $39.4 billion in 1900-( to $72.5 billion in 1949-57. The actual 1949-57 level was $167.7 billion.

[23] J. W. Kendrick has estimated that about half of the increment in real output betwe 1889 and 1953 was due to increases in factor inputs [13, Ch. 4 and App. A]. The diffe ence between Kendrick's estimate and the one given above is attributable mainly to Ke drick's practice of weighting man-hours worked in various sectors by average hourly earnin to obtain an aggregate measure of labor input. Owing to the shift of man-hours into high paying industries, labor input increased by 200 per cent while man-hours increased by on 130 per cent. There was a similar though smaller difference between the increase in capi input and the rise in real capital.

| | Ratio, Reproducible Capital to: | | | Income (dollars) Per: | | | Price of Labor (Wage per Man-Hour) | Price of Capital (Return per $100) Related to: | | $P_k/P_L$ |
|---|---|---|---|---|---|---|---|---|---|---|
| | | Labor | | | | | | | | |
| | Product (1) | Per Member of Labor Force (2) | Per Man-hour (3) | Member of Labor Force (4) | Capita (5) | Man-Hour (6) | (7) | Reproducible Assets (8) | Total Wealth (9) | (10) |
| 1900–09 | 3.18 | 3773 | 1.26 | 1186 | 472 | .395 | .284 | 8.80 | 5.75 | .309 |
| 1905–14 | 3.31 | 3994 | 1.39 | 1205 | 490 | .420 | .301 | 8.54 | 5.67 | .284 |
| 1910–19 | 3.12 | 4323 | 1.59 | 1384 | 558 | .508 | .357 | 9.54 | 6.46 | .267 |
| 1915–24 | 3.13 | 4703 | 1.79 | 1503 | 594 | .574 | .416 | 8.82 | 6.03 | .212 |
| 1920–29 | 3.29 | 5184 | 2.06 | 1578 | 621 | .627 | .448 | 8.71 | 5.96 | .194 |
| 1925–34 | 3.74 | 5438 | 2.23 | 1455 | 582 | .597 | .447 | 6.72 | 4.60 | .150 |
| 1930–39 | 3.77 | 5251 | 2.23 | 1392 | 557 | .591 | .465 | 5.64 | 3.93 | .121 |
| 1929–38 | 3.72 | 5143 | 2.19 | 1381 | 572 | .587 | .465 | 5.56 (5.73)[a] | 3.86 (3.97)[a] | .120 (.122)[a] |
| 1934–43 | 2.87 | 4894 | 2.11 | 1705 | 733 | .735 | .578 | 7.44 (7.80) | 5.32 (5.59) | .129 (.134) |
| 1939–48 | 2.26 | 4805 | 2.10 | 2125 | 942 | .926 | .733 | 9.21 (10.07) | 6.77 (7.40) | .126 (.136) |
| 1944–53 | 2.17 | 5046 | 2.32 | 2322 | 1011 | 1.068 | .859 | 8.99 (10.40) | 6.69 (7.75) | .105 (.119) |
| 1949–57 | 2.18 | 5417 | 2.60 | 2490 | 1049 | 1.194 | .964 | 8.86 (10.67) | 6.61 (7.96) | .092 (.109) |

*Sources:* (Roman numerals refer to columns of Table 9):

Col. (1): V÷IV.
Col. (2): V÷II.
Col. (3): V÷III.
Col. (4): IV÷II.
Col. (5): IV÷I.
Col. (6): IV÷III.
Col. (7): (6)×Labor share in income (100—col. 11, Table 1).
Col. (8): Property share in income (col. 11, Table 1)÷(1).
Col. (9): Property share in income (col. 11, Table 1)÷ratio of VI to IV.
Col. (10): (8)÷(7).

[a] ( ) refers to original cost basis.

factor shares, account must therefore be taken of the background of rapid economic growth made possible in large part by technical progress.

Although none of the changes proceeded at a steady rate during the entire period, we shall somewhat simplify the problem of analysis by assuming that the process of change was a continuous one and by seeking an explanation of the net decline in the property share that occurred in the period as a whole.

It is possible to formulate a variety of tautological relationships between the wage or the property share on the one hand and its various "determinants" on the other hand.[24] For example, if:

[24] Cf. the formulation offered by N. Kaldor [12]. His basic equation may be written as follows:

$$\frac{R}{Y} = \left(\frac{1}{s_r - s_w}\right)\left(\frac{I}{Y} - s_w\right)$$

where $I$ = investment, $s_r$ = marginal propensity to save out of property income, $s_W$ = marginal propensity to save out of wage income, and $R$, $W$, and $Y$ have the same meanings as in the text.

Kaldor advances the "Keynesian" hypothesis that the investment propensity, $I/Y$, is an independent variable, invariant with respect to the two saving propensities, which determines $R/Y$, the property share. However, our interest here is not so much in this theory as in the tautology itself which gathers together a different group of variables affecting relative shares than those we have used in the text.

Kaldor's formulation calls attention to the relative changes in the propensities to save out of wage and property incomes, respectively. However, occupation and other socio-economic characteristics of consumer units may be more important than the source of income in determining the disposition of income between spending and saving. Indeed, it is not clear that propensities to save out of particular types of income are meaningful concepts for empirical analysis since the groups which receive property incomes tend to earn significant amounts of labor income as well. For example, estimates prepared for a forthcoming study indicate that in 1950 urban consumer units headed by salaried managers received 25 per cent of total interest, dividends and rents accruing to all urban consumer units and 13.5 per cent of total wages and salaries, the former accounting for 21 per cent and the latter for 78 per cent of their income. Kaldor's formulation implies that these and others receiving incomes from different sources have different propensities to spend each type of income.

Leaving these questions aside, we examine the inferences concerning the change in the property share that can be drawn from the variables in Kaldor's equation. We know from the investigation reported above that $R/Y$ declined from 28 per cent in 1900-09 to 19.3 per cent in 1949-57, and we have Kuznets' estimates showing that $I/Y$—actually, the ratio of net capital formation to net national product—was .135 in 1899-1900 and .074 in 1949-55 [17, Table III-8]. The saving propensities are, of course, unknown, but if, as Kaldor seems to believe, the propensities tend to remain unchanged over long periods of time, they may have been the same in both the beginning and terminal decades of our period. This gives us two equations, one for each decade, with the two saving propensities as unknowns. The values come out to .643 for $s_r$ and —.062 for $s_w$. On the assumption of constant propensities, the implication is that the decline in the property share was associated with the decline in the investment propensity. However, we have no knowledge of the actual behavior of the propensities, or, indeed, whether they can actually be calculated meaningfully. An increase in $s_r$ or in $(s_r - s_w)$ or a decrease in $s_w$, with $I/Y$ constant, would also have lowered $R/Y$.

$P$ = price  $L$ = labor  $W$ = aggregate wages
$Q$ = quantity  $K$ = capital  $R$ = total property income
$Y$ = total income = $W + R$

$$\frac{R}{W} = \frac{Q_K}{Q_L} \times \frac{P_K}{P_L}. \tag{1}$$

Thus the relation of property to wage income[25] can be regarded as the product of the capital-labor quantity and price ratios, and changes in the division of income result from changes in these ratios. Since the ratio of the percentage change in the quantity ratio to the percentage change in the price ratio is equal to the elasticity of substitution [9, Ch. 4], we are dealing with the familiar proposition that changes in relative shares depend upon the elasticity of substitution.

Now if we could assume that the price and quantity ratios would move in opposite directions, the opportunity for factor substitution would clearly serve as a built-in stabilizing mechanism limiting changes in relative shares. Where the opposite percentage changes in the quantity and price ratios are equal—i.e., where the elasticity of substitution is unity—relative shares will of course remain unchanged. Even with fairly large departures from unity, however, factor substitution may confine share shifts to fairly narrow limits. For example, with a 75-25 division of national income between labor and capital, a 20 per cent increase in the ratio of the price of labor to the price of capital would not cause the labor share to stray more than 3 or 4 percentage points from 75 were the elasticity of substitution as low as .25 or as high as 2.

In the historical event, the quantity and price ratios did move in opposite directions thus tending to limit the extent of the change in relative shares. Our data for the terminal periods, using reproducible capital for both decades and replacement-cost figures for 1949-57, are as follows:

| | $Q_K/Q_L$ | $P_K/P_L$ | $R/W$ |
|---|---|---|---|
| 1900-09 | 1.26 | .309 | .39 |
| 1949-57 | 2.60 | .092 | .24 |

The doubling of the quantity ratio and the drastic decline of the price ratio imply an "historical" elasticity of substitution of .64, but the

[25] Once $R/W$ is known, the property share in income, $R/Y$ can be calculated since the sum of the wage and property shares must equal 1:

$$w + r = 1, \text{ where } w = W/Y \text{ and } r = R/Y$$
$$(r \div R/W) + r = 1$$

The property share can be found by substituting the numerical value of $R/W$ and solving for $r$.

mechanism underlying these changes is far from clear.

A hypothesis consistent with the facts is that the behavior of the price and quantity ratios is attributable to the differences in the supply conditions under which capital and labor are provided, and that the demand conditions were permissive rather than determining. This can be brought out by making a series of alternative assumptions about the relative impact of economic growth upon the demands for capital and labor specifically, and determining what supply conditions for the two factors could have produced the historical changes in their price and quantity ratios that have been observed.

Let us first assume that the expansion in demand was neutral in the sense that the relative marginal productivities were unchanged for any given ratio of capital to labor. (Geometrically, the isoquant drawn with capital on the vertical and labor on the horizontal axis merely shifts upward and to the right; its slope is unchanged at the point of intersection with any given gradient drawn from the origin indicating the capital-labor ratio.) If both capital and labor were perfectly elastic in supply, there would be proportionate increases in both and no change in relative prices or shares in income would occur. If one were more elastic in supply than the other, the relative quantity of the more elastic factor would increase, and given imperfect substitutability between the factors, its relative price would fall. In the actual event, capital nearly tripled in quantity with no secular increase in rate of return,[26] while the number of man-hours rose by less than 50 per cent despite a better than threefold rise in hourly compensation. Thus if technical progress was neutral, the facts would fit the hypothesis.

Next consider the case in which economic growth is not neutral but increases the demand for capital relative to labor: For any given capital-labor ratio, the marginal productivity of capital has improved relative to that of labor. In Hicks' terminology, technical progress is labor-saving [9, pp. 121-22]. (The higher isoquant has a smaller slope than the lower one at the points of intersection with any given gradient from the origin.) But we are immediately confronted with the problem of explaining why the return on capital did not rise relatively to that of labor. As in the previous case, the answer appears to turn upon the supply conditions; additional supplies of capital were readily forthcoming, while the supply of labor was so inelastic that even the smaller increase in the demand for it resulted in large price increases.

[26] Actually, our estimates of the rate of return on capital show considerable variation from decade to decade, but even when the high rates of the prosperous 'forties and 'fifties are reduced to take account of replacement cost depreciation, there is slight evidence of a downward trend. An examination of stock and bond yields does not appear to contradict this conclusion [28, pp. 279-81] [30, pp. 98-100].

Finally, there is the case in which innovations raise the marginal productivity of labor in relation to that of capital, Hicks' capital-saving category. If the supplies of both factors were perfectly elastic, there would be an increase in the relative quantity of labor. If, at the other extreme, the supplies of both factors were perfectly inelastic, the relative quantities would remain the same and the relative price of labor would rise. The only circumstances that would produce a *decrease* in the relative quantity of labor as well as an increase in its relative price, such as occurred in the history of the period, are those in which the supply of capital was more readily responsive to increased demand than was the supply of labor.

Whatever the nature of the demand influences, therefore, the increase in the capital-labor ratio and the relative increase in the price of labor are attributable to differences in supply conditions. However, we have explained only the direction of the movements of the quantity and price ratios; to account for the rise in the labor share, we have to explain why labor increased in relative price more than it decreased in relative quantity. The answer to this question depends not only upon the difference in the supply elasticities but also upon the marginal rate of substitution between capital and labor (MRS). If the MRS were constant from one equilibrium position to another, there could be no change in relative prices; only relative quantities would change. The fact that relative prices changed so much indicates not only that there was a great difference in the elasticities of supply, but also that the substitutability of capital for labor diminished. Nevertheless, the impetus to the change came from the supply conditions for the factors, for otherwise with neutral innovations there would have been no change in the MRS and with labor-saving innovations the change in the MRS would have been in the opposite from the observed direction. Only in the less likely case of capital-saving inventions would the MRS move in the right direction without aid from the supply side, and even in this case we have to invoke the supply conditions to explain the quantity changes.

We have thus far glided over the difficulties encountered in deriving supply and demand curves from historical data. Not the least of these is the familiar problem of distinguishing the supply from the demand effects in historical statistics. If we could assume that the supply schedule for labor had remained constant over the entire half century and that the observed prices and quantities reflect the upward shift of the demand for labor, the arc elasticity of supply of man-hours could be easily calculated as .31. If the supply also shifted upward though not as much as did the demand, the elasticity may well have been less than .31; if the number of man-hours offered at each real hourly wage actually tended to diminish, the elasticity would have been greater than .31.

In any case, the tripling of real earnings per man-hour with an increase in man-hours worked of only 40 per cent is consistent with the hypothesis of inelastic supply or with the related hypothesis that the growth in demand was greater than the growth in supply.

Matters are much less clear-cut with respect to capital. Even if we assume that the actual rate of return represents a good stand-in for the expected rate of return, the rate which presumably governs the supply of capital, it is difficult to draw any generalizations (see Table 11).

TABLE 11—REPRODUCIBLE TANGIBLE ASSETS

| | Total Stock ($ bil) | Increase over Preceding Period | | Price of Capital (per cent) |
|---|---|---|---|---|
| | | $ Bil | Per Cent | |
| 1900–09 | 125.2 | | | 8.80 |
| 1905–14 | 149.3 | 24.1 | 19.3 | 8.54 |
| 1910–19 | 174.2 | 24.9 | 17.4 | 9.54 |
| 1915–24 | 199.5 | 25.3 | 14.4 | 8.82 |
| 1920–29 | 234.8 | 35.3 | 20.6 | 8.71 |
| 1925–34 | 263.6 | 28.8 | 12.3 | 6.72 |
| 1930–39 | 268.3 | 4.7 | 1.8 | 5.64 |
| 1929–38 | 268.4 | | | 5.56 ( 5.73) |
| 1934–43 | 275.5 | 7.1 | 2.6 | 7.44 ( 7.80) |
| 1939–48 | 293.7 | 18.2 | 6.6 | 9.21 (10.07) |
| 1944–53 | 325.6 | 31.9 | 10.9 | 8.99 (10.40) |
| 1949–57 | 364.9 | 39.3 | 12.1 | 8.86 (10.67) |

*Sources:* Tables 9 and 10. Bracketed figures refer to cost depreciation.

The periods spanning the years of the great depression have lower rates of return and smaller increments in the stock of capital than the more prosperous periods before and after. In the more prosperous periods, however, there is little association between changes in the rate of capital formation and changes in the actual rate of return from one overlapping decade to another. Perhaps the actual rate of return is a poor proxy for the expected rate, particularly since capital gains, which are excluded from the actual rate of return, may at certain times have been a more important motive to capital formation than was income in the sense used in the national accounts. Also, the rate of return which is relevant to new investment is the marginal rather than the average rate which is shown. Finally, and this is an important limitation for labor as well as capital, price might be a relatively minor influence in determining changes in available quantities.

Whatever the reason, then, under conditions of rapid expansion in production labor was relatively inelastic in supply and rising rapidly in price and capital was apparently either much more elastic or at any

rate rapidly growing in supply. Thus entrepreneurs substituted capital for labor, or to put it more precisely, they increased their use of capital at a more rapid rate than their use of labor. It is not clear in what degree the relatively expanded use of capital was possible by virtue of existing techniques—owing either to the adaptability of equipment in use or to the availability of stand-by techniques requiring more capital and less labor—or in what degree it was the result of newly created capital-using techniques the development of which was stimulated by the growing relative scarcity and high price of labor. If the common conception of the rate of application of new knowledge to industrial processes has any validity innovations must have played a major role in the change in the ratio.

A slightly different view of the same phenomena can be obtained by decomposing the elements in equation (1) so as to express the property-labor income shares as the product of (a) output per man-hour, (b) the capital-output ratio, and (c) the ratio of the price of capital to the price of labor:

$$\frac{R}{W} = \frac{Y}{Q_L} \times \frac{Q_K}{Y} \times \frac{P_K}{P_L}. \tag{2}$$

One of our new terms $(Y/Q_L)$ is a measure of the average productivity of labor and the other $(Q_K/Y)$, the capital-output ratio, is the reciprocal of the average productivity of capital. The capital-output ratio rose in the depressed 'thirties when capacity was not fully utilized, declined in the booming 'forties when capacity was used to the hilt, and during the 'fifties remained at a level corresponding to about 70 per cent of the 1900-09 level; in terms of capital productivity this amounts to an increase of less than 45 per cent between the opening and closing decades of our period. Labor productivity, on the other hand, doubled within the same time span. This relative economizing in the use of labor may seem to imply that innovations during the period must have been labor-saving, but the economizing may flow merely from a shift in factor proportions; the average productivity of labor necessarily rises relative to that of capital whenever the quantity of labor falls relative to that of capital,[27] whether the change in relative quantities is or is not due to changes in techniques. The presumption in favor of the hypothesis that adoption of labor-saving techniques accompanied the growth in capital relative to labor is supported by the further presumption that a rapidly rising relative wage rate created an inducement for innovations biased in this direction.[28]

[27] $$\frac{Y}{Q_L} \div \frac{Y}{Q_k} = \frac{Q_k}{Q_L}.$$

[28] Probably the most common view is that inventions have been labor-saving. See Hicks

Our discussion thus leads to the view that the impetus to the rise in the labor share came from sharp increases in real wages owing to the lack of responsiveness in the supply of man-hours to the rising demand for labor attendant upon rapid economic growth. The use of relatively more capital was made possible by price-induced substitution and by price-induced capital-using (labor-saving) innovations. We may infer that the substitutions encouraged by the relative cheapening of capital were not confined to the combinations of factors used to produce given products, but extended to the alteration of the structure of consumption in favor of capital-intensive products to the disadvantage of labor-intensive goods.

## III. *Additional Parts of the Puzzle*

Three other elements that will have to be taken into account when a complete theory of factor shares is developed are (1) price level changes, (2) changes in the degree of monopoly, and (3) other shifts in microeconomic relationships.

1. For rising prices to bring about a reduction in the property share, their adverse effect upon contractual property incomes (rent and interest) must outweigh their positive effect upon residual property incomes (profits). This differential effect of rising prices upon rent and profit may explain much of the fall in rent and rise in profits that concerned us in an earlier section of this paper.

2. The assumption of a significant and growing degree of monopoly was used by Kalecki as the basis for a theory of relative shares which has been criticized elsewhere.[29] In a firm enjoying a sheltered market position, both wage and nonwage incomes may be higher than under competitive conditions, but the nonwage incomes are apt to be more above the competitive level than the wage incomes since the firm is presumably hiring labor under market conditions that are more competitive than those under which it is selling its product. Therefore, the share of labor will vary inversely with the degree of monopoly exercised by firms, and if monopoly has had anything to do with the rise in the labor share monopoly must have become less pervasive. Or, if the degree of monopoly in product markets has remained unchanged or even increased somewhat, perhaps the rising role of unions has provided an offsetting source of monopoly power that has pushed the wage share up. However, there is no agreement among labor economists that unions have in fact succeeded in raising labor's share [22, pp. 184-85].

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[9, p. 124]. For an argument that inventions may have been neutral, see Solow [25] and Reder [22].

[29] Kaldor [12]. It should be noted, however, that Kalecki's original formulation referred essentially to wage earners in manufacturing.

3. Explanations of changes in relative shares have been sought also in microeconomic labor-capital relationships that are not necessarily related to the degree of monopoly. Several writers have, for example, analyzed the effects upon relative shares of changes in the importance of different sectors of the economy. Kuznets [19, pp. 241-50], Denison [4] and Johnson [11], working with different periods during the past half century, have produced results that indicate that changes in the industrial composition of employment and income have tended somewhat to increase the share of labor. Johnson, dealing with the entire 50-year period, stressed the favorable effect on the labor share of the relative expansion of the labor-intensive industries such as trade and services at the expense of capital-intensive agriculture. Denison, studying the period between 1929 and 1950, found that the small increase in the employee share in income produced by the business sector was due to changes in industrial composition which were sufficiently great to offset intra-industry decreases in labor shares.

These investigations did not stress the possibility of systematic relationhips between changes in wages in particular sectors of the economy on the one hand, and changes in the relative importance of the sectors in national output on the other hand—relationships which might, if they existed, have significant effects upon the wage share in the economy as a whole. This possibility was recently considered by R. M. Solow in a suggestive but preliminary survey of the connections between sector and over-all wage shares [24]. Solow called attention to the possible operation of what might be termed a microeconomic share-stabilizing mechanism. This would exist if there were a tendency for industries with rising labor shares to diminish in relative importance in originating national income and for those with falling labor shares to increase in relative importance. However, Solow's findings concerning the existence of such a mechanism were generally negative, as were the results of our own limited experiment along similar lines.[30] On the other hand, the data are not without some traces of a tendency for shifts in the relative importance of sectors to be in the opposite direction from changes in the wage shares of the corresponding sectors.[31] A more thorough study based on cyclically adjusted data is necessary before the negative evidence found thus far is confirmed.

Some of the changes which we have been discussing as though they

[30] We compared changes in shares in national income between 1929 and 1956 for 55 private industries included in Commerce Department national accounts data with changes in the wage share in income originating in each industry.

[31] For example, if we examine data for the 7 sectors used by Solow in his Table 1 for the extreme years in his tabulation, 1929 and 1953, we find that in 5 out of the 7 cases the sector's share in national income moved in the opposite direction from the wage share in the income originating in the sector.

characterized the half century as a whole actually occurred within a briefer span of years within the period. For example, the rise in capital-labor ratios is mainly a pre-1929 phenomenon, rather than one continuously operative throughout the whole period, and it must be remembered that all the ratios involving capital are affected by the under-utilization of capacity during the depressed 'thirties and by the high rates of utilization in the prosperous 'forties and 'fifties. If we tried to revise our speculations to take account of these differences in time, our task would be greatly complicated by the necessity for choosing among alternative hypotheses regarding leads and lags, and it is doubtful that we have sufficiently reliable data for a long enough period to make this exercise worth while.

There is, however, one further point to which reference might be made as long as our speculative license has carried us this far. It is to the possibly important pieces of the share puzzle that lie within realms of the social mechanism other than the purely economic. Some of these are obvious and tangible like the dependence of changes in the size of the labor force upon the rate of population growth and labor force participation rates. Others, perhaps equally important, are much more difficult to establish. One source of the factors favoring the labor share may conceivably be found in the relative effects upon the supply prices of labor and capital of our society's quest for social justice and emphasis upon egalitarianism; perhaps the social setting has tended to prepare rentiers for euthanasia and to lead workers into a revolution of rising expectations.

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# MANUFACTURERS' INVENTORY INVESTMENT, 1947-1958: AN APPLICATION OF ACCELERATION ANALYSIS

*By* PAUL G. DARLING*

Perhaps the best known explanation of fluctuations in inventory investment is that of Lloyd Metzler [7] [8] which rests on an aggregative stock-flow or acceleration analysis. The Metzler theory bids for attention because it is simple and general, which on a priori grounds is scientifically appealing, and because certain of its implications are compatible with empirical data. Nevertheless, for at least three reasons one may question its usefulness both in forecasting and in explaining past events: first, its mechanism is extremely aggregative; second, it relies on a constant "normal" stock-sales ratio whereas in reality this ratio may fluctuate over the course of the cycle; and finally, the structure of business behavior, institutions, and technology may prove to be too unstable over time to permit a Metzler-type econometric model, whose parameters have been derived from a past period, to be useful in prediction.[1]

It is possible to move part way towards a resolution of these three uncertainties by subjecting several inventory investment functions, incorporating the basic ingredients of the accelerator part of the Metzler theory, to a test against time series of manufacturers' inventory and sales data. In Part I a number of regressions are described which yield new evidence bearing on the problems of aggregation, the variability of the stock-sales ratio, and the stability of functional parameters. The significance of these findings is assessed in Part II.

## I. *The Regressions*

The accelerator function suggested by Metzler [7, p. 125], provides for a target ("normal") ratio of stocks to sales, a term which compares the actual level of inventory with the target level, and a lagged response

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[1] Among the writers who have discussed the problem of aggregation in inventory analysis are Abramovitz [1, Ch. 13], Bassie [3, p. 39ff] [4, pp. 221-25], Mack [6], Metzler [9, pp. 330-33], Modigliani [10], Nurkse [11]. Concerning the conformity of the Metzler model to empirical data see Abramovitz [1, Chs. 4 and 6] [2] and Metzler [9].

to the comparison term. This sort of function has to be modified, however, by inclusion of a speed-of-adjustment parameter which measures the rate at which technological-logistical factors permit inventories to be altered once the response has been made. Accordingly, the basic function underlying the regressions of this section, in linear form for the sake of simplicity, is as follows:[2]

$$\Delta H = a_0 + a_1 \, (rS - H)_{-t} + u \qquad (1)$$

where $\Delta H$ = inventory investment; $H$ = aggregate inventories; $S$ = aggregate sales; $r$ = the target ratio of inventories to sales; $a_1$ = the speed-of-adjustment factor measuring the portion of the divergence, $(rS - H)$, which it is feasible to adjust each period; and $u$ = random disturbance. The response lag, $t$, is discussed below.

The U.S. Commerce Department's seasonally adjusted monthly aggregates for manufacturers' sales in current prices and inventory at current book-value, now available for the substantial period from 1947 to date, are the underlying data for the regression tests. Three aspects of the regression time series require comment. First, the monthly data are combined into quarterly aggregates (see definitions of variables in Table 1 below) in order to reduce the effects of very short-run disurbances likely to appear in exaggerated form in monthly series. Second, inventory investment ($\Delta H$) is measured as the quarterly change in book-value of inventories; since few of these inventories were valued on a LIFO basis this is approximately equivalent to measuring the flow of funds expended out of working capital for inventory purposes, rather than a measurement of the current value of the physical change in stocks. And third, the time series are kept in current-value form, i.e., not deflated for price-level change.

Although the use of book-value data in current dollar form may bias somewhat the regressions described below, several mitigating factors

[2] Because this function is to be fitted to the data in isolation from the other relationships that define a closed system, an additional comment may be needed. The dependent variable in (1) measures *intended* inventory change whereas the data to which this variable is to be fitted represents, of course, *actual* change. Since shifts in production levels feed back on sales through a multiplier mechanism divergences between intended and actual investment will tend to be systematically distributed over the course of the inventory cycle, with actual investment in inventory falling short of intended accumulation during the expansion phase and conversely during the downswing. These divergences for each current period will appear as part of the residual, $u$, of equation (1), with past period differences accounted for in the level of stocks in the comparison term, $(rS - H)_{-t}$. The current period divergences, however, appear to be not so large (see values for $\delta^2/s^2$ and $\overline{R}$ in Table 1) as to preclude the use of a single-equation model of this sort in short-run forecasting. It might also be added that a finding that current-period divergences between actual and intended investment are relatively small is not necessarily damaging to the Metzler theory where they play an important role in generating cycles. Even though small, such divergences produce a "never-quite-catching-up" process that helps propel the system through cyclical phases.

TABLE 1—REGRESSION MEASURES FOR INVENTORY INVESTMENT FUNCTIONS—ALL MANUFACTURING COMPANIES, THIRD QUARTER 1947 THROUGH THIRD QUARTER 1958

(based on quarterly data in billions of dollars at current prices, inventory at book value)

| Regression No. | Period | Coefficients and Standard Errors (in Parentheses) with $\Delta H$ a Function of: | | | | $\bar{S}$ | $\bar{R}$ | $\delta^2/s^2$ | Implied Value of $r$ (when $\Delta O$ = zero) |
|---|---|---|---|---|---|---|---|---|---|
| | | Constant | $S_{-1}$ | $H_{-2}$ | $\Delta O_{-1}$ | | | | |
| I | Full period | −.625 | .581 (.046) | −.299 (.022) | — | .508 | .896 | 1.49 | — |
| II | Full period | −.387 | .415 (.044) | −.212 (.022) | .324 (.054) | .375 | .945 | 1.85 | 1.95 |
| III | Full period less 1949-2Q and 1949-3Q | −.039 | .392 (.041) | −.207 (.020) | .310 (.049) | .340 | .953 | 2.15 | 1.89 |
| IV | Last 23 quarters | .009 | .374 (.066) | −.198 (.029) | .370 (.088) | .350 | .952 | 1.72 | 1.89 |

*Notes*: i. Definitions of variables:

$\Delta H$ = Quarterly change in seasonally adjusted inventories.

$S_{-1}$ = Mean monthly seasonally adjusted sales of preceding quarter.

$H_{-2}$ = Mean end-of-month seasonally adjusted inventories of second preceding quarter.

$\Delta O_{-1}$ = Mean monthly difference of preceding quarter of seasonally adjusted new orders minus seasonally adjusted sales.

ii. Coefficients of simple correlation between pairs of the four variables entering into Regression II above are as follows (variables in left-hand column are the dependent variables and those along top row are independent variables):

| | $S_{-1}$ | $H_{-2}$ | $\Delta O_{-1}$ |
|---|---|---|---|
| $\Delta H$ | +.035 | −.323 | +.818 |
| $S_{-1}$ | — | +.919 | +.002 |
| $H_{-2}$ | — | — | −.258 |

*Source of data*: *Business Statistics* and *Survey of Current Business*.

should be borne in mind. In the absence of accurate data concerning seasonal and cyclical variations in the time composition of end-of-month inventories and because of the constantly increasing use of LIFO valuation methods [5, p. 17], the process of attempting to reduce inventory data to a measurement of physical volume would be almost sure to introduce its own form of bias. Also important is the fact that the regressions of this section are based on time-series differences, that is, $\Delta H$ as the dependent variable and the difference term $(rS - H)$, as the principal independent variable, a procedure which eliminates much of what otherwise might be a spurious correlation caused by price-level movements.[3] Finally, a matter to be treated more fully in Section II, inventory decisions are based partly on financial considerations (e.g., the availability of working-capital funds) which bear on stocks measured in current rather than deflated prices.

The monthly inventory and sales data underlying the regression time series are plotted in Chart 1, covering the period from the third quarter of 1947 through the third quarter of 1958, with sales in current dollars and inventory at current book value (sections A and B). The ratio of monthly inventory to smoothed monthly sales,[4] and the quarterly change in stocks in bar-graph form, are also shown (sections C and D). The over-all pattern of fluctuations in these series is consistent with a Metzler-type accelerator function. In general, inventory rises and falls in lagged association with sales. The ratio of stocks to sales consequently moves up and down with a still longer lag, and the magnitude of this ratio appears to influence inventory investment very strongly, high values for the ratio being associated with negative investment and conversely. These graphic patterns suggest the existence of a measurable functional relationship.

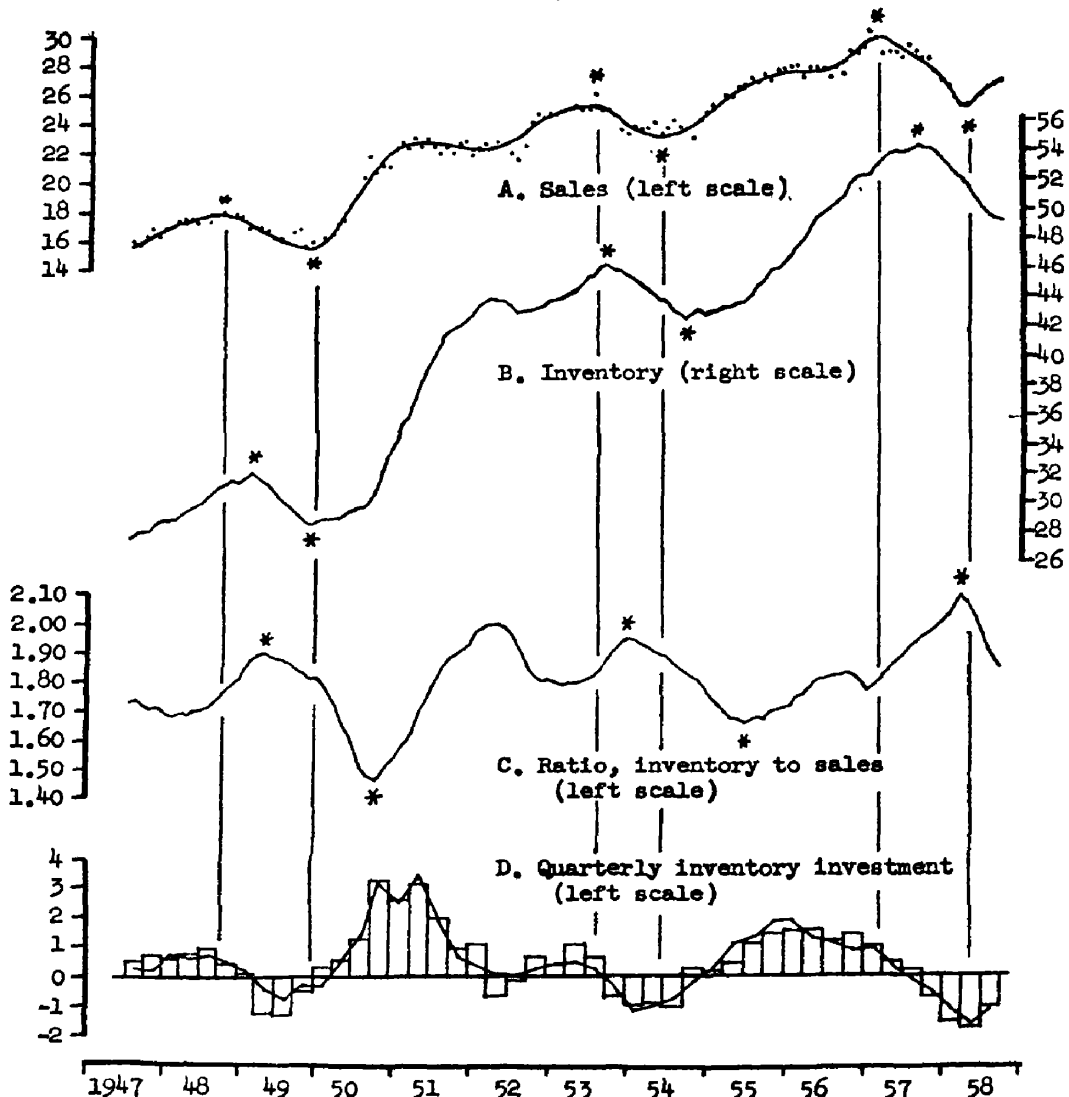
To determine this relationship, equation (1) is fitted to quarterly data after first being simplified to the following form:

$$\Delta H = a_0 + bS_{-1} - a_1 H_{-2} + u \tag{2}$$

where the coefficient $b = a_1 r$, and the lag subscripts are measured in quarter-year units. The one-quarter lag for $S$ and the two-quarter lag for $H$ were empirically determined by trial-and-error regressions. The longer lag for $H$ as compared with $S$ may reflect the fact that sales data are more current than inventory data at the time inventory decisions are made. However, because the equation has been abstracted from the

[3] For the period 1919-1938, Abramovitz finds "there is little difference between the behavior of manufacturers' inventory investment measured in constant prices and in current prices" [1, p. 341 and Chart 64].

[4] In Chart 1 the actual monthly sales data appear as dots. Smoothed sales are also shown as a freehand line. The regressions of this section are based on the actual, not the smoothed, data and are therefore unaffected by this somewhat subjective procedure.

CHART 1. SELECTED DATA FOR MANUFACTURERS, THIRD QUARTER, 1947, THROUGH THIRD QUARTER, 1958[a]

[a] All dollar figures in billions of current dollars with inventory at book value. *Section A:* Seasonally adjusted monthly sales (dots) and monthly sales smoothed with freehand curve; *Section B:* Seasonally adjusted end-of-month inventory; *Section C:* Ratio of seasonally adjusted inventory to smoothed monthly sales: *Section D:* Quarterly inventory investment seasonally adjusted (bar graph) and inventory investment computed from regression equation II of Table 1 (line graph). Peaks and troughs of series A, B and C during the 3 major postwar cycles are marked with asterisks.

full set of relationships that define a closed system, how closely these empirically derived lags measure true behavioral lags is questionable.[5]

A least-squares regression of equation (2) against the quarterly data

[5] See footnote 2 above. The fairly long lag for $H$ may result from the failure of the equation's variables to account for the differences between actual and intended investment.

yields the measures shown as Regression I in Table 1, where $\overline{R}$ and $\overline{S}$ represent the adjusted multiple correlation coefficient and the adjusted standard error of estimate, respectively, while $\delta^2/s^2$ stands for the Von Neumann ratio of the mean-square-successive-difference to the variance.[6] The regression fit is reasonably close. The coefficients for $S_{-1}$ and $H_{-2}$ are of the expected sign, +.581 and −.299, respectively, and both exceed their standard errors by more than 10 times. The multiple correlation coefficient, $\overline{R}$ = .896, implies that the regression plane accounts for 80 per cent of the variation in inventory investment over the 45-quarter period.

Nevertheless, several aspects of the foregoing regression leave something to be desired. First, one would hope for a zero-valued constant term in the regression equation since the underlying hypothesis implies that a firm should be content with an existing level of inventory (i.e., $\Delta H = 0$) when the difference term, $(rS - H)$, has been brought to a zero value. The constant term for Regression I of −.625 (billions of dollars) is a substantial departure from the expected zero. Second, the Von Neumann ratio, $\delta^2/s^2$, is somewhat low, suggesting the possibility that the residuals, $u$, contain an element of systematic variation. Although a part of such nonrandom variation might be due to the systematic distribution of unintended inventory changes (see footnote 2 above), another part might be the result of the fact that the target stock-sales ratio, $r$, is not a constant but shifts in value in some ordered way over the course of the inventory cycle.

Regression II assumes a variable stock-sales ratio. For this test it is hypothesized that $r$ rises in value when new orders booked by firms exceed their sales, i.e., when backlogs are rising and increasing future sales are indicated, while conversely, $r$ falls when new orders fall short of sales. This hypothesis may be tested with the following functional

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For example, during the very early part of an upswing when actual accumulation will tend to fall short of intended investment a long lag for $H$ will make equation (1) predict a smaller amount of inventory investment than will a short lag. During the later part of the expansion the longer lag swells the prediction of inventory accumulation as compared with a short lag. Thus the empirically determined lag of two quarters for $H$ may provide a merely mechanical and coincidental offset which compensates for at least some of the changes in the difference between actual and intended inventory investment over the course of the cycle. Even though the length of this lag is spurious in the sense that firms do not behave on the basis of this long lag, one might be justified, in short-run forecasting, in making use of such a mechanical offset as a device which partially compensates for the errors introduced when one equation is abstracted from a system of related equations.

[6] As with most time-series work, multicollinearity in the data requires that the regressions measures be interpreted with some caution. This problem does not appear to be especially serious here, as the reader may judge for himself by inspecting the tabulation in a footnote to Table 1 of measures of simple correlation between pairs of the variables used in Regression II.

representation, again in linear form for the sake of simplicity:

$$\Delta H = a_0 + a_1 (rS_{-1} - H_{-2}) = u \tag{3}$$

$$\text{where } r = b_0 + b_1 \left(\frac{\Delta O}{S}\right)_{-1} \tag{4}$$

In equation (4), $\Delta O$ = change in unfilled orders, that is, new orders minus sales, and this term is kept relative to the scale of operations by the denominator, $S$. Before being fitted to the data, equations (3) and (4) are simplified into the form:

$$\Delta H = a_0 + c_1 S_{-1} + c_2 \Delta O_{-1} - a_1 H_{-2} + u \tag{5}$$

where $c_1 = a_1 b_0$ and $c_2 = a_1 b_1$.

The results of fitting equation (5) to quarterly data for manufacturers are shown as Regression II in Table 1. Inspection of these measures shows that the coefficients are all of the expected sign and are 6 times their standard errors or better. $\bar{R}$ is increased to a value of .945 as compared with its value in Regression I, implying that the regression hyperplane accounts for 89 per cent of the variation in $\Delta H$. The Von Neumann statistic, $\delta^2/s^2 = 1.85$, has increased materially over that for the first regression. The coefficient values for Regression II imply the following equation defining shifts in $r$:

$$r = 1.953 + 1.529 \left(\frac{\Delta O}{S}\right)_{-1} \tag{6}$$

Thus, where new orders are equal to sales ($\Delta O$ = zero), the target ratio of stocks to sales is 1.953, as is shown also in the last colunm of Table 1. The ratio rises, however, when new orders exceed sales, and conversely, in accordance with the second term of equation (6) above.[7]

Computed values of inventory investment based on the Regression II equation are plotted as a line graph in section D of Chart 1, where it will be observed that these estimates conform very closely to the variations in actual inventory investment.

Regression II, accounting for a high percentage of the variation in quarterly inventory investment over the 11¼ year period and providing support for the hypothesis that the target stock-sales ratio varies systematically over the cycle, stands as the basic finding of this study. Two of its shortcomings, however, need still to be pointed out. Although the constant term in this regression, −.387, is considerably smaller than in Regression I, it is still not very close to the expected zero value.

[7] *Cf.* Bassie [4, pp. 666-70]. Bassie's regressions, which are based on annual data, include a rate-of-change-of-sales variable for the current period (which is data not available to the firm when inventory decisions are made). My use of $\Delta O_{-1}$ as a factor influencing the ratio, $r$, seems to me a more reasonable explanation and it yields a closer fit.

Second, the implied value of 1.953 for the target stock-sales ratio (when $\Delta O$ = zero) appears somewhat high when compared with the plot of actual ratios in section C of Chart 1. Investigation of the partial relationships has shown that both of these aspects of Regression II are the consequences of the very large differences between computed and actual values of inventory investment during the second and third quarters of 1949 (see Chart 1). Whatever factors caused the unusually sharp contraction in investment during these two quarters they are not recognized by the Regression II equation.[8] In any case it is of interest to ask what the regression results would have been in the absence of these factors. In Table 1, the results of a regression based on equation (5) but omitting the second and third quarters of 1949 are shown as Regression III.

The measures for Regression III shown in Table 1 indicate a very satisfactory fit. With the constant term, −.039, substantially equal to zero, the expectation of finding a homogeneous function is fulfilled. As shown in the table, the target stock-sales ratio (when $\Delta O$ = zero) implied by the regression coefficients is 1.89 which seems not unreasonable when compared with the actual values of the inventory-sales ratio plotted in Chart 1. A strikingly high order of correlation is shown by $\bar{R} = .953$ which implies that the regression "explains" approximately 91 per cent of the variation in inventory investment.

Finally, in order to test whether the structure of relationships remained stable during the period, a comparison of the last half of the period with the full period is provided by Regression IV which covers the 23 quarters from the first quarter of 1953 through the third quarter of 1958.[9] The constant terms and coefficients of each of the independent variables for both Regressions III and IV (Table 1) are of the same approximate order, their differences lying within a range that might be caused by chance error. The implied values for $r$ (when $\Delta O$ = zero) are both 1.89, and both regressions explain about the same percentage of the total variation in inventory investment.

[8] To account for this nontypical contraction Bassie points to forebodings by businessmen that a major readjustment would follow the postwar splurge of consumer spending [4, p. 212]. Their sentiments were, it turned out, unduly pessimistic but they may well have been a principal factor leading to the unusually large run-off of inventories during the second and third quarters of 1949.

[9] For the first half of the period (third quarter 1947 through fourth quarter 1952, omitting the second and third quarters of 1949), the regression equation is:

$$\Delta H = -.508 + .445S_{-1} - .222H_{-1} + .223\,\Delta O_{-1}$$

A study of scatter diagrams of the partial relationships shows that this regression's parameters are heavily influenced by two extreme values for $\Delta O_{-1}$ (fourth quarter of 1950 and second quarter of 1951 of the Korean war period). This regression is omitted from Table 1 as not being very significant for this reason. These two values do not affect so strongly the partial relationships for the full period (Regression III) because the means of $S$ and of $H$ are much larger and approximately twice as many observations are included.

## II. *Significance of the Findings*

The bearing of the foregoing findings on the three problems cited in the introduction remains to be assessed. These three problems, it will be recalled, concern the feasibility of aggregation in inventory analysis, the variability of the target stock-sales ratio, and the stability of economic structures as it affects inventory behavior.

1. *Aggregation.* Based on the aggregates, total inventories, sales, and new orders for all manufacturers, Regressions II, III, and IV yield correlation coefficients of the order of .95. This degree of closeness of fit, which is quite striking for regressions involving first differences of time series, clearly indicates the presence of a strong central tendency in the relationships among the aggregated variables. It appears that aggregation may be pushed much further in inventory analysis than has heretofore been generally felt justified.

Why is this the case? Although a comprehensive reconcilement of this conclusion with recent findings of considerable diversity in the management of inventory components cannot be attempted here, several lines of possible justification for the aggregation of inventory data may be offered as clues. Pointing to the three basic reasons for holding inventories—to take advantage of optimum lot-size, to permit the smoothing of production in the face of fluctuating sales, and to reduce risks through the holding of buffer stocks—Modigliani [10] has shown, in the first place, that a rational adaptation to these three requirements is equivalent to behavior aimed at maintaining an optimum relation of stocks to sales. Even though some firms may not calculate in these equivalent terms, on rational grounds *they will act as though they do.* As long, therefore, as the composition of the universe of firms, risks, seasonal fluctuations, and cost structures remains fairly stable, a central tendency with regard to the relation of stocks to sales will manifest itself in the aggregated data.

A second clue to explain why aggregation in this area appears to be justified concerns the financial aspects of individual business firms. It is likely that an aggregate of purchased materials, goods-in-process, and finished goods, will exhibit more stable behavior (relative to sales) than will each component considered separately because it is the total inventory that must conform to the constraint of available working-capital funds. Thus, a sudden run-off of finished goods due, say, to a sudden rise in sales releases working capital and *permits* the financing out of working-capital funds of a compensating addition to, say, purchased materials. Conversely, a piling-up of finished goods when sales unexpectedly fall off will force the firm, to some extent at least, to conserve its working capital by reducing other components of stocks,

quite apart from what it might do on other grounds. A financial practice of planning working capital needs as a function of sales may, then, explain in some degree why total stocks show a strong central tendency in their relation to sales. (These financial constraints, furthermore, provide a part of the justification for the measurement of the variables in current dollar units rather than in deflated form, as noted in Section I.)

In the third place, even within the limited field of manufacturing, many outputs of one firm become the inputs for another. To the extent that such interfirm purchases and sales occur within the manufacturing sector, greater stability (in relation to sales) may be expected for aggregate manufacturers' stocks than for the inventory, or components of inventory, of an individual firm. To illustrate, even though one firm may perceive a fall in general business activity earlier than another firm, and cuts back its stocks of purchased materials very quickly in relation to the fall in sales, the sudden cancellation of orders may result in a piling up of finished goods stocks in the hands of its suppliers. Within the aggregate what is a minus for one firm may be a plus for another, so that some canceling out of the inventory effects of diverse, nonhomogeneous behavior may be expected.[10]

2. *The Target Stock-Sales Ratio.* When the target ratio of inventory to sales, $r$, is permitted to fluctuate in accordance with the relationship of new orders to sales, a significant improvement in the multiple correlation coefficient occurs. The hypothesis that the desired ratio of stocks to sales is not constant over the cycle is supported by this finding.[11] Whether the more comprehensive hypothesis is true—that the ratio's systematic rise and fall over the cycle is the consequence of variations in $\Delta O$—is less certain, even though it is consistent with the data and seems reasonable.[12] The possibility that $\Delta O$ is itself correlated with a third variable which is the true determinant of $r$ has not been excluded.

3. *Stability of Economic Structure.* The regressions of Part I do not permit one to reach a strong conclusion regarding the stability over time of the economic structure surrounding inventory decisions. A compari-

[10] As Bassie points out [3, pp. 39-41], this aspect provides some justification for the aggregation of all business inventories, not just those for manufacturers, when studying the relationship of inventory changes and business activity.

[11] The possibility remains, however, that at least a part of the improvement in the multiple correlation coefficient (when a variable ratio, $r$, is employed) may occur because variations in $r$ provide a purely mechanical offset to systematic divergences between actual and intended inventory investment. See footnote 2 above.

[12] Shifts in $\Delta O$ may measure by proxy more fundamental changes. For example, since the data used in this study are stated in current dollars an excess of new orders over sales may signifiy to the firm a future rise in both prices of materials and quantities of shipments, both of these factors raising the desired ratio of stocks to sales.

son of the parameters of Regression IV, covering the last half of the period studied, with those of Regression III, based on the full period excepting two quarters in 1949, shows that they are of the same general order. Although this result is consistent with the view that structural changes over a period of about a decade do not materially alter regression parameters, the test on which it depends is a weak one. The time periods studied are very short. Two observations (in 1949) were excluded from one of the regressions, and no comparison of the second half of the period directly with the first half seemed feasible in view of certain extreme values for $\Delta O$ during the Korean war period.

Since completion of the regressions reported in Table 1, additional data has become available permitting a test of the form and parameter stability of regression equations II, III, and IV, by assessing their predictions of inventory change for three recent quarters (fourth quarter of 1958 through the second quarter of 1959). A comparison of predicted values with the actual change in seasonally adjusted manufacturers' inventories at book value is shown in Table 2. All three regression equations predict the slight decumulation of inventories during the fourth quarter of 1958 with only very small errors ranging from .05 to .08 (in billions of dollars). For the first quarter of 1959, the equations predict accumulations in the range of .66 to .71 whereas actual increases in inventories amounted to 1.25. For this quarter the errors are quite sizable ranging from .54 to .59. During the second quarter, finally, inventory investment of from 1.27 to 1.32 is predicted by the equations, with actual inventory accumulation aggregating 1.63, yielding errors in the range of .31 to .36. The parameter differences among the three equations produce only relatively small differences in the predictions.

The results of this prediction test do not appear to be inconsistent with the hypothesis that the parameters of the equations are sufficiently stable to be useful in short-run forecasting. The prediction for the fourth quarter of 1958 is very accurate. The shift from decumulation to accumulation in the first and second quarters of 1959 was successfully predicted by the equations. With regard to the excess of actual over predicted inventory investment for the first two quarters of 1959, on the other hand, a substantial part is probably attributable to precautionary buying of steel and copper in anticipation of work stoppages at mid-year. But because these predictions cover only three quarters of a year it does not seem wise to go beyond the statement that the hypothesis of reasonable parameter stability is not refuted by the prediction results. In time these functions may be tested more adequately by a longer series of forecasts than those shown in Table 2.

It should be clear from the tentative character of the foregoing comments that this paper reports progress, not final conclusions. Much more research in this area needs to be done. To check the soundness of the arguments advanced to justify the highly aggregative procedures used in this investigation, for one example, it would be useful to test functional relations covering each such inventory component as

TABLE 2—COMPARISON OF ACTUAL CHANGE IN MANUFACTURERS' INVENTORIES WITH AMOUNTS PREDICTED BY REGRESSION EQUATIONS II, III, AND IV—FOR FOURTH QUARTER OF 1958 AND FIRST TWO QUARTERS OF 1959
(all figures in billions of dollars)

| Prediction Based on Equation of: | Fourth Quarter, 1958 | | | First Quarter, 1959 | | | Second Quarter, 1959 | | |
|---|---|---|---|---|---|---|---|---|---|
| | Predicted | Actual | Difference | Predicted | Actual | Difference | Predicted | Actual | Difference |
| Regression II | −.170 | −.090 | +.080 | +.710 | +1.250 | +.540 | +1.310 | +1.630 | +.320 |
| Regression III | −.170 | −.090 | +.080 | +.660 | +1.250 | +.590 | +1.320 | +1.630 | +.310 |
| Regression IV | −.140 | −.090 | +.050 | +.690 | +1.250 | +.560 | +1.270 | +1.630 | +.360 |

*Note:* All figures based on seasonally adjusted inventories at book value.
*Source of data:* Current issues of *Survey of Current Business.*

purchased materials, goods-in-process, and finished goods, as well as further break-downs such as between goods produced on order and those produced for market. As soon as better procedures and data are available for an accurate deflation of monthly inventories, it would be desirable also to rerun such regressions as those of Section I to test empirically the assumption underlying this report that price-level influences do not seriously impair regression results based on current-value data.

In spite of these reservations, however, the closeness of fit of the "stock-flow" functions used in this study strongly supports the view that the acceleration principle is a powerful tool of economic analysis.

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# PLANNING WITH MATERIAL BALANCES IN SOVIET-TYPE ECONOMIES

*By* J. M. MONTIAS*

The material balance is at the core of Soviet planning; it is the most operational (or bureaucratic) of all balances in the sense that all its elements—output orders, import and export quotas, inventory changes and allotments of materials to various consuming groups—hang on administrative decisions.

Until recently so little was known about the practice of central planning in the Soviet Union and in Eastern Europe that material balances and other "balanced estimates" meshing the supply and demand for commodities and for factors of production were rarely discussed in Western economic literature.[1] But of late Soviet and Czech publications have become more candid about planning methods actually in use; while of course the Poles and the Hungarians, ever since 1955, have been less reticent than any other members of the Soviet bloc to expose their economic system to public scrutiny.

The structure of planning and of administrative organs differs in these various countries, and it has undergone a substantial reform since 1957 in the Soviet Union itself. Yet the task of elaborating a set of consistent balances presents the same basic problems in every centralized economy despite these differences: regional or industrial organs of the bureaucratic apparatus, whether higher or lower, are called upon to make economic decisions, the coordination of which, in the absence of markets and of a flexible price system,[2] must be effected by matching the total demand for every major resource against its available supply

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[1] A notable exception is a recently published book by Hans Hirsch [20]. Useful information on the subject is also to be found in Granick [15] and Grossman [16]. H. S. Levine's highly competent paper [42] was published while this article was in the press.

[2] Soviet economists are eager to mend their price system to improve economic accounting, but they have no intention at the present time of allowing prices to play a major role in allocating resources. After three years of lively debate, the Poles now seem to have reconciled themselves to the primacy of physical planning.

at the highest level of planning. This is the role of the U.S.S.R. *Gosplan* and of the Planning Commissions of the other Soviet bloc countries.[3]

The first section of this paper briefly describes the administrative framework of central planning in Soviet-type economies. In the second, I consider various procedures open to the central planners for approximating perfect consistency in building up a large set of interlocking balances. These theoretical alternatives are then compared with the information available about actual planning methods in the Soviet Union and in the other countries of the Soviet bloc. In the last two sections, the practical limitations of planning by the method of balances are brought out in detail on the basis of recent evidence taken from these economies.

## I. *The Administrative Framework*

Material balances may be drawn up for short-run planning purposes (for a quarter or for a year) or as the foundation for the long-run plans (five years and upwards). The latter, called "perspective balances," are not so operational as the short-term balances, since the long-term-plan output figures are subject to change and the requirements for materials (the outlay side of the balance) are only round estimates. The long-term plans, however, do mark out the course along which the socialized sector of the economy will travel, and they tend to hold good, barring major changes in policy, as long as no really troublesome obstacles supervene. However, "shortfalls" due to forecasting errors on the credit or on the debit side of perspective balances may have repercussions on the yearly balances, which can only be "closed" by cutting the output of certain branches of the economy short of their productive capacity.

The following procedure illustrates how the yearly plans for critical ("funded") products[4] were elaborated in the Soviet Union and in Poland, Czechoslovakia and Hungary before various—and divergent—reforms were put into effect in 1957 and 1958.

Some six to eight months before the beginning of the plan-year, the Planning Commission prepared preliminary balances of essential materials, taking into account the latest production figures as well as forecasts of productive capacity and labor force. Tentative targets ("con-

[3] As of 1958, *Gosplan* prepared balances for ferrous and nonferrous metals, fuels, oil products, electric energy, chemicals, lumber and building materials, the main types of machinery and equipment, agricultural raw materials and the chief products of the light and food-processing industries [22, p. 14].

[4] "Funded" products were essential materials (from planned or from above-plan production of socialized enterprises), which could be distributed solely by the Council of Ministers of the U.S.S.R. The most widely used raw materials and semifabricates were still funded in 1958.

trol figures") based on these balances were handed down to the various industrial ministries, which subdivided them among their Chief Industrial Administrations (*glavki*). Each *glavk* in turn set specific targets for its subordinate enterprises, which were then expected to calculate the material inputs they would require to hit these production targets. The enterprise's material requirements, written up in formal applications (*zaiavki*), were transmitted to its *glavk* and eventually to the procurement organization of its ministry. At every echelon, all applications were checked and their approved version consolidated with other applications. Each authority made sure that the material requirements corresponded both with the latest available output figures and with the established "technical-progressive norms" regulating the maximum permissible expenditure of materials per unit of output.[5]

The ministry finally turned over its procurement plan (covering the material requirements of all its enterprises) to the Council of Ministers and to *Gosplan*. Simultaneously with this process, ministries, with the help of their marketing organizations and of their *glavki*, drafted more detailed production plans, modifying and complementing the control figures they had previously received. These plans were also submitted to *Gosplan*, whose specialized industrial departments were now charged with preparing material balances for funded commodities on the basis of these latest production and procurement data. The process of concurrent adjustment of the supply and demand for each balanced commodity ended with the "closing" of the material balance—when the sum total of allocations earmarked for the various consuming groups matched the total supply from all sources planned for the year. Before all balances could simultaneously be closed, it was often necessary to go through most of this administrative procedure a number of times, on each occasion the relevant department of *Gosplan* or the ministry being expected to figure out the input requirements of subordinate enterprises from the output targets in the latest "version" of the yearly plan.[6]

Once all the material balances had been closed and had received approval by the Council of Ministers, each ministry subdivided its global allotment of material inputs among subordinate enterprises [11, p. 8]. The latter, within their appointed portions, communicated their exact requirements by size, type or make of the material to procurement agencies at various administrative levels. The detailed require-

[5] These norms or coefficients of output are fixed by special commissions for each enterprise. They must, in principle, be rigorous enough to guarantee that the enterprise will only maintain itself within their limits by dint of the most stringent economy. See below, pp. 977-79.

[6] In the later stages of planning, the input requirements of new output targets were usually calculated within the Planning Commission itself. (For certain limitations and exceptions to this basic scheme, see below, pp. 977-79.)

ments were eventually transmitted to the marketing organization of the supplying ministry, which supervised the transfer of goods from selling to purchasing enterprises.

In 1957, as part of the reorganization of Soviet planning on territorial principles, most industrial ministries were liquidated and a hundred-odd regional Economic Councils (*Sovnarkhozy*) were charged with the concrete supervision of 18,000 enterprises, accounting for the bulk of industrial output [25, p. 56]. Production and procurement plans instead of shuttling back and forth between *Gosplan* of the U.S.S.R. and the enterprise along functional-ministerial lines were now made to travel along territorial lines—through the *Gosplan* of each constituent Republic down to the regional Economic Council. Transmission channels are now different but the procedure for building up and controlling procurement plans is still essentially the same as it was prior to the reform [23, p. 325].

Some Soviet economists argue that the reorganization will make it easier to coordinate plans from the center: *Gosplan* of the U.S.S.R. will work on fewer balances (800 to 1000 in all). This reduction will be effected by grouping together many commodities formerly balanced separately. These more aggregated balances are supposed to account for a more complete coverage of industrial production than in the past, when nonfunded commodities were planned by ministries and their output was imperfectly coordinated with the rest [25, pp. 59-60]. In any case, it should now be possible to manipulate a smaller number of balances more flexibly and a more systematic effort can be made to reach over-all consistency.

## II. *Theoretical Models of Administrative Planning*

Let us first consider theoretically how all the balances can simultaneously be "closed" with the labor force and resources available, leaving aside for the moment the problem of limited plant capacity.

We assume that the material balances form an interlocking set which can be arranged as an input-output table.[7] Coefficients are now calculated relating the amount of each material input needed for producing a unit of each of the different outputs and ordered as a square matrix,

[7] In input-output terminology, our simplified balances look as follows:

*Resources* *Disposals*

$$x_i = \sum_{j=1}^{n} a_{ij}x_j + y_i$$

where $x_i$ is the gross output of the $i$th commodity, $a_{ij}$ is the technological coefficient showing the amount of $x_i$ required to produce every one of $n$ commodities, and $y_i$ is the final demand for the $i$th commodity. There are $n$ balances, one for each commodity.

A mathematical appendix to this section of the paper is obtainable from the author.

with as many rows and columns as there are balances. Since the balances are all expressed in physical units (e.g., tons of steel, thousands of tractors), the coefficients are "technological" (e.g., tons of coke per ton of pig iron), unlike the coefficients in a Leontief matrix which express the costs of input necessary to produce a dollar's worth of output.

Let us suppose that certain final demands are communicated by the authorities to the planners for investment goods and construction, defense, consumer goods, exports and reserves. The gross output target for each good necessary to fulfill its quota of final demand plus any amounts required by other industries as inputs can theoretically be calculated by input-output methods.[8] In recent years, the Russians and their more advanced partners (the Poles, the Czechs, the Hungarians and the East Germans) have studied the use of systematic methods of input-output programming, but their attempts to apply these techniques have hitherto been confined to small-scale (highly aggregated) pilot schemes.[9] Up to now, no system of interlocking balances for the national economic plan has been "harmonized," or made to "close" by these methods.

Soviet planners, who apparently believe that an internally consistent plan can be framed by traditional bureaucratic methods, given any feasible initial conditions of final demand or of minimum gross outputs, envisage the use of input-output methods mainly to work out the ultimate effects of different "variants" in these initial conditions. At present, the method of balances with its successive approximations to reach consistency, involves so cumbersome and time-consuming a procedure that an insufficient number of variants can be investigated, particularly in drafting long-range plans.[10]

But can the method of balances even lead to one consistent plan? The shuttling back and forth of targets, the groping toward a simul-

[8] In general, for this to be possible, the matrix formed by subtracting the technology matrix from the identity matrix must have an inverse. It is not certain that such an inverse will always exist, since the technological coefficients are supposed to be drawn, not from the actual performance of the economy, but from engineering data; and it may turn out, by a freak of chance, that the total inputs required to produce a given set of gross outputs with the prescribed technical processes exceed these outputs. The system would then "eat itself up"; no positive final bill of goods could be produced. It is only when the coefficients are calculated *after* the plan has been fulfilled, that we can be sure that the iteration process will be convergent. We may note in passing that if the technology matrix can be transformed into a Leontief matrix by a suitable change in units, and if every column of these new coefficients adds up to less than one, inversion will be possible. In this case, every product could be produced at zero or positive profit.

[9] Articles which describe some of the basic research being conducted in the Soviet-bloc countries are: for the Soviet Union, V. Belkin [3]; for Poland, K. Porwit [33] and E. Krzekowska and others [28]; for Czechoslovakia, A. Červený and J. Vácha [6]; for East Germany, H. Schöhen [38]; and for Hungary, A. Bródy [4].

[10] Interview material.

taneous "closing" of the balances, suggests various iterative techniques, which, systematically carried out, would, in effect, invert a matrix made up of the technological coefficients and approximate full consistency in the output plan. Our task now consists in finding the iterative technique which most closely resembles the administrative procedures actually followed in Soviet-type economies.

We start again with our somewhat unrealistic assumption, namely, that the first set of output targets circulated to all industries consists of final demands only. On the basis of these targets, all industries producing end-products calculate their input requirements, which are then summed for the whole economy, and added to the original targets of final demand to yield first estimates of gross output. These estimates make up a second set of targets which again are sent around to the various industries. Input requirements are now revised upward in line with the higher targets. The old summation procedure is then repeated to yield second estimates of gross outputs. It can be shown that every new set of gross-output targets obtained in this manner would come closer to the perfectly consistent set of targets that could be calculated by direct matrix inversion.[11]

An immediate improvement in this method suggests itself. Final demands are usually not a good starting point for the first iteration, especially if they represent only a small proportion of total production; therefore, gross-output targets adapted from the five-year plan or adjusted upward from last year's targets may be used, instead, to compute requirements of intermediate products in the first round. If these preliminary targets (the "control figures" of Soviet practice) are not too far out of line with each other, the first iteration will yield larger, more accurate estimates of the "correct" (consistent) output targets than by starting with final demand. The successive approximations starting from this arbitrary point should converge toward the same consistent set of gross outputs as before [13, p. 69].

[11] Letting $x_i^{(1)}$ stand for the first estimate of any gross output, we may use the terminology of note 7 to represent the initial phase:

$$x_i^{(1)} = \sum_{j=1}^{n} a_{ij} y_j + y_i \quad (1)$$

The second set of targets follows by repeating this procedure. Thus for the *i*th sector:

$$x_i^{(2)} = \sum_{j=1}^{n} a_{ij} x_j^{(1)} + y_i \quad (2)$$

This expression can be expanded by substituting (1) into (2). Each subsequent set of targets can be derived from the preceding set and expanded in the above manner. The iterative technique described here is equivalent to the inversion of a Leontief-type matrix (formed by subtracting the technological coefficients from an identity matrix) by a power series. It is assumed of course that this matrix has an inverse, otherwise iteration will fail to converge toward *any* set of targets [13, p. 63].

Are there any other shortcuts which would pare down the time required to reach an acceptable approximation? At least three come to mind: (1) aggregation of coefficients, (2) extrapolation toward more exact estimates, and (3) more "efficient" routing of the control figures throughout the administrative system.

1. The principal advantage of aggregation lies in the possibility of restricting the iteration process to a smaller number of planning organs and of speeding up the entire procedure. In Poland, for example, it is known that the last stages of planning a yearly program are all carried out within the Planning Commission itself, with the various departments of material balances (e.g., for metallurgy or for light industries) grinding out input requirements on the basis of aggregated coefficients [33, p. 338]. In this way the material requirements needed to produce the various estimates of the gross output targets can be worked out much more rapidly—though less exactly—than if these estimates had to filter all the way down to the producing plants.

Aggregation entails a loss in accuracy, unless the input-output coefficients of every product in each consolidated group happen to be the same or unless the relative outputs of the individual commodities to be aggregated are expected to remain in exactly the same proportions for all variations in the final program [29, pp. 195-200]. American experience with inverting large matrices indicates that the loss of accuracy due to this cause is not so damaging as might at first blush be supposed [30]. In practice, as we shall see in the next section, the worst losses due to aggregation probably stem from errors and omissions in the aggregation process itself.

2. Extrapolation: if the iteration process converges, then the differences between successive estimates of the gross outputs will tend to fall according to a constant ratio.

Let this ratio be called $K$. Then the planners could reach forward from, say, the third iteration to an ultimate approximation of the correct values of the gross outputs by computing the increase in the gross output of any good generated by the third iteration (compared to the second), multiplying this increase by the ratio of $K$ to $1 - K$, and adding the result to the output of the third iteration for this good [13, p. 74].[12] No such formal methods are used in Soviet planning, although the planners do seem to anticipate feedbacks by using their past experience and their planning "instinct" to extrapolate the estimates of material requirements on the outlay side of their balances.[13]

[12] Experience with the inversion of U.S. input-output matrices has shown that four to five iterations *cum* extrapolation were sufficient to encompass at least 96 per cent of the true value of the gross outputs.

[13] Interview material.

3. In this theoretical model of administrative planning, inefficient use has been made of the data generated at each step in the process. All the agencies and firms have computed their material needs from estimates of gross outputs corresponding to the *same* stage in the iteration process. It will usually be more advantageous to start the iteration process with industries producing mainly finished products and work back toward raw materials, making use at each stage of the gross outputs already generated. The advantages of this method are illustrated by the following example.

Let all industries be classified into three groups: (1) manufactures, (2) semifabricates, and (3) raw materials. Suppose that the last two groups drew no inputs from any manufacturing industry, while raw material industries consumed neither manufactured nor semifabricated products. The matrix of input-output coefficients for these three groups would then be free of all "circular relations" between groups.[14] The Planning Commission might be instructed first to calculate all the internal needs of the manufacturing group, add these needs to the final demand for manufactures and fix gross output targets for this group;[15] then use this target (together with the final demand for the second group) to work out the total needs for semifabricates. And, in the third stage, use both the gross-output targets for groups 1 and 2 to derive the target for group 3. These three steps would be sufficient to provide the planners with a consistent plan for all three sectors. No further interations would be necessary. The above procedure, which corresponds formally to the Gauss-Seidel method for finding the inverse of a matrix by iteration, would obviously save much time, provided that the technology matrix lent itself to its employment.

In practice, according to a Soviet economist interviewed, work on the balances starts simultaneously from "both ends": balances are first prepared for industries turning out mainly finished products and for industries producing mainly raw materials. Inconsistencies come to light somewhere "in the middle" as balances of intermediate products are reconciled with both raw-material availabilities and final demand re-

[14] Letting $a_{ij}$ $(i, j = 1 \text{ to } 3)$ stand for an aggregated coefficient relating any two groups, the matrix of these three industries could be arranged as follows:

$$\begin{bmatrix} a_{11} & 0 & 0 \\ a_{21} & a_{22} & 0 \\ a_{31} & a_{32} & a_{33} \end{bmatrix}$$

A positive value for any coefficient above the diagonal of the matrix means that two or more groups are purchasing inputs from each other ("circular relation").

[15] Note that the requirements of any group for its own products ($a_{ii}$) add a complication. The gross output target for the first group should theoretically equal the final demand for this group divided by $1 - a_{ii}$. But if the $a_{ii}$ were small, the error involved in ignoring their higher powers would be negligible.

quirements. This procedure probably reaps only a fraction of the computational savings of the Gauss-Seidel method.

In order to test roughly the effect of calculating gross outputs from final demands in a few iterations, the Gauss-Seidel method was tried out on the RAND Corporation's twenty-sector input-output matrix of the Soviet economy for 1941 [21] (rearranged so as to minimize the total value of above-diagonal elements).[16] In one complete iteration, approximately 84 per cent of the known value of the total of gross outputs was derived from the final bill of goods. Another iteration left 9 per cent unaccounted for. However, certain sectors with a small percentage of final demand to gross outputs—such as coal, peat, ferrous and nonferrous metallurgy—were still underestimated by as much as 30 or 40 per cent. By adjusting for the heavy internal requirements of these sectors,[17] and by limiting the next Gauss-Seidel iteration to just those sectors (using values already obtained for the remaining sectors), all sectors can be brought up to a minimum 85-90 per cent of the correct outputs. In practice, if sufficient reserves and inventories were available, and if the final demand for exports and for low-priority sectors offered a modicum of flexibility, the results might already be held satisfactory for operational purposes.

There is a strong presumption that better estimates could be made after two iterations if control figures of gross output were used in the first iteration, since some of the final demands amounted to only a fraction of gross output (e.g., 6 per cent in the coal industry).

In the long run, however, the case for iterative planning is not so strong. As a country develops, its products normally become more fabricated. Fewer products go directly from the mine or from the field to the ultimate consumer. The proportion of final demand in total output may be expected to fall. Once the industrial structure has become more complex, it should take more iterations to reach acceptable estimates of gross output whether we start from final demand or from any arbitrary starting point [13, p. 83]. In addition, relations among industries consuming each other's products (directly or indirectly) are more likely to develop with time. Indeed, such industries as chemicals, paper products, ferrous and nonferrous metals, which account for a large share of the circular relations, tend to grow more rapidly than the rest and assume increasing importance in more developed countries [8, p. 497]. These circular relations will have the effect of slowing down the convergence process for any method of iteration. In so far as

[16] The above-diagonal elements were reduced in a few operations to 2.5 per cent of the sum of gross outputs. These elements could be brought down further by more trial- and-error reshuffling of rows and columns.

[17] See note 15 above.

the Russian economy conforms to these general trends in development, the Soviet planners are likely to get larger errors than in the past (for a given number of iterations). This in itself may help explain the recent flurry of Soviet interest in the application of formal input-output techniques to planning problems.

Up to this point we have assumed that gross output targets could be systematically derived from a neat bundle of final demands.[18] But it has been suggested by at least one observer of the Soviet economy that certain intermediate products might belong to a higher order of priorities than the end-uses they can generate.[19]

Take for example, the "leading links" of Soviet planning, sectors of the economy that were given top priority in order to widen bottlenecks in development. The yearly increases in the output or services planned for these sectors were not determined directly by their net contributions to the investment program, to defense or to private consumption; neither did they follow automatically from the nicely calculated requirements of other sectors for their products. The planning of these leading links had a profound impact on the entire program for the year—which may in certain cases have overridden planners' preferences for end-products.

Still this objection cannot be pushed too far: the ultimate decision-makers need not have a sharply etched preference map to tell whether they wish to see more tanks produced or more automobiles, whether they plan more investments in electric power or in dairy production; and the choice of gross-output targets does have a decisive effect on whether tanks or automobiles will be produced this year, and electric light or milk five years hence. The planners must eventually adjust gross-output targets to any strong preferences they may have for end-products. Moreover, as the quality of long-range planning improves and flagrant disproportions in the economy are gradually eliminated, hectic campaigns to build up top-priority sectors "at all costs" should eventually yield to a more comprehensive approach to planning problems.

Capacity limitations in certain key industries also contradict the basic assumption of one-factor scarcity implicit in our iterative solution of an input-output program. It may actually wreck all our theorizing, for, strictly speaking, no program saddled with both capacity

[18] The use of "control figures" as initial estimates of gross outputs, instead of final-demand requirements is only a convenient device to accelerate convergence. In the last analysis final demand still determines gross outputs and not the other way around.

[19] In the Soviet Union, according to Gregory Grossman "production targets for certain key intermediate goods are set by political decision. . . . These are . . . the goods which symbolize military-economic power and independence in the mind of the regime: e.g. coal, petroleum, electric power, steel, etc. . . ." [16, p. 102].

limitations and a limited supply of labor is susceptible of solution by input-output methods; and linear programming, which can handle this type of problems, has no counterpart in the planning of Soviet economies. Fortunately, only one not-too-far-fetched assumption needs to be made to bring us back into charted territory, namely, that the entire capacity of every capacity-limited industry should be used up. This implies that the planners will have to be content with just those quantities of net output in the capacity-limited industries that will be left over after satisfying the internal requirements of the system and the final demands prescribed for the remaining industries.[20]

Economists of the Polish Planning Commission have recently analyzed the problem of finding an input-output solution to such a program, in the belief that, for short-run planning, a "mixed model" (with capacity limitations) would better answer practical needs than one where output in every industry was determined solely by its supply of material inputs [34, pp. 11-30].

An approximate solution to a mixed program can be found by iteration with even less effort than in the case where no capacity limitations have been allowed for. The planners may proceed as follows: they may calculate by iteration the gross-output requirements of the industries whose output is limited by their supply of material inputs (introducing the gross outputs of the capacity-limited industries only as a source of demand for inputs from the industries under consideration) and, once acceptable estimates of these gross outputs have been ground out, use them to calculate the residual net outputs of the capacity-limited industries. The iteration process can therefore be confined to sectors with a predetermined final demand, while data from the limited-capacity sectors need only be introduced into the balances after all the time-consuming calculations on the remaining sectors have been completed.[21]

[20] That this is an assumption and not a necessity can be shown by the following example. Suppose industries A and B were capacity-limited and bought inputs from each other. Then the *net* output of A could be increased slightly by operating B below capacity, thus releasing some of A's intermediate products for final demand, which would otherwise be brought by B from A. This possibility is ruled out in the model that follows.

[21] We again use the terminology of notes 7 and 11 for a three-sector model. It is assumed that the output of sector 1 (the capacity-limited industry) cannot exceed $x_1$. Final demand is known for the remaining sectors ($y_2$ and $y_3$). Our unknowns are $y_1$ (residual final demand in sector 1) and gross outputs $x_2$ and $x_3$. We call $x_2^{(1)}$ and $x_3^{(1)}$ our first estimates of $x_2$ and $x_3$, equal to $(y_2 + a_{21} x_1)$ and $(y_3 + a_{31} x_1)$ respectively. The second estimates of $x_2$ equals the first estimate plus indirect requirements amounting to $(a_{22} x_2^{(1)} + a_{23} x_3^{(1)})$. This sum expands to $(1 + a_{22})\ (y_2 + a_{21} x_1) + a_{23}\ (y_3 + a_{31} x_1)$. Further estimates can be obtained by repeating the same operations. Once acceptable estimates of the unknown outputs have been reached after $k$ iterations, the residual final demand of the first industry can be obtained as follows:

$$y_1 = (1 - a_{11})\ x_1 - a_{12}\ x_2^{(k)} - a_{13}\ x_3^{(k)}$$

where $x_2^{(k)}$ and $x_3^{(k)}$ are the $k$th estimates of $x_2$ and $x_3$.

To conclude the foregoing analysis: the method of material balances is not inherently wasteful or theoretically unsound. It *may* lead to full consistency if the iteration process is carried on long enough and if the technical coefficients are accurate. Even if we knew that only one or two iterations were carried out, we still could not be sure that the method would lead to large errors; for the organization of planning and the nature of the technology matrix might be such that substantially correct estimates of gross outputs could be derived by its use.

Finally, attention should be drawn to the essential flexibility of iterative procedures. The technological coefficients need not all be transmitted to the Planning Commission. A "control figure," or preliminary output target, may be sent to a ministry, to a *glavk* or to a regional Economic Council, depending on how industry is organized and on where it may be convenient to calculate the inputs to hit this target. The working out of input needs at lower echelons saves on the time and expense and on the errors in aggregation involved in bringing all the coefficients together in one place. It is conceivable that these savings might more than offset the increased cost of transmitting input and output estimates back and forth between the center and the peripheries.

A Yugoslav economist and former central planner has pointed out that trial-and-error methods for reaching consistency permit more *ad hoc* adjustments in the coefficients than the mechanical inversion of a matrix [31, p. 215]. Suppose, for example, that an increased final demand for aluminum hollowware and agricultural machinery has given rise to larger electric-power requirements than were originally planned. The full extent of the discrepancy has perhaps come to the attention of the planners only in the third "version" of the program. The extra power must come from inefficient generating capacity with a high coal intake. The average coal-electricity coefficient can now be raised to take these new circumstances into account. And there is no need to start the whole programming over again—as long as coal output can be raised to the new required level.

Similarly, if the balances for certain key materials cannot be closed, substitution of other materials in more abundant supply may be enjoined on all producers by the Planning Commission, and corresponding adjustments in the technological matrix and in the production program may be made for further iterations.

### III. *The Models Compared with Actual Planning Methods*

In the models of the preceding section, a set of interlocking balances has been posited for all commodities: if aggregation had to be resorted to, it was not supposed to affect the interdependences linking all the commodities in the system. This assumption will not bear scrutiny: in

all the economies under study, a good many of the less important commodities were planned, balanced and distributed at lower administrative levels—sometimes even by the producers themselves, more often by marketing organizations.[22] These plans were poorly integrated, if at all, with the more fundamental balances elaborated in the Planning Commission and approved by the Council of Ministers. Furthermore, at all administrative stages, material requirements were consolidated along "organizational" lines (by ministries or lower organizations named as "custodians" of allotted funds) rather than according to the type of output which they would help to produce. Thus, prior to the reorganization of Soviet industry, total coal disposals in *Gosplan* balances were broken down by ministry, not by detailed use. (Only the principal commodities for which coal served as input, such as coke and steel, were typed out under each ministry in the Polish balances.) According to a Soviet statistician, though detailed data are collected from each producing enterprise on its consumption of materials (corresponding to the widest industrial classification in use), "this extremely valuable material is neither summarized nor processed" [35, p. 368]. It is evident, however, that the central planners must have at their disposal aggregated input-output norms for all commodities for which they wish to draw up a consistent plan by successive approximations.

In the opinion of Soviet experts, the organizational makeup of the balances also limits their use as a source of data for input-output programming; considerable expense would have to be incurred to fill the gaps by working up raw data furnished by enterprises.[23]

Summarizing some of these difficulties, the *Gosplan* economist A. N. Efimov complains that "material balances . . . for individual products, not being integrated into a single system, solve only limited problems of intersectoral proportions" [10, p. 108].[24]

[22] In 1954, out of 62 machinery groups or subgroups balanced by the Polish planners, 18 were balanced in the Planning Commission, 14 by the Central Agency for Machine Economy (C.Z.G.M.); 3 by departments of ministries, 9 by central boards, 4 by procurement organizations, and 14 by marketing organizations [32, p. 650]. Some, but not all, of those balances were integrated into summary balances drafted in the Planning Commission.

[23] The input-output team of the Polish Planning Commission has found an ingenious way of circumventing this difficulty by combining data based on "product-organizational" lines (e.g., tons of coal per million zlotys of output of the ministry of the chemical industry) with detailed physical output data supplied by the different ministries and their subordinate agencies [34, pp. 31-42]. The Czechs propose to develop a new industrial classification to make up their input-output tables and to collect the requisite data directly from plants for this purpose [6, pp. 339-41].

[24] Similar, but more detailed, criticism of the balances is to be found in the work of various Hungarian economists (for example [1, p. 568]). In a 1957 article, another Hungarian economist, A. Bródy, complained that statistical data available at present are not sufficient to prepare a detailed input-output table for industry as a whole. Differences in nomenclature among the various administrative organs are mostly to blame. No informa-

This failure is particularly evident when changes must be made in the last stages of drawing up the year's program. Efimov remarks that, in view of the great expenditure of computational labor needed to rework the material balances, as well as the lack of time for carrying out this work, "recalculations may be limited in practice to the balances directly affected by the changes." These he calls "first-order linkages" (e.g., the adjustment of the steel balance to an increase in the demand for trucks). "Balances related to the original change by second-order and especially by third- and fourth-order linkages are altered only where the changes are significant" [10, p. 107].[25]

Nevertheless, if I may judge from the Polish example, a determined effort *is* made to achieve consistency at least for priority sectors, even if the balances have to be closed by cutting down allotments to lower-priority sectors. Two plausible courses of action for resolving basic inconsistencies in the master-plan can be looked into. First, the planners may meet deficits in the balances by calling for a reduction in the input-output coefficients of heavy users of materials in short supply; second, the allocation of materials to consumer-goods industries may function as a buffer which softens the impact of shortages on the other key sectors. Both hypotheses are partially valid but neither holds up under all circumstances.

1. The input-output coefficients the planners work with are already strained. Only a hortatory purpose can be served by decreeing their further reduction:[26] firms will have to make shift with the materials actually dealt out to them; if they exert enough effort toward reducing unit costs, they may still hit their output targets with lower coefficients. But they may just as easily exceed their allowances—especially if the quality of materials rationed out is below standards. This approach will not go far toward eliminating the trouble.

2. The quotas of materials earmarked for household consumption are sometimes cut; but these quotas already tend to be niggardly, since the original bill of consumer goods—low in the planners' priority scale—

tion can be had on "products that are not easily measurable" (steel castings and forgings, some types of machines etc. . . .). Consequently, "one is unable to find out what happens to over half of the output of heavy industry" [4, pp. 138-39]. These, and all the other Hungarian references in this paper, were kindly made available to me by Bela Balassa from his forthcoming study on the economy of communist Hungary.

[25] For further comments of the same nature, see Belkin [3, p. 140] and Ausch's remarks on Hungarian planning [1, p. 568].

[26] In the stormy industrialization period of the early 1930's, systematic overcommitment of resources may well have helped to "mobilize the masses" toward more strenuous efforts to fulfill impossible plans. (*Cf.* the chapter "Problem Solving, the Overcommitment of Resources and 'Storming'," in [2].) In the 1950's, the Soviet economic system seems to have moved away from these *Sturm und Drang* methods toward a tighter budgeting of resources. This trend is important in assessing the present Soviet search for more rational planning procedures.

was already pared down to bare essentials. There is not much more "play" here than in the producer-goods sector.

It is interesting to note, in the case of Poland, that, from about the middle of 1954 on, the consumer-goods sector (of key industry) became the least resilient of all—after acting as one of the most elastic buffers in former years: an effort was being made at that time to regain the losses in real wages suffered in the first stages of the six-year plan, and if any element of demand had to "give" when shortages arose, it was industrial construction and other investment activities rather than any consumer-goods industry.[27]

A minor component of demand to feel the pinch in certain cases is the "pool" of materials going to producers' cooperatives and to private handicrafts. The beneficiaries of this pool—whether they produce consumer or producer goods—receive harsher treatment at the hands of the planning board than large-scale textile or food-processing plants by reason of their inferior social status and of the marginal nature of their deliveries in the total supply of most goods. It is apparently believed that ups and downs in the output of these dwarf producers (caused by their procurement difficulties) are less damaging than occasional cuts in the production of consumer staples by plants employing thousands of workers.

If exports are an essential ingredient needed for industrial growth—through the imports of raw materials that they make possible—then exports will be the last "end-use" to be cut. This was apparently the case in Hungary. However, in Poland, exports of raw materials and semifabricates (including coal and rolled zinc) were frequently trimmed when the requirements of domestic industry and transportation could not otherwise be satisfied.[28]

There is no set of rules binding for every time and place to deal with shortages in the balances as they arise: consumer goods, investment projects, exports, or even the defense program may bear the brunt of adjustments, depending on the priorities of the moment.

## IV. *Obstacles to Accurate Planning by the Method of Balances*

The routine obstacles that stand in the way of accurate planning by the method of balances may be divided into two groups: (1) failures

[27] This does not imply that the investment budget was greatly reduced—it was actually held at approximately the same absolute level as in former years—but it was thought expedient to maintain a certain level of output of consumer goods as an irreducible minimum, irrespective of the vagaries of planning.

[28] In the routine planning of material balances in Poland, the officials of the Planning Commission earmarked for export those quantities of goods that could be drawn off from the pool of domestic allotments "without prejudice to procurement needs at home." The quantities exported of many goods ended up as a resultant item, "left over after other needs had been met" [36, p. 25].

in the transmission to higher authorities of information about the production functions of individual producers, and (2) errors that come to light in the process of fulfilling the plan.

1. The knowledge which planners in *Gosplan* or the equivalent organization have of the relevant input-coefficients may be inaccurate for a number of reasons:

(a) Technical norms are often unrealistic to start with, since they usually assume a quality of material inputs and conditions of repair and maintenance of equipment which are not normally met in practice. They may also hinge on an above-average performance of the workers operating the materials-consuming equipment.

(b) Many norms at the plant level are ignored because the complexities of the production processes and the frequently changing specification of the products render them useless for planning purposes.[29]

(c) Due to rapid technical progress, it is not possible to keep all the norms up to date.

(d) The norms are often poorly aggregated even at the level of the enterprise—there are as many norms of coal utilization as there are furnaces and boilers in a plant (and there may be several plants in the enterprise)—but particularly, prior to the reorganization of Soviet industry, at the level of *glavk*. Only a few of the aggregated norms are systematically built up from their output-weighted components; in a majority of cases the planners make use of "statistical coefficients" relating inputs to outputs for a group of enterprises in the most recent planning period.[30]

(e) The relation between the output of a product and the consumption of materials for repairs and maintenance associated with its production cannot be predicted with accuracy. The quotas of materials earmarked for these uses are liable to a wide margin of error [37, pp. 61, 62].

(f) The output-mix of an industry is frequently so complex that materials needs can only be gauged in proportion to the gross value of output of the industry or of a group of products rather than for each of the products separately. In the Soviet Union, the material inputs of the construction industry are estimated as so many tons of bricks, cement or lumber per million rubles of construction. This is a highly unreliable method since the material-intensity of different stages of construction

[29] The complex factors bearing on fuel norms and the difficulty of working out usable technical-progressive norms are described in a Czech article [40, pp. 549-52].

[30] The technical difficulties of conveying usable information about production functions to the ministries and to the Planning Commission must be formidable when we consider that a single machine-building factory in the Urals turned over to its superiors 17,000 sheets of "documentation" relating to its norms and to its actual consumption of materials [14, p. 46].

varies appreciably [22, p. 11]. In Polish industry, only about 60-70 per cent of total output could be planned with physical coefficients; for the rest, materials needs were geared to gross output forecasts and were to some extent distorted by the irrational elements in the prices that served as output weights for the individual commodities aggregated.

Breakdowns in the transmission of operational information cause still another source of errors in the balancing process. The correct material quota on the expenditure side of a balance depends not only on the output of its consumers and on their coefficients but also on the inventories of materials and goods in process these consumers may have on hand. Unfortunately, accurate data on these inventories are hard to get, not only for technical reasons—constant fluctuations, poor accounting methods, and the like—but because firms find an advantage in concealing their materials hoards to be able to claim higher rations.[31]

The shortcomings of the norms might be alleviated if producing firms had a real voice in planning their inputs. But because firms tend to "plan upwards" (inflating their requirements in expectation of cuts) and because it takes too much time and trouble to bring every firm into the laborious planning process, the contribution of producers to the drafting of material balances is often perfunctory. This has evidently been the case in Poland since 1954 when "planning from above," as this short-cut method was termed, began to prevail.[32] In Russia, also, ministries did not always consult their subordinate enterprises in drafting their plans, particularly in industries with a highly diversified output.[33]

2. So many unforeseen contingencies may cause production and consumption plans to go awry in the course of their fulfillment that we can only list the major sources of disturbance.

(a) Changes in the demand for finished products ordered by the authorities during the year: political events—such as war scares—may precipitate short-run revisions in the investment program affecting the

[31] E. Devons [9] reports on the British experience with physical planning of aircraft production during the second world war, offering a vivid sketch of similar statistical deficiencies and of the mistakes they sometimes occasion. Indeed, a good part of the book might apply to planning in Soviet-type economies.

[32] [39, p. 379]. Before firms were cut off from detailed planning, they had to rework their industrial-financial technical plans as many as ten times a years in response to changes suggested by higher authorities. To reduce excessive "versionism," the instructions for the 1954 plan called for a maximum of independent planning by the Planning Commission and by the ministries—the first on the basis of the previous year's results, the second on the basis of technical data—with a final reconciliation of all projects in the last stage of planning [41, p. 6]. This arrangement seems to have been patterned after Soviet practice of the late 1930's [15, pp. 64-68].

[33] Eidel'man writes that "when ministries prepare their applications for materials without the participation of producing firms, they are subsequently compelled to introduce significant corrections in their applications" [11, p. 32]. For further details on Russia see [14, p. 48-49], and on Czechoslovakia [18, p. 59].

make-up of most material balances. If time is too short to elaborate a new consistent set of interlocking balances, then quotas earmarked for one purpose may be preempted for another without adjustment for the consequent indirect effects. This will upset the fulfillment of enterprise plans.[34]

(b) The time-lag between the drafting of the plans and operational decisions is such that, for the first quarter of the year, most firms operate without any approved plan at all.[35] In the Soviet Union, special "advances" of funded materials equivalent to the previous year's rate of consumption must be extended to consumers of materials pending receipt of final plans [22, p. 11].[36] During this hiatus, there is a minimum of coordination, since with a high, uneven rate of growth in the different sectors of the economy, the structure of the economy's output is constantly undergoing changes which will disturb the balances of last year's plans. Even if the previous year's plans were all perfectly balanced, a projection of these plans into the next year, disregarding the increased capacity and the new needs for end-uses that have come up in the meantime, could hardly be consistent.

(c) Producers frequently fail to notify consumers of production breakdowns and, when their deliveries must be postponed, they present their clients with a *fait accompli,* leaving them no chance to find alternative sources of supply.

(d) Above-plan output, rewarded by bonuses to management, usually necessitates extra consignments of material inputs, which must either be deflected from other consumers (by administrative fiat) or made good by unplanned output somewhere else. Unforeseen increases in materials requirements by priority consumers, from this and from other causes too numerous to mention, call for a myriad of lower-level decisions which may or may not be in harmony with the central plan or, for that matter, with each other.

(e) Certain industrial consumers are allotted supplies from "new production" of factories scheduled to be launched during the year. But it is difficult to fix an exact date for the start of full-scale operations in

[34] A Slovak economist, writing in the official organ of the Czechoslovak Planning Board, remarks that "the effects of foreign trade, changes in the demand of private consumers, technical progress, natural events, and defects in the elaboration of the plan militate against the absolute inflexibility of yearly plans . . ." [27, p. 34].

[35] In Poland, the ministries and the Planning Commission started work on the coordination of the plans as early as May and June of the year preceding the plan year. Most of the work of formulating an internally consistent plan was supposed to be over by mid-September [41, p. 9]. Yet producers only received concrete directives by the end of the first quarter of the plan year or, in 1953 for instance, as late as May [17, p. 30].

[36] Polish enterprises operated during the first quarter on the basis of tentative plans submitted to the Central Boards of Industry in the fall of the preceding year—which usually differed substantially from later versions [17, p. 30].

new projects. Consumers are therefore at the mercy of "bugs" in the production processes of their suppliers. Shortfalls in deliveries will of course upset their own output plans with repercussions in the rest of the economy. This element takes on special significance in the first periods of rapid industrialization when each new plant put into operation may contribute a large proportion of the nation's output of a particular commodity.

What do all these discrepancies amount to? According to *Pravda* (August 10, 1955), 31 to 40 per cent of all industrial plants in the Soviet Union failed to fulfill their annual plans between 1951 and 1954.[37] Even if a large number of plants produced over and above their plan, the chances are that differences in the location of the surplus-output plans and in the specifications of their products would make the latter somewhat less than perfectly substitutable for the products in deficient supply.

Disproportions in short-run planning are made good, in so far as possible, by the manipulation of reserves held by the planning board and by other agencies, or by fluctuations in the inventories maintained by producers and consumers. Inventories in the Soviet Union apparently have to be built up to higher levels than in capitalist economies such as the United States [7, pp. 561-65]. But they still are not sufficient to cushion all disturbances in procurement; and production breakdowns due to material shortages remain a perennial source of complaints in all the centralized economies.[38]

## V. *Concluding Remarks*

This paper confines itself to the purely technical aspects of drawing up a consistent set of interlocking balances of material resources. We have not related these balance sheets to the composite ("synthetic") balance sheets of money flows which are used, among other purposes, to equilibrate the aggregate supply and demand for consumer goods. Neither have we related the short-term balances to investment planning or to plant capacity. We have thus by-passed one essential function of the material balances: the detection of bottlenecks and their eventual elimination by suitable investments (in plant capacity, in equipment, or in the expansion of extractive industries, including geological prospecting).

[37] In Czechoslovakia, according to a detailed breakdown for 1957 published in 1958, only 15 per cent of 1,429 enterprises covered in a survey failed to fulfill their production plan [5, p. 51].

[38] These snags in procurement are chiefly responsible for the uneven rate of production in many plants (*nieritmitchnost*), which must slow down during certain periods while they wait for essential parts or materials and then must "storm" the plan in the last ten days of the month.

This technical survey also fails to explore the relation between our limited "ideal" of Soviet planning (the working out of a perfectly consistent program) and the static model for the efficient allocation of resources. In view of the fact that factor-mixes are endowed with *some* flexibility in many, if not in most, production processes, we might ask how the method of balances and administrative rationing are expected to reach an efficient allocation (where the ratios of marginal physical products of any two factors will be equated in every use). How can the most economical production processes be selected without any more refined knowledge of relative scarcities than the occurence of surpluses or deficits in trial-balances? Can an effective price system be grafted on physical planning and help to resolve these dilemmas?

It will appear to some readers that these questions need only to be raised in order to be answered.[39] But before we become complacently critical, let us bear in mind that static efficiency is not the be-all and end-all of the art of planning. The Soviet system with all its compulsion and waste is a vehicle for high rates of growth. To some extent, a higher rate of growth than might otherwise be feasible makes up for short-run inefficiencies. These speculative remarks aside, my conclusions are leveled at a lower plane.

Whether or not the method of balances is, at best, inherently wasteful, there is vast room for improvement before this "best" can be attained in the economies of the Soviet bloc. It is clear that achievement of the things the planners strive for, such as better technological norms, standardization, faster and more exact materials-accounting procedures at the plant level, and more efficient processing of data at intermediate levels, must help to overcome some of the present weaknesses of the planning process.

For the purpose of improving the coordination of the plans, the Soviets may have much to gain from the studies of input-output programming being conducted at present by their own theoreticians. Even if they do not find it practicable to invert a matrix of coefficients with electronic machines within *Gosplan*—possibly because not all the coefficients can be brought together in one place—they may learn from studying their *tableaux économiques* how to devise a scheme of administrative iteration which will invert the matrix without shedding along the way any of the significant interdependences that link together the different sectors of the economy. If, moreover, the Communist planners, by resort to some form of linear programming, can contrive to

[39] Walter Eucken, in his illuminating analysis of German wartime planning [12], supplied an unequivocal answer to our questions—"it cannot be done." Another West German economist, K. P. Hensel, argues that surpluses and deficits in the balances do give a sufficiently exact measure of relative scarcities [19, p. 134]. Hensel's contentions on this point are refuted by Hirsch [20, pp. 25-26].

find more economical input-mixes to achieve their goals, they will be tapping a new potential for increased power and growth.

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# COMPETITION AND GROWTH: THE LESSON OF WEST GERMANY

*By* Egon Sohmen*

Modern economists have not been inclined to deal kindly with the policies promoted by 19th century economic liberalism. Many of the theoretical refinements developed during the first half of the 20th century were prompted by persuasive empirical evidence that economies in the real world did not operate as smoothly as the deceptive harmony of the abstract model of general equilibrium under perfect competition suggested.

The criticism of *laissez-faire* policies can be classified under four broad headings. (1) The free play of market forces can lead to optimal resource allocation in the Paretian sense only if production and utility functions fulfill a number of apparently highly restrictive conditions [15] [3]. (2) Quite apart from these restrictions imposed by nature, as it were, the free pursuit of maximum private gain induces independent decision-making units to seek coordination of economic action and can lead to a vast variety of market imperfections. (3) A free-enterprise economy may fail to guarantee full employment of all usable resources. Although the causes of underemployment can always be traced back to phenomena of the kind listed under the first two headings, underutilization is sufficiently distinct from misallocation of fully employed resources to have prompted the creation of an entirely different set of analytical tools. (4) Even if all resources were fully and efficiently employed, the distribution of income among the members of the community would leave unsettled an additional dimension of political decisions which the market mechanism is constitutionally incapable of solving.

Awareness of this multitude of obstacles to the realization of an optimum solution through the free market, even when the price mechanism is modified and mollified by discretionary governmental action on various fronts, often results in a state of only thinly disguised bewilderment in an economist's mind when he is called upon to give concrete policy advice. Mere theorizing cannot hope to dispel the uncertainty that surrounds the issue whether or not a determined attempt to make an economy more competitive is worth the effort. It is therefore worth

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while to appraise the experience of countries whose governments have, for better or for worse, undertaken such an attempt.

The postwar reforms in West Germany stand out as one of the most ambitious efforts in this direction. The details of the reforms are known to U.S. readers from H. C. Wallich's comprehensive survey [34]. The most important measures were the currency reform of June 1948, the abolition of all but a small residual of rationing and price controls immediately after the currency reform, and the Allied ordinances of 1947 prohibiting cartel agreements and providing for the breakup of the largest combines in the German steel, coal and chemical industries and in commercial banking.[1]

## I. *West Germany's Recovery*

The West German index of industrial production indicates the rapid pace of economic growth. It rose by about 46 per cent in the six months following the monetary reform of 1948. The production index for 1949 stood at 143 per cent of 1948. The index increased by 26, 18, 7, 10, 12, 14, 8, 6, and 3 per cent in the following years.[2] It cannot be claimed that

[1] For details of the currency reform, see Wallich [34, Ch. 4]. The deconcentration laws are described in a British government publication [27, Ch. 4].

The objectives of the deconcentration measures have changed over time. The Allies regarded the large German combines as one of the hotbeds of Nazi Germany's aggressive foreign policy, and their breakup was originally intended to help prevent the recurrence of German aggression rather than to promote more efficient resource allocation. The same policies were later continued primarily with the latter aim in mind. Cynics observed that trust-busting was apparently such a fundamental principle of American occupation policy that it was equally applicable whether the intention was to strengthen or to weaken the German economy [34, p. 384].

An institution hardly conceivable for Americans was the licensing system found in most countries of prewar Central Europe, including Germany. The establishment of every independent enterprise, regardless of size, required a license that was granted only when the guild concerned, in other words, the applicant's potential competitors, felt that the entry of an additional competitor was "justified" by market conditions. The U.S. occupation authorities abolished this system in 1948 for the American sector. After intense lobbying by small-business pressure groups, a partial restoration of the old order took place in the early 1950's.

An interesting feature of the West German economy carried over from the Nazi era is government ownership of a few strategically important companies in the steel industry and scattered holdings in other sectors, such as the well-known Volkswagen works. Price leadership by a nationalized enterprise may serve as an important competitive element in an oligopolistic market, while the danger of sclerosis in industrial management by government bureaucrats is more easily avoided if the firm is embedded in a free-enterprise industry. The West German government is, however, in the process of divesting itself of most of its industrial holdings.

[2] [23]. The West German production index is based on value added and comprises approximately 60 per cent of the gross national product. The weights for the years up to 1950 are the prices of 1936. For later years, 1950 weights are used. Comparisons with prewar years always refer to the contribution of the present territory of the German Federal Republic. The reliability of the production figures for the years prior to 1950, and especially before 1948, is subject to doubt. During the first two quarters of 1959,

West Germany has, after the few postwar years of relative stagnation, merely been catching up with its neighbors since 1948. This argument is refuted by a comparison with prewar years. As early as 1951, West Germany had "caught up" with Britain in terms of the cumulative rate of increase since 1938 of the gross national product at constant prices. By 1956, this index had passed that of every other country in Western Europe.[3]

The growth rate of the Soviet economy appears not to have exceeded that of West Germany after the second world war. D. R. Hodgman's index of Soviet industrial production, computed on the basis of 1934 weights, shows an increase by 68 per cent from 1950 to 1955.[4] The official Soviet index claims a rise in industrial production by 85 per cent for this period.[5] The West German index of industrial production increased by 78 per cent from 1950 to 1955.

U.S. aid was an important factor in the German recovery. West Germany has, however, received a smaller amount of aid than the United Kingdom and France.[6] Dismantling took away part of what U.S. assistance gave during the first years after the end of the war.[7] Although West Germany did not have to support a military establishment of her own, her contributions toward the support of the Allied occupation forces did not absorb a materially smaller share of her national income

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industrial production was 4.8 per cent above that in the first two quarters of 1958 [24, July 1959, p. 339].

[3] [19, July 1958, p. 95]. The prewar indices for West Germany listed in the source refer to 1936. To permit international comparability, the index has been raised by 23 per cent, the relevant German growth rate between 1936 and 1938. It has been obtained from [16, p. 600]. National product series are available only for the territory of the German Reich within the borders of 1937, not for the present territory of the German Federal Republic, and only for net instead of gross national product. The discrepancies between growth rates should be negligible.

It is also worth pointing out that mass unemployment persisted in the United Kingdom and most other European countries in 1938 while the German economy was running full blast. Germany's growth potential since 1938 was therefore considerably smaller.

With the advantage of hindsight, Thomas Balogh's gloomy predictions make interesting reading now [1]. So does Wilhelm Röpke's booklet [20], written in the same year, a remarkably prophetic statement of the opposite school of thought.

[4] Hodgman's index [10] has been extended to 1955 by the staff of the Joint Economic Committee of the United States Congress [12, p. 24].

[5] The construction of the official Soviet index is open to serious objections since it is not based on value added, but on gross output of industry, and therefore tends to overstate growth during a period of increasing industrial specialization. The Soviet index now uses 1952 weights.

[6] From 1946 to 1956, U.S. governmental nomilitary grants and long-term loans to the three countries were as follows (in billion dollars): France, 5.5; Germany 3.9; United Kingdom, 6.8 [31, pp. 124-27].

[7] The losses from dismantling have been estimated at approximately $500 million [34, p. 370].

than defense expenditures in other European countries at this time.[8]

Two important handicaps for West Germany deserve to be emphasized. One is the unfavorable age composition of her population, a result of the casualties of the second world war. Another was the obliteration of a large part of many German cities through bombing during the war. The provision of adequate housing both for West Germany's original population and the 10 million refugees absorbed a major proportion of the country's resources for a type of investment whose low payoff in terms of current production is well known.[9]

West Germany's impressive recovery took place under policies that were in many respects the direct antithesis of post-Keynesian prescriptions for rapid economic growth. Few other countries have pursued a hard-money policy with similar determination. Although prices initially rose sharply after the lifting of controls in 1948, the price level has been almost constant from 1951 to 1957.[10]

Particularly remarkable was the virtual renunciation of fiscal policy as an anticyclical device. The Minister of Finance during the recovery period, Fritz Schäffer, remained unflinchingly devoted to the principle of a balanced budget. The West German authorities made considerable, and highly successful, use of tax legislation in order to spur capital formation. These measures cannot, however, be classified as functional finance. Their intention was not a rise in aggregate demand, but an increase in private investment, cancelled by a reduction of consumption by the same amount. The unwillingness to use fiscal policy for the purpose which many economists are inclined to consider as one of its most

[8] From 1950 to 1955, occupation costs amounted to an average of 5.8 per cent of the gross national product in West Germany. The share of defense expenditures in the gross national product of the United Kingdom, one of the countries with the most intense defense effort, was 8.1 per cent for the same period [19, July 1958, pp. 104, 116].

[9] During the years 1950-1956, West Germany devoted an average of 6.5 per cent of her gross national product to the construction of dwellings, the highest ratio of any European country [23, 1957, p. 257]. In the United Kingdom, for example, the value of new housing represented 3 per cent of gross national product from 1955 to 1957 [28, 1958, p. 166]. Detailed figures for earlier years are not given in the source, but the total value of new construction during these years suggests that the proportion of GNP devoted to the construction of dwellings was, if anything, lower.

[10] [34, Ch. 4] [4]. From a level of 91, the wholesale price index of industrial products rose to 104 during the six months following the currency reform of June 1948 (1949 = 100). It fell gradually to 96 in June 1950. The outbreak of the Korean war in that month reversed the trend. The index reached a maximum of 124 in December 1951 and remained at or below that level until 1958. The movement of the cost-of-living index closely paralleled that of the wholesale price index of industrial products. Its rise between 1951 and 1958 was a fraction higher owing to the rise in the wholesale price index of agricultural produce by about 12 per cent during this period [23, 1952-1957].

A recent interview in which Hjalmar Schacht denounces the West German government for its inflationary policies provides an amusing footnote to history. *Der Spiegel,* October 8, 1958, pp. 20-31.

important functions left monetary policy in virtually exclusive control of the regulation of aggregate demand. Relying on the traditional tools, monetary policy performed its task smoothly and effectively. Modern monetary theory owes us an explanation of why it did.[11]

The high rate of unemployment during the first few years after the currency reform cannot be cited as evidence that the Bank Deutscher Länder did not, or could not, pursue effective anticyclical policy. The unemployment that occured then was almost exclusively of the structural variety. A population of only 40 million people had to absorb an inflow of 10 million refugees from the East. The destruction of cities and of a large proportion of the country's industrial assets made it extremely difficult to provide the refugee labor force with housing in the areas of labor shortage and with the equipment that would suit individual skills.[12] More surprising than the existence of an unemployment problem during the early years of recovery is the fact it vanished after a few years. The constancy of the employment level precisely during those years when West Germany's national product was rising most rapidly also illustrates how relatively unimportant for the level of production full employment of the labor force may occasionally be as compared to its efficient allocation.[13]

[11] A remark by J. K. Galbraith on 19th century central banking reflects the widespread disdain for orthodox monetary policy: "Such was the charm of this policy that this affection was easily translated into claims for its effectiveness which, in fact, did invade the supernatural. Monetary policy was graced by effects not only mysterious but magical" [6, p. 228].

[12] The average rate of unemployment as a percentage of the available labor force developed as follows (beginning with 1948): 4.2, 8.3, 10.2, 9.0, 8.5, 7.5, 7.0, 5.1, 4.0, 3.4, 3.4. The structural nature of unemployment during the early years of recovery is clearly revealed by the regional differences. In December 1950 when unemployment reached its peak, the rate of unemployment was 26.3 per cent in Schleswig-Holstein, a predominantly rural state whose dwellings had suffered comparatively little destruction and which had therefore absorbed a large percentage of the refugee population, and only 4.6 per cent in North Rhine-Westphalia, the largest state in the federation with 28 per cent of the total population. The former state of Württemberg-Hohenzollern showed an unemployment rate of only 3.5 per cent. The Ruhr industries experienced a labor shortage [24, 1951, p. 10*]. Additional evidence is the rise in the West German employment index by 36 per cent from 1950 to 1957, when most of the necessary long-run adjustments had taken place.

[13] The average employment level remained unchanged from 1948 to 1949 while the index of industrial production rose by 43 per cent. In 1950, employment increased by 2.8 per cent while industrial production rose by 26 per cent. A major reallocation of the labor force took place, however, reflected in the rise of employment in manufacturing industries by 7 per cent from 1948 to 1949 and by 6 per cent in 1950 [24, 1949-1951]. It must not be overlooked, of course, that a much higher growth rate was to be expected during the period of postwar reconstruction than in normal peacetime years. Those who argue that there was nothing exceptional about the rapid growth rates in 1948 and immediately thereafter because recovery "really" got under way only then should not overstate their case, however, for they invite the fundamental question why there should not have been any reconstruction to speak of during the three preceding years of relatively full employment.

## II. *Currently Popular Explanations of the Recovery*

The explanation of the "German miracle" on the basis of the vigorous competition that characterized the West German economy and the reliance on stiff interest rates rather than on qualitative controls in the allocation of investment has not been the most popular one. Factors that have been cited more often are German enthusiasm for hard work, the docility of German workers and the weakness of German unions that made a low wage level possible. Low wages supposedly held private consumption down and permitted a high rate of investment. Although particularly popular among post-Keynesians,[14] these arguments are somewhat incongruous with post-Keynesian doctrine. The proposition that one has only to reduce consumption in order to stimulate investment involves an inadvertent acceptance of Say's Law. It would be more consistent with post-Keynesian ideas to argue the opposite, that a reduction of consumer demand must *depress* the volume of investment, especially if credit is tight and the price level is stable. This was indeed one of the principal fears of Erhard's post-Keynesian critics during the early stage of the experiment [1] [17] [33].

Closer inspection reveals, moreover, that the facts hardly match the implied popular conception of West Germany as a country in which big business domineers over labor relations. In 1951, the union movement secured passage of the Law on Codetermination. This law stipulated that half of the members of the boards of directors of all companies in the coal and steel industries were to be delegates of the workers and the union movement. The workers in these industries were also given representation in management through the election from their ranks of vice presidents for industrial relations. The Works Constitution Law of 1952 provided that one-third of the boards of directors of all companies in other industries was to be composed of workers' delegates [34, Ch. 10] [5]. Would American unions succeed if, contrary to fact, they were interested in lobbying for the adoption by Congress of a law of this kind?

It is true that unions did not press with similar determination for higher wages during the early years of recovery. U.S. observers, accustomed as they are to seeing unions essentially as business organizations with relatively limited objectives, are prone to mistake self-restraint in matters of wage policy, a result of the political composition of the West German trade union council, for weakness.

Equally mistaken is the usual conclusion that wages did not rise as a result of the moderation of West German unions. During the six months following the currency reform of June 1948, the index of hourly wages

[14] For example, A. H. Hansen [7, p. 15].

in industry rose by 15 per cent. During 1949 and 1950 the rates of increase were 8.5 and 10 per cent, respectively. The index of hourly wages increased by 68 per cent from 1950 to 1957. The comparable index for the United Kingdom, a country with a supposedly more powerful labor movement, increased by slightly less. The increase in British *real* wage rates was, of course, only a fraction of that observed in West Germany. The British cost-of-living index rose by 42 per cent from 1950 to 1957, as compared to a rise by 14 per cent of the West German cost-of-living index. A comparison with prewar figures shows that the index of real hourly wage rates in West Germany in 1957 was 52 per cent higher than in 1938, while its British equivalent had increased by only 21 per cent [19].

During the early years of the German recovery, the share of wages and salaries in the national income fell slightly below the level of some of the years before the war. The explanation is hardly that real wages were falling, as has usually been taken for granted. It is rather that the national product, the denominator of the proportion, rose too fast to permit wages and salaries, always one of the less flexible economic variables, to follow with equal speed. Moreover, the share of wage-and-salary earners in West Germany's national income rose continuously from 1951 on [23]. Computations by the U.N. Economic Commission for Europe show that in 1955 the share of wages and salaries in West Germany's national income at factor cost (63.6 per cent) was the second highest in Western Europe [30, 1955, p. VIII-3].[15]

Although the rate of capital formation was higher than in most Western countries, consumption was not unusually low in West Germany by prewar German standards. In 1949, the proportion of the gross national product devoted to private consumption was 65.2 per cent; in 1950, 63.6 per cent, as compared to 62.2 per cent in 1936. Only after 1950 did it fall to about 59 per cent [23]. During the years 1950-1957, gross domestic fixed capital formation amounted to an average of 20.5 per cent of West Germany's gross national product, as compared to an average of 28 per cent for Norway, for example. During the same years, West Germany achieved an average growth rate of the real gross national product of almost 8 per cent, compared to 3 per cent for Norway [18] [19, July 1958].

Another comparison between the United Kingdom and West Ger-

[15] The discussion refers to income before taxes. It is usually believed that the German tax structure is regressive by international standards. We cannot enter into a detailed investigation here. The income tax schedule is less progressive in West Germany than in the United Kingdom, for example [30, 1956, pp. ix-18-24]. Britain's reputation as the country with the most highly progressive tax system is not borne out so well by the share of direct taxes in total tax revenue. The average proportion of direct taxes for the years 1953-57 was 50 per cent, as compared to a share of 47 per cent in West Germany and more than 80 per cent in the United States. In France, direct taxes accounted for only 28 per cent of the

many yields a striking result. O.E.E.C. computations show that the increase in real per capita private consumption between 1938 and 1957 was 25 per cent for West Germany and only 7 per cent for the United Kingdom.[16] This comparison is all the more remarkable in view of the fact that the inflow of refugees increased the population of West Germany by 25 per cent while the population of the United Kingdom rose by only 8 per cent during this period.[17]

As an explanation of Britain's slow rate of growth, one frequently encounters the argument that her extensive social security program dragged on her resources. The West German economy, it is sometimes believed, did not have to carry a comparable burden. But the exact opposite is true. When the aggregate value of welfare services is compared with national income at factor cost, we find that during 1953 it amounted to 16 per cent of the latter in West Germany and to only 12 per cent in the United Kingdom.[18] Only the fact that the German social security system was initiated as far back as 1881 and has been gradually expanded without fanfare ever since whereas many British social security services were created from scratch at the end of the war can explain the popular conception of Britain as a more highly developed welfare state.

### III. *The Case for Competition—a Relic of the Past?*

Monocausal explanations of unusual phenomena are always suspect, and rightly so. With all due respect to all other factors, biological,

[16] [19, July 1958, p. 96]. The prewar index figure given for West Germany refers to 1936. It has been raised by 15 per cent to account for the increase in per capita consumption that took place between 1936 and 1938. See footnote 3 for the adjustment procedure.

[17] [23, 1957] [28, 1957]. The age composition of refugees was not significantly different from that of the resident population. In 1950, the percentage of males between the ages of 20 and 45 among the resident refugee population was approximately 16.7 in 1950, compared to 16.1 for the total population of West Germany [23, 1957, p. 42].

[18] [25, Ch. 12] [28, 1957, p. 50]. For the purposes of this comparison, the following items were included in the German figure (in billions of Deutsche Mark): medical insurance benefits, 3.3; accident insurance benefits, .8; old age pensions, 6.2; unemployment insurance benefits, .9; unemployment relief, 1.0; general relief, 1.1; war victims pensions, 2.5; transitional aid to refugees under the Law of Equalization of Burdens of 1952, 1.7. The total of DM 17.5 billions does not include a figure of .7 billion of pensions to former civil service personnel among the refugees.

The British figure includes the following (all figures in million £): payments to individuals under the British national insurance schemes, 495 (this includes retirement pensions, sickness and maternity benefits, death grants, unemployment benefits and industrial injuries benefits); noncontributory old age pensions, 24.3; national assistance, 129.8; war and service disability pensions, 82.2; family allowances, 93.1; industrial rehabilitation grants, 2.8; nutrition services, 80.1; scholarships, 40; grants for child care, 18; contributions under the National Health Service and other health services, 490.1. The aggregate figure for fiscal 1953 amounts to £1,456 million.

There is a significant difference between the methods of financing medical services. The British National Health Service is financed fully out of general tax receipts. Compulsory medical insurance in Germany is administered on a strictly actuarial basis. In this respect, the German social security system is more regressive than the British scheme.

sociological and metaphysical, the conclusion is nevertheless inescapable that only more efficient resource allocation can explain the difference between West Germany's rate of growth and that of other countries in a comparable state of development. Nobody in his right mind would claim that the West German economy is even close to *perfect* competition. The same variety of different forms of market structure can be found there which we are used to observing in all other advanced industrial countries. But the postwar German experience seems to suggest that it is feasible to intensify competition sufficiently to make an economy perform markedly better than under alternative forms of social organization [9, pp. 92-106]. It is often held that enforcement of competition through prohibition of cartel agreements and the breakup of large combines can never be effective. If that were true, it would be hard to understand why German industrialists have fought so bitterly, and unfortunately with some measure of success, for the repeal of the Allied ordinances.

The hypothesis that the attempt at strengthening competition deserves principal credit is here advanced with all due caution, but the evidence in its favor seems overwhelming. Of the world's leading countries, only the Soviet Union and Japan[19] have shown a comparable growth rate. It should be remembered that the Soviet economy is still on a level of development at which growth rates have been typically higher also for capitalist countries. In addition, Soviet engineers now have at their disposal all the accumulated technological knowledge which more advanced countries had to acquire on their own. Finally, among economies with the same rate of growth, a widely, though apparently not universally, held value judgment would prefer the one in which governmental compulsion is least.

Apart from the internal measures that have been described above, West Germany's liberal trade policies have undoubtedly been a major factor contributing towards active competition. Economists who exhibit pessimism concerning the feasibility of effective measures for curbing market imperfections often overlook the possibility of removing barriers to foreign trade, by far the simplest and most effective method (although possibly a politically strenuous one) of increasing the number of competitors in a monopolistic or oligopolistic industry. This factor is quite independent of the improvement in the allocation of the world's resources that results from international specialization.

In this connection, it is worth emphasizing one of the principal advantages of monetary stability that is consistently overlooked. In no

[19] Japan's economic policy since 1949 has been based on the same general principles as West Germany's. See footnote 32 below.

small measure, West Germany's stable price level since 1951 saved the country from the foreign-exchange difficulties that confronted most of the rest of the world. As a result, the authorities did not have to impose the severe restrictions on foreign trade which had to be applied elsewhere. The gains from intensive participation in international trade were certainly among the major causes of West Germany's rapid growth. It is one of the most remarkable aspects of the German recovery, on the other hand, that the wounds inflicted by the war were hardly healed when the country was able to assume, without visible strain, what is probably the highest per capita surplus on current account in the world today.[20]

The American economy has in recent years been developing at a disappointingly slow rate. Even before the latest recession set in, the Federal Reserve index of industrial production had risen by a mere 7 per cent from 1953 to 1957. Economists who argue that a determined effort at strengthening competition is a necessary first step toward improving matters are a relatively small minority. The advocacy, instead, of expansionary monetary and fiscal measures, possibly combined with direct controls of various sorts, finds a considerably larger number of supporters. One may well favor a high rate of economic growth without endorsing the view that a rise in the general price level is an inevitable by-product of, or perhaps even a necessary condition for, economic expansion and should be little or no cause for concern.[21] Judging from postwar experience, one can argue convincingly that an attempt merely at an expansion of effective demand without simultaneous reforms affecting market structure may perhaps bring about full employment or even a labor shortage, together with a rise in prices, but will increase the volume of real output little or not at all.

An examination of price and production data for different countries

[20] West Germany's gold and foreign-exchange reserves increased from $270 million at the end of 1950 to $6.3 billion at the end of 1958. The average surplus on current account from 1951 to 1958 amounted to approximately 3.3 per cent of the gross national product, compared to an average of .5 per cent for the United States for the same years (excluding military supplies). Even during the height of U.S. economic aid to Europe, the 1946-50 average for the United States was a mere 1.7 per cent [23] [32].

The West German government has, in fact, been frequently criticized for permitting the country's huge export surplus. But should not a substantial export surplus be the normal state of affairs for a highly industrialized and prosperous country, and is not rather its absence in other similarly developed countries a matter for concern? The objections to West Germany's export surplus would be understandable in the unemployment wonderland of the 1930's, but they are hardly so in the world of inflationary exuberance that has been the rule during the past 15 years. It may be true that the form in which the surplus has been financed (largely through accumulation of gold and short-term claims) is not the best possible one, but this is an aspect of secondary importance.

[21] "Creeping inflation is a necessary cost of achieving a steadier growth of production and employment" [21, p. 28].

shows that no positive correlation can be detected between growth rates of real output and rates of increase in the price level. Table 1 ranks the O.E.E.C. countries, the United States and Canada according to rates of growth and also shows the development of prices in these countries for the same years.[22]

The almost universally observable *qualitative* deterioration of private investment during times of inflationary pressure seems to be sufficiently

TABLE 1—RATES OF GROWTH AND INCREASE IN PRICES, OEEC COUNTRIES, UNITED STATES AND CANADA

| Country | Average Annual Rate of Growth of Real Gross National Product 1952–1956 | Average Annual Increase in the Implicit GNP Price Deflator, 1952–1956 |
|---|---|---|
| West Germany | 7.8 | 1.3 |
| Greece | 7.7 | 10.0 |
| Austria | 6.7 | 2.0 |
| Netherlands | 6.0 | 1.9 |
| Italy | 5.7 | 2.1 |
| Switzerland | 5.4 | .5 |
| O.E.E.C. | 4.7 | 2.4 |
| Canada | 4.5 | 1.5 |
| Portugal | 4.5 | 0 |
| France | 4.1 | 1.9 |
| Sweden | 3.7 | 1.9 |
| Turkey | 3.7 | 10.0 |
| United States | 3.3 | 1.5 |
| Belgium | 3.2 | .8 |
| Norway | 3.2 | 3.3 |
| United Kingdom | 3.2 | 3.3 |
| Denmark | 2.3 | 3.3 |
| Ireland | .5 | 2.9 |

*Source:* O.E.E.C. [19, July 1958].

important to outweigh before long whatever stimulating effects a price rise may have in the very short run. The exclusive preoccupation of macroeconometricians with aggregate components of effective demand to the complete neglect of their qualitative composition has undoubtedly led to some of the more costly fallacies in current economic analysis.

Is there any scope for intensifying competitive pressure in the

[22] Several periods have been tried, but the general result was the same. The period 1952-56 has been selected in order to meet the legitimate point that the immediate postwar years and the period following the outbreak of the Korean war were too disturbed by extraneous factors to permit meaningful comparisons. At the time of writing, 1956 was the latest year for which national income data were available for all the countries. Needless to say, the reliability of the data varies considerably from country to country. The GNP deflator was used as the measure of price change that is least dependent on movements of world market prices.

American economy as an alternative remedy? In contrast to the situation in prewar Germany and most other European countries, American legal doctrine has been presumed to hold agreements in restraint of competition illegal. There are important exceptions, however. The National Industrial Recovery Act of 1933 was the most notorious, though short-lived, expression of a different school of thought. This school has subsequently reasserted itself in pieces of legislation such as the Robinson-Patman Act or the fair-trade laws at the state level. The practices of the Texas Railroad Commission and the encouragement of "voluntary" (and occasionally mandatory) restrictions of oil imports by the federal government fly in the face of every notion of competitive behavior. So does federal legislation in the agricultural sector. Oligopoly rules sovereign in some of the most important American industries. Comfortable tariffs and the imponderabilia of United States customs procedures protect many imperfect markets from exposure to the more competitive world market.

A majority in our profession has usually received proposals for a breakup of the giants of American industry into smaller, more competitive units with little more than amused sympathy for the quaintness of the idea. Not only are such proposals regarded as politically unfeasible, but it is also often feared that significant economies of scale would be lost. The pessimists may be right on the first point, but perhaps largely because the apathy of public opinion has long been conditioned by their pessimism. The second point has also been a favorite among German industrialists in their years of agitation for permission to reunite the shattered giants. West German production figures would rather suggest that German industries were doing exceptionally well under the alleged handicap of deconcentration. It will be interesting to see whether the current West German merger movement will increase productivity as much as the advocates of bigness want us to believe it will. Preliminary evidence does not favor their side.

Can labor relations in West Germany serve as a guide for other countries? We have argued above that the alleged weakness of German unions is contradicted by their legislative successes, although it cannot be disputed that unions did not bargain for higher wages with quite as much determination as they did in other Western countries. Whatever the reason—and the result would have been the same had it indeed been weakness—the figures quoted earlier on the evolution of German wages and the share of wage-and-salary earners in the national income suggest that German workers fared singularly well.

Is there a convincing reason to defend monopoly power of labor unions if a determined attempt at enforcing competition is made in all other markets? Is it believed that the redistribution of incomes that is

expected to follow from collective bargaining is achieved more justly and more effortlessly in this way than through more progressive taxation of incomes and wealth? The subject is laden with emotion, and even in posing these questions, I become acutely aware of the fact that "questioning the virtues of the organized labor movement is like attacking religion, monogamy, motherhood, or the home" [22, p. 1]. A thoroughgoing re-examination of the economic role of unions in a full-employment economy is long overdue. An impartial appraisal might well reveal that some of their most basic functions which few people are as yet prepared to challenge are, at best, superfluous and could be more efficiently performed by government agencies, or at worst, that they are positively harmful. To accomodate union pressure by permitting creeping inflation does not seem to help matters.[23] The race between prices and wages has, in addition, frequently resulted in a decline of the share of wage earners in the national product, defeating one of the principal objectives of union strength. "Forced saving" by depressing the real income of the labor force through a rise in prices has, in fact, been seriously advocated as a means of accelerating capital formation.[24]

## IV. *Recent Trends in West Germany*

If my basic theory is correct, present trends in West Germany are not encouraging. The early 1950's were years of bitter controversy between representatives of industry, who favored a replacement of the blanket prohibition of cartels under the postwar Allied decrees by as complete a restoration as possible of the legal situation before the war, and Minister of Economic Affairs Ludwig Erhard and his small but politically influential group of followers. In July 1957, the struggle ended with a compromise. The Bundestag passed what was ostensibly an anticartel law. The cartel authority set up to implement the new legislation was given discretionary powers, however, to grant dispensation from the ban on restraints of trade whenever it was satisfied that a restrictive agreement was in the best interest of the economy as a whole. It stands to reason that the instructions provided by a law of this kind must necessarily be so general that the competitive climate will be largely conditioned by the personal convictions of the adminis-

[23] See Table 1 above.

[24] For a history of the forced-saving doctrine, see Hayek [8, pp. 18-25]. Various Latin American governments have attempted to apply the doctrine [35, pp. 28-29]. Available statistical data indicate that, while the share of the lower-income groups may usually have fallen slightly during inflationary periods, inflation has not often served to accelerate the formation of capital, and certainly not the formation of the type of capital that would contribute towards a rapid growth of output. The Latin American experience is especially illuminating.

trators of the cartel authority. One suspects also that it must be exceedingly difficult to decide whether a new cartel whose case is pending will be more or less harmful than others for which permission was previously granted, and that the absence of visibly "criminal" provisions in the typical cartel agreement will make it more difficult for a lawyer's mind to justify the denial of permission than to justify the acquiescence of the authorities.[25]

Another disconcerting development is the massive merger movement that is now taking place. When the country was granted full soveignty by the Western powers in 1955, the West German government pledged itself to continue the deconcentration measures inaugurated under the occupation regime. The Allies conceded, however, that deconcentration had gone too far in a few instances and expressed agreement with West German proposals for permitting partial reconcentration. The banking system was one of the sectors whose reconcentration was envisaged.

The mergers that have recently taken place go beyond this understanding. The acquisition of the Bochumer Verein, one of the largest steel producers in the Ruhr, by the Krupp interests in violation of a binding commitment has been widely publicized.[26] It is only one example illustrating the general trend. The mergers of commercial banks also have wider significance than meets the eye.[27] Unlike banks in the Anglo-Saxon countries, German commercial banks have traditionally held a substantial proportion of their assets in the form of equities. Their holdings often amounted to a controlling interest in the companies concerned. One of the principal consequences of the Allied program of deconcentration has undoubtedly been a greater diversification of the portfolios of commerical banks and less likelihood that a bank could acquire control over a whole industry. Reconcentration of banks can be expected to carry with it an increase in the degree of integration of West German industries that is none the less effective because it escapes the eye of the public.

Not enough time has evolved to appraise the economic consequences of the reduction in competitive pressure. The rate of growth of industrial production during 1958 was 3 per cent, only half the next lowest growth rate in any year since the currency reform. Clear inferences

[25] "Bonn's Elastic Cartel Law," *The Economist*, July 20, 1957, p. 277.

[26] *New York Times*, January 7 and 11, 1959.

[27] After the war, the Allies broke up the "Big Three" of the German commercial banking system into 30 independent units. West Germany was subdivided into 10 banking districts and no bank was allowed to establish branch offices outside its own district. In 1952 the occupation authorities conceded a reduction in the number of banking districts to 3 and a merger of the 30 successor banks into only 9 independent units. A law of December 24, 1956 eliminated the restrictions on chain-banking and removed the legal barriers to further bank mergers. In May 1957, the "Big Three" were re-established [26].

are difficult since it can be argued that the lower rate of growth is largely due to the latest U. S. recession and to the reduction in the work week in most sectors during 1958.[28, 29]

## V. *Concluding Remarks*

We may tend to smugness about the progress economics has made during the past few decades. If performance in the real world is a criterion, the success of a country whose government has systematically disregarded some of the most widely accepted doctrines of recent vintage should give us pause. We have had several occasions to contrast West Germany with the United Kingdom and the Scandinavian countries—the latter countries the cradles of many of these doctrines and countries where they have guided public policy during most of the postwar era. None of the underdeveloped countries that have followed the recommendations of policy advisors steeped in post-Keynesian analysis can honestly claim a satisfactory rate of growth.[30] The disappointing results of the Indian experiment are legitimate cause for alarm, although they have as yet prompted few other countries in Southeast Asia to abandon India's leadership in matters of economic policy.[31]

The example of West Germany, on the other hand, is not the only one that speaks for the abandonment of unqualified post-Keynesianism. After years of inflation, Japan undertook a thoroughgoing program

[28] The impact of the U.S. recession is difficult to appraise. West German exports for 1958 were, in fact, 3 per cent above the 1957 level (both value and volume), but the rate of increase was considerably below that of previous years. The value of imports, on the other hand, fell by 1.8 per cent while their volume increased by 7 per cent. The rate of unemployment remained unchanged during 1958. [24, January 1959].

[29] The implicit GNP price deflator (based on preliminary figures) rose by 3.2 per cent during 1958. The rise in the price indices for wholesale and resale trade was less marked owing to the favorable development in West Germany's terms of trade. The sharp rise in the stock market index (54 per cent) during this year of comparative lull would normally also have to be interpreted as a disturbing development. Much of it was, however, due to the substantial reduction in the corporate income tax rate. It is therefore difficult to evaluate the effect of increased monopoly power (or its anticipation) on the stock market index [24, Jan. 1959]. As this article goes into print, the bull market continues. At mid-1959, the stock market index had more than doubled compared to its level in early 1958. This unprecedented increase is seen in proper perspective when it is remembered that the index had remained virtually constant from 1955 to 1957.

[30] The use of the term "post-Keynesian" rather than "Keynesian" throughout this essay was prompted by my conviction that Keynes would not have been too eager to identify himself with some of the ideas for which the authority of his name has been appropriated. The disregard of resource allocation, the neglect of international repercussions of domestic policies, the acceptance or even advocacy of creeping inflation, to name only a few characteristics of an important segment of the post-Keynesian school, seem anathema to Keynes' economic philosophy.

[31] Pakistan has recently begun a thoroughgoing reappraisal of its development planning along neoliberal lines. *New York Times*, Feb. 26, 1959.

of monetary stabilization in April 1949. The return to orthodox monetary management has since been accompanied by one of the world's highest rates of economic growth.[32] Numerous other countries could be cited that followed similar, though less consistent and less remarkably successful policies.

Comparison of the payoffs of different economic policies has led to a marked change in the political climate of Western Europe. Erhard's remarkable popularity which only became fully apparent in the recent West German "war of succession" should be food for thought for those who have been known to decry like policies as politically unfeasible. West Germany's Social Democratic Party bitterly opposed Erhard's policies during the early years of recovery. It stands to reason that the party had little sympathy for the government's lack of concern for full employment at all costs. The degree of market perfection, on the other hand, used to be a *Fremdwort* in the orthodox Marxist's handbook. Many German socialists have traditionally regarded the "chaos" of competition in a free-enterprise economy as one of their principal targets of attack, and some even saw in the formation of cartels a welcome move towards a guided economy.[33] Events appear to have been persuasive enough to bring about a profound change in this attitude. In some recent legislative contests, the Social Democrats have turned out to be Erhard's supporters against a majority of his own party. The anticartel law of 1957 was unanimously (albeit unsuccessfully) opposed by the Social Democrats on the grounds that its provisions were not strict enough.

Recent German trends underscore the enormous difficulties faced by a government bent on sustaining vigorous competition. It has become plain, as neoliberals have long pointed out, that a policy of letting things drift is not the way to realize the ideal of classical *laissez faire*. It is equally obvious that liberal parties, the traditional vehicles for the realization of effective competition, may depend too much on the financial contributions of precisely those groups against whom continuous

[32] The wholesale price index for 1958 was 2 per cent above that of 1951. The index of industrial production tripled between 1950 and 1958. The index is weighted by value added in 1955 [2, Jan. 1959]. The increase in the production index from 1950 to 1952 is perhaps a less noteworthy achievement. Only in 1952 did industrial production attain the highest level established before the war, and it is well known that the marginal returns to reconstruction and repairs are high. It can be assumed that genuine growth took place after 1952. The industrial production index doubled in the six years that followed.

The policies pursued by the American occupation regime in Japan, notably its strong antimonopoly measures, paralleled the pattern set in West Germany in many other respects. So has, unfortunately, the trend of Japanese policy with regard to industrial concentration after the end of the occupation regime.

[33] This charge cannot, however, be leveled against some of the founders of the German socialist movement in the 19th century [13, pp. 78-79].

policing of markets must be primarily directed. Socialist parties may, however, eventually emancipate themselves from an exclusively Marxist or post-Keynesian orientation. The rumblings among younger leaders of West Germany's Social Democratic Party hold out definite promise for a change.[34] By and large, few signs of revolt against the entrenched authority of post-Keynesian thinking can as yet be detected among economists in the Anglo-Saxon countries, in spite of the incontrovertible evidence that something is rotten in the body of doctrine that constitutes the orthodoxy of today. Let us be mindful of Keynes' warning as to the "encroachment of ideas; not, indeed, immediately, but after a certain interval, . . . so that the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest" [14, pp. 383-84]. May not the *General Theory* now be in danger of becoming the material from which "madmen in authority, who hear voices in the air, are distilling their frenzy" [*loc. cit.*]?

[34] *New York Times*, May 1 and 21, 1958; "After Erhard," *The Economist*, July 20, 1957, pp. 193-95.

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# MAN-LAND EQUALIZATION THROUGH MIGRATION

*By* FRANK T. BACHMURA*

During the unprecedented continuous period of full urban employment of the recent past, the U. S. farm sector has experienced extremely high rates of net emigration. Despite these massive movements of population off farms and the growth of part-time farming, considerable areas of low factor reward to farm workers persist in a significant portion of the U. S. agricultural labor force. While this diminution of farm labor inputs has mitigated the problem of low incomes per farm worker, there nonetheless continues to be a wide gap in average agricultural incomes between high farm-income areas such as Iowa and a low farm-income area such as the Mississippi delta. Some students look upon this persistent income differential over the last 19 years as testimony to the failure of migration in achieving regional equalization of farm incomes. While emigration of farm population cannot *alone* solve the farm poverty problem, any solution to this problem which implies a reduction of farm labor inputs necessarily bears upon the problem of farm labor redundance. This is true whether the solution envisaged involves part-time farming, employment in local nonagricultural industries, or actual residential change through migration.

The present paper is concerned with the effectiveness of farm-labor migration in diminishing regional differentials in agricultural incomes. A comparative study has been made of two areas: Iowa, and a 24-county case-study area around Memphis.[1] In Section I we consider the relative changes that have occurred in the ratio of farm land per farm worker in the two areas between 1940 and 1950; make some extrapolations of these results to 1965; and compute the migration rate required for the case-study area to bring the ratio of farm land per farm worker in that area up to the ratio for Iowa for various years. In Section II, various policy recommendations are briefly examined.

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[1] These counties (selected for a small-area study of regional income disparity) are: (in Arkansas) Crittenden, Lee, Mississippi, Phillips, and St. Francis; (in Mississippi) Benton, Bolivar, Coahoma, DeSoto, Leflore, Marshall, Panola, Quitman, Sunflower, Tallahatchie, Tate, Tunica, and Washington; and (in Tennessee) Fayette, Hardeman, Haywood, Lauderdale, Shelby, and Tipton. The land area of these counties is considerably smaller than that of Iowa, but exceeds the area of Maryland. The counties lie in the lower Mississippi valley and most of the area is alluvial in nature.

## I. *The Required Rates of Migration*

Because of the dynamics of natural increase, low per-worker incomes may persist in a given region despite high rates of net emigration and even when stable levels of aggregate farm income prevail. Under certain circumstances, moreover, a high farm-income area may have higher rates of net emigration than a low-income area so as to cause increasing rather than decreasing regional income disparity. In any case, high net emigration rates did characterize the higher farm-income areas of the country during 1940-50. Fortunately, from the viewpoint of correcting regional farm-income disparities, the lower-income areas showed even higher rates of net emigration during this period. However, even when there is a diminution of the *relative* differences in average income as a result of differential rates of migration in a corrective direction, it is still possible for the absolute differences in average income in real terms actually to widen. This appears to have been the situation in the instant case. Under conditions of this sort, equalization depends upon the continued maintenance of corrective differential rates of migration for long periods. To maintain such differential rates implies, as a minimum condition, the existence of full urban employment for sustained periods.

The primary analysis of this paper depends upon a rather heroic assumption: that the number of farm acres per farm worker is a fair index of average net incomes to farmers for the purpose of comparing the two specified areas. Further, it is assumed that with the equalization of the number of farm acres per worker in the two areas, average net income per farm worker would be approximately equal in the two areas.[2]

[2] Because the case-study area is, at present, a predominantly cotton-producing area, a crop which produces a considerably higher value per acre than corn or soybeans, it may be argued that less land could be used in production to achieve a given average income level vis-à-vis Iowa. Iowa, however, does provide high revenues per acre through its large capital investment in livestock, which provides an economic use for much of the state's farm-labor input. Gross values of farm products per acre taken from *U.S. Census of Agriculture* [10] data for Iowa and the case-study area respectively follow: 1954, $54.05, $50.49; 1949, $47.73, $38.48; 1939, $16.45, $15.29. Thus, there is a remarkable similarity in these value-per-acre figures. On the basis of estimated net returns to family and hired labor for 1940, there was a considerable difference in the proportion of total product residual to the labor factor. For Iowa my estimate for 1940 is 38 per cent; for the case-study area my estimate is 60 per cent. Implicit in my basic assumption is the idea that capital and farm organization will be adjusted so that a smaller proportion of gross product will go to labor and a higher proportion to other factors with the transfer from labor-intensive production to labor-extensive production. Despite the revolutionary nature of adjustments of farm capitalization and organization resulting from mechanical cotton harvesting since 1940, the amount of capital per farm worker is at least three or four times greater in Iowa than in the case-study area at the present time. My estimates, for Iowa and the case-study area respectively, follow: 1940, $5,392, $709; 1950, $13,073, $3,463; 1954 (based on 1950 labor-

The foregoing assumptions rest upon the evidence that both areas possess unusually high soil fertility, such that yields for crops common to both areas are reasonably close,[3] particularly with the use of supplemental irrigation.[4] This is not, of course, a necessary relationship, since land tends to be nonhomogeneous even in small areas, and since the patterns and amounts of productive capital investment and the production functions themselves are likely to differ with geography, climate, soil physics and chemistry, and other qualities. Actually I regard the similarities of the two regions as a special case, and precisely for this reason have I selected them. This special analysis permits an approach to the general problem of farm labor redundancy which presents itself most acutely in the South.

At least one important advantage adheres to the use of the man-land relationship in judging the possibility of future income equalization. Since the required amount of migration for the low-income case-study area here considered is of massive dimensions, the direct use of future income data would presume knowledge or require estimation of future production functions. Since the kinds of changes likely to occur with massive net farm emigration might well require shifts from a hitherto labor-intensive crop like cotton to other types of agricultural production, necessary assumptions as to the future course of farm production in the area would be no less heroic than the basic ones made above. These assumptions appear to me to provide a meaningful basis for arriving at a first approximation of the required emigration from the case-study area—an area of low agricultural incomes reflecting low labor productivities with, paradoxically, a rich natural resource base.

Most studies of low-income farm areas use the U. S. average as a yardstick to measure income improvement. To use the number of farm acres per farm worker for the United States as a whole would clearly provide an inappropriate criterion. In addition, average U. S. farm

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force estimate) $17,831, $4,110. Although these estimates and the estimates of net income for the two areas result from a laborious and complex process of estimation, I believe that they provide at least a rough order of relative magnitude and that they tend to justify my analytical assumptions.

[3] The 1949 yield per acre for soybeans harvested for grain was 9.19 bushels per acre in Iowa and 13.08 bushels per acre in the case-study area. Corn yields (for grain) were 46.42 and 20.61 bushels per acre respectively. Cotton, the existing principal crop in the case-study area is not common to both areas. These yields are, of course, not invariable, nor definitive, but do provide an approximation of relative physical productivity of soils in the two areas.

[4] Because of the rainfall pattern in the case-study area, with insufficient precipitation in most years at the time most necessary for the optimal growth of the corn plant, investment in supplemental irrigation equipment promises to have an important effect upon corn yields. In test plots, considerable increases in corn yields per acre resulted from the use of supplemental irrigation [3] [6].

incomes do not actually represent income levels reflecting farm-labor factor returns equivalent to those in urban employment. In Iowa, where median county farm incomes per income-receiving unit exceed those for comparable nonfarm units in many counties,[5] a claim can be made that such incomes represent labor factor rewards which are closer to a farm-nonfarm equilibrium level than are average U. S. farm incomes. For this reason, the Iowa level is the criterion for the adequacy of migration from the farm sector of the case-study area. In lieu of a U. S. average, the "midpoint," one-half the difference between the average acreages of the two areas added to the average acreage of the case-study area, is employed in the tables which follow.

TABLE 1—FARM ACREAGE PER MAN-EQUIVALENT FARM WORKER, 1940 AND 1950

| Area | 1940 | 1950 |
|---|---|---|
| Iowa | 113.95 | 154.71 |
| Case-study Area | 36.60 | 57.18 |
| Midpoint | 75.27 | 105.95 |

*Source: U. S. Census: Agriculture* and *Population*, 1940 and 1950.

Table 1 presents the number of acres per man-equivalent farm worker for 1940 and 1950 for the two areas, plus the midpoint. The man-equivalent farm worker is defined to include rural-farm males aged 15-64, adjusted for part-time farming by either farm or nonfarm people. The acreages shown reflect a 26 per cent reduction of the Iowa farm labor force during the 1940-50 decade as compared with a 31 per cent reduction for the case-study area. In Iowa, there was in 1940, 3.11 times as much farm land per farm worker as in the case-study area. By 1950, this relative difference had declined to 2.71 times. However, even with the extremely high rates of net farm emigration for the case-study area during 1940-50, the absolute difference in average per-worker acreage between the two areas *increased* from 77.35 acres in 1940 to 97.53 acres in 1950. A simple extrapolation of this trend to 1965, although continuing to reduce relative differences, would not reduce the absolute acreage per-worker difference. This is explained by the high rates of migration in Iowa as well as by the higher rates of natural increase in the case-study area.

Of course, such a simple extrapolation does not take into account the age, sex, and race proportions of the farm population with their differing rates of migration and natural increase. If the age-sex-race specific

[5] The computations are included in my doctoral dissertation. [1, Appendix 1]; see also [13, 1950, Tables 45 and 46].

migration rates experienced within the case-study area during 1940-50[6] are applied for a 15-year period to the 1950 farm resident male labor force of 157,089 together with age-sex-race specific survival rates,[7] a 1965 farm resident male labor force of 112,186 results. Now the estimated surviving potential farm resident male labor force for the case-study area amounts to 221,104 assuming no net migration, and is based upon a net natural increase of 64,015. In 1950, an estimated 88.02 per cent of the farm resident labor force in the case-study area actually participated in the farm labor force. This same proportion applied to the 1965 estimates of the farm resident labor force yields a farm-participating labor force of 194,616 before migration, and of 98,746 after migration.

TABLE 2—MAXIMUM SIZE OF FARM LABOR FORCE OF CASE-STUDY AREA NEEDED TO EQUATE FARM-ACREAGE PER WORKER WITH THAT OF IOWA

| To Equate Case-Study Area's 1965 Farm-Acreage per Worker | Maximum Farm-Labor Force, Case-Study Area |
|---|---|
| With Iowa's in 1940 | 69,384 |
| With Iowa's in 1950 | 51,104 |
| With Iowa's in 1965[a] | 32,842 |
| With Midpoint, 1940 | 105,040 |
| With Midpoint, 1950 | 74,623 |

[a] Simple extrapolation.

The net emigration rate computed from these data amounts to 69.33 per cent for 15 years, or, on a straight-line basis, 46.22 per cent decennially.[8] On the assumption of this much migration, the study area

[6] [2] The age-sex-race specific migration rates applied are the simple averages of applicable rates for economic subregions 61 and 76, which include the case-study area. The rates were applied decennially, that is, the rate for the 30-34 age cohort during 1940-50 was applied to the cohort of that age as of 1950 for the ten years 1950-60. For the remaining five years to 1965, the rate for the 40-44 age cohort was applied to the members of the original 1950 cohort still in the area as of 1960.

[7] [15] I applied unweighted three-state averages of stationary population survival rates to each age-sex-race specific component. Because of the indivisibility of the Census quinquennial age classification, I used the median annual age survival rate for each age category.

[8] Migrations are typically inferred from census population data. The population at the beginning of the period is compared with the population at the end of the period—any decrease in the population by the terminal date, after adjustments for enumerated births and deaths, is considered net emigration. The decennial migration rate is based on the population at the beginning of the period. I have followed this technique in computing the required 15-year rates of migration. The comparison of decennial with 15-year rates necessitates some assumption about the pattern of annual migration. I have used the straight-line method which assumes equal absolute numbers of migrants each year and converts a 15-year to a decennial rate by applying a factor of 2/3. This method yields a lower rate than do methods which standardize annual or decennial rates by the use of a

would have 80.07 acres per farm worker by 1965. This analysis, by using 1965 as a terminal date, avoids the further complication of estimating population increase by births subsequent to 1950, since labor-force entrants who will come of age by 1965 were already recorded by the 1950 census. Although arbitrary, the application of the 1940-50 migration rates to the 1950-65 period gives some idea of the adequacy of the area's farm labor force response to reasonably full employment for a continuous period of twenty-five years (1940-65).

Data in Table 2 permit comparison of the study area's predicted farm labor force of 98,746 and the maximum size of the farm labor force needed to achieve a farm acreage per worker equal to that of Iowa, assuming no change in farm acreage in the two areas after 1950. It is apparent that the predicted farm labor force would be at least twice as large, even after a generous estimated net emigration, as the maximum permissible to equalize the man-land ratio with the 1950 Iowa ratio. Moreover, the estimated 80.07 acres per worker by 1965 would fall short of the 1940 Iowa figure of 113.95 acres per worker, and even of the 1950 midpoint of 105.95 acres per worker (Table 1). An encouraging note is that the 1940 midpoint of 75.27 acres per worker would have been surpassed by 1965. Obviously, even if the Iowa man-land ratio were to remain entirely stationary between 1950 and 1965, there would be no equalization by that time. Giving effect to the dynamic aspect of Iowan agricultural adjustments during 1950-65 by a simple extrapolation of the state's 1940-50 rate of reduction of the number of farm workers, there would be 240.07 acres per farm worker by 1965. Viewed in these terms the farm labor redundancy problem of the study area becomes formidable.

How large would migration rates have to be to achieve the Iowa ratio by 1965? A 15-year net farm emigration rate of 103.79 per cent[9] would be needed to achieve the 1950 Iowa ratio by 1965. On a straight-line basis this becomes 69.2 per cent decennially. To achieve the 1965 Iowa ratio would require a net farm emigration rate of 116.97 per cent on a 15-year basis, or 77.98 per cent decennially on a straight-line basis.

Although the 1940-50 migration rates for the study area are very high, one might well ask, "What is the maximum practicable migration

compound interest formula. In my judgment, such rates are too high for the most meaningful comparisons because the formula implicitly reduces the adjusted annual or decennial base well below that of the actual population. This fact arises from the omission of population additions through birth and is especially important when migration rates are very high. In actual practice, natural increase is taken into account at the next census and a new base for rate computations is then formed.

[9] Emigration rates in excess of 100 per cent are possible because of the widely accepted use of a historical base which does not adjust for births. Such a rate signifies that children born since the last census enumeration will necessarily be included among the migrants.

under conditions of full employment?" While there is no simple answer to this question, I have taken, for each age-race (male) specific group, the maximum net emigration rate experienced in any subregion in the United States during 1940-50 for an approximation. These data, together with the comparable case-study area data, appear in Table 3. It is clear that rates in many areas were considerably higher than those for the study area during 1940-50. Moreover, because these rates do not take into account reductions in farm-labor inputs through part-time farming and the nonagricultural employment of farm resident labor

TABLE 3—RURAL-FARM MALE AGE-RACE SPECIFIC MIGRATION RATES, 1940–50
(Figures in parentheses indicate applicable census economic area)

| Age Group | Case Study Area | | "Maximum Practicable" | |
|---|---|---|---|---|
| | White | Nonwhite | White | Nonwhite |
| 0– 4 | −14.7 | −26.5 | (98) −45.0 | (77) −48.1 |
| 5– 9 | −24.1 | −29.5 | (98) −48.0 | (97) −49.3 |
| 10–14 | −53.0 | −53.2 | (79) −72.4 | (95) −75.8 |
| 15–19 | −56.0 | −63.8 | (79, 80) −75.2 | (79) −80.9 |
| 20–24 | −45.4 | −59.4 | (80) −62.3 | (97) −77.4 |
| 25–29 | −28.6 | −48.0 | (97) −45.9 | (78) −67.8 |
| 30–34 | −19.8 | −40.1 | (80) −39.0 | (78) −57.1 |
| 35–39 | −18.5 | −30.3 | (98) −36.0 | (97) −45.3 |
| 40–44 | −17.6 | −23.1 | (78) −32.0 | (78) −48.7 |
| 45–49 | −20.8 | −28.6 | (105) −36.1 | (97) −42.3 |
| 50–54 | −23.2 | −27.0 | (105) −43.9 | (77) −40.4 |

*Source:* Gladys K. Bowles, *Farm Population—Net Migration from the Rural-farm Population, 1940–50.* Washington 1956.

force, they very likely understate the actual rate of reduction of the labor force employed in agriculture. Because of this understatement and the fact that drought and similar influences did not affect the relevant census economic areas appreciably, these measures of the attractive power of nonfarm job opportunities under conditions of full employment are highly encouraging.

The next question is: Would "maximum practicable" migration rates applied to the study area during 1950-65 be sufficient to achieve the 1950 Iowa level? The answer is no. The application of such rates would reduce the farm labor force to 61,027 by 1965, still about 10,000 in excess of the labor force of 51,104 required to achieve the 1950 Iowa man-land ratio. Such a migration would result in a man-land ratio of 129.56 acres per farm worker in the case-study area, a level surpassing the 1940 Iowa ratio. This migration rate, expressed as a decennial rate, on a straight-line basis, would be 61.82 per cent, still some 10

or 15 per cent less than the net emigration rate necessary for achieving the 1950 Iowa man-land ratio.

Is there any evidence that the 1940-50 rates of emigration were lower than those experienced since that time? Such data as are available are not adequate for definitely answering this question. The information contained in the quinquennial census of agriculture [10, 1954, County Table 1] regarding the number of farm operators indicates a net reduction for approximately a 5-year period of 17.95 per cent for the study area and a comparable rate of 5.04 per cent for Iowa. These 5-year rates of reduction are greater than the comparable decennial rates for 1940-50, which were 14.36 and 4.76 per cent respectively. The presumption therefore is that the 1940-50 rates may well be surpassed, if not doubled, during 1950-60, and that the "maximum practicable" rates may well be exceeded. Although the inadequacy of coverage of the quinquennial census and the differing dates of enumeration of the quinquennial and decennial censuses may create an illusion of a more rapid rate of labor-input diminution during 1950-54 than during 1940-50, recent estimates of the farm population made by the Department of Agriculture support the view that reduction in the farm population continued at a very high rate between 1950 and 1957 [8]. On the other hand, conditions of less-than-full employment since 1957 may be expected to have slowed down the rate of reduction in farm population. However, if we accept J. H. Street's opinion [5, pp. 232, 241] that reverse labor inflows into cotton areas are unlikely, the effect of recent less-than-full urban employment may be simply to retard net outflow rather than to reverse the process for the study area.

It is clear from Figure 1 that the nonwhite proportion of the population of the case-study area is much greater than that for Iowa. This fact taken with the knowledge of relatively lower income earning opportunities for nonwhites in the nonagricultural sector of all major regions of the country could be used to argue against the possibility of an equilibrium equalization of the man-land ratio in the two areas. Although it is very difficult to explain the racial difference in nonagricultural incomes as an equilibrium one, let us assume that the existing racial differential in income-earning opportunities outside agriculture is somehow immutable. Under such an assumption, one could then argue that nonwhites would have a greater inducement to remain in agriculture than would whites so that, given the different racial composition of the farm sectors of the two areas, more individuals would remain in the case-study area per acre than would remain in Iowa. Such a condition would not, however, indicate an equilibrium within agriculture with respect to the two areas since it presumes that nonwhites
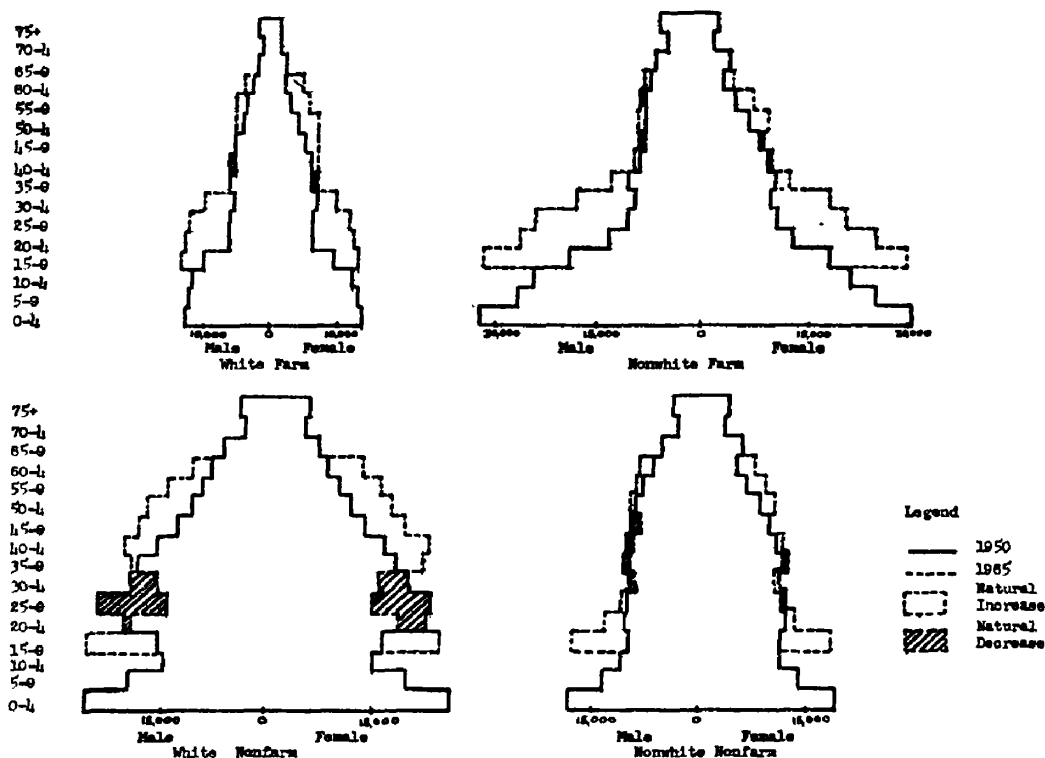
FIGURE 1—1950 POPULATION PYRAMIDS, WITH 1965 LABOR FORCE (ASSUMING NO MIGRATION) SUPERIMPOSED, MEMPHIS AREA

could somehow not move into Iowa agriculture to take advantage of superior agricultural opportunities there. As a matter of fact, the migration rates shown in Table 3 indicate higher rates of migration off farms for nonwhites than for whites in almost all categories. This would suggest that despite whatever racial segmentations of the nonagricultural labor force may exist, the differential between farm and nonfarm incomes is so great as to outweigh the effects of such a segmentation. Although the racial composition of the two areas complicates the analysis, it does not disturb the basis of my original assumptions.

## II. *Policy Recommendations*

Despite the encouragement afforded by the available data regarding the rapid reduction for both races of the study area's farm-labor input since 1950, the achievement of the 1950 Iowa level by 1965 is a far from likely possibility. The equalization of man-land ratios in the two areas, when we take into account the dynamic character of the Iowan adjustment, appears even more remote. Two alternative policy recommendations affecting farm migration present themselves. The first, simply to postpone eventual goals for equalization, carries with it a significant

pitfall. High birth rates in both farm and nonfarm areas during 1940-50 will shortly bring increasing numbers of entrants into the national labor force. Particularly since the net farm emigration of the last 19 years took place in a situation in which nonfarm entrants of nonfarm birth were relatively scarce, the farm sector was in a more favorable situation for the reduction of its redundant labor supply than it will be in the next 19 years. Because nonfarm entrants are already on the scene when an employment opportunity appears, they are likely to preempt many nonfarm job opportunities. For the study area, Figure 1 displays the actual population pyramids and the projected 1965 population on the assumption of no migration after 1950. It is clear that the competitive importance of nonfarm entrants will become increasingly great with the passage of time. Postponement of the eventual solution will thus make the solution more difficult.

A second policy recommendation is to encourage migration by methods ranging from subsidization of local industry and the provision of improved job information service to outright subsidy and supervision such as that provided to reservation Indians by the Bureau of Indian Affairs [14, p. 238]. The relatively low migration rates for adults aged 30-55 and for children suggests that family groups face considerable difficulty in financing migration, so that more adequate job information, migration and resettlement loans, or outright subsidy could markedly accelerate migration and simultaneously relieve farm areas of much of the economic burden of high replacement rates. An outright migration subsidy necessitates distinguishing those groups or individuals who would have migrated without subsidy from those who would migrate only with a subsidy, or else there would result a somewhat wasteful expenditure. The greatest impediment to direct policies to encourage migration seems not to be the magnitude of expenditures but real or imagined geographical conflicts of interest which may be expected to result in political opposition against any such program. Thus it has not proved possible to create a truly national job information service under the Rural Development Program, although the cost would be relatively small and the concept was an integral part of the originally conceived program [9, pp. 16-17] [7].[10]

The political palatability of local industrialization as a means of raising average farm and nonfarm incomes in the South generally has resulted in a considerable emphasis upon this means, and a de-emphasis of or an active opposition to the role of migration, particularly when the

[10] Although policies which endeavor to increase farm incomes through additions of farm capital, farm reorganization, and so on contain considerable promise, the likelihood of their solving the farm poverty problem without a reduction of farm-labor inputs is extremely remote.

latter involved interregional movement by whites. Although the problem of optimal location of industry, particularly in its dynamic aspects, lies well outside the scope of this paper, there is little doubt that local industrialization tends to minimize the private and social costs of movement vis-à-vis long-distance movement. There is a real question, however, as to the possibility of absorbing redundant farm labor locally.

We can gauge the prospects of local absorption in the case-study area if we regard the area as a closed economy with respect to net immigration of labor. Considering historical precedent, the bulk of new nonagricultural job opportunities within the area during 1950-65 are likely to develop in already existing urban centers with by far the greatest growth in Shelby county, containing Memphis. For most persons entering the Memphis labor market who had resided in the area's farm sector in 1950, an actual shift of residence is reasonably to be expected, considering the long distances involved and the objection of plantation managers to their tenants' engaging in part-time nonagricultural employment.

In order to estimate the power of urban centers to absorb farm-emigrating labor within the case-study area I have relied on Census data relating to gainful employment in nonagricultural industries for 1940 and 1950 [13, 1940, *2*, Tables 23, 27] [13, 1950, *2*, Tables 39, 49], extrapolated the 1940-50 experience to the 1950-65 period, and assumed that available jobs would be filled first by nonfarm residents. For these estimates, I excluded the female labor force, although its increase from 57,507 in 1940 to 100,080 by 1950, could, if maintained at this decennial 74 per cent rate, have very important effects; but these effects would not necessarily serve to help reduce the redundant male farm-labor force. The nonagricultural gainfully employed male labor force increased at a much lower decennial rate (20.65 per cent) from 156,597 in 1940 to 188,936 in 1950. If this rate of increase could be maintained until 1965, the nonagricultural male labor force would grow to 251,475. Of this number 218,012 would come from the 1950 nonagricultural labor force and the entry of 1950 nonfarm residents into the nonagricultural labor force as they came of working age. This leaves 33,463 potential job openings for farm migrants. Now, assuming that the number of gainfully employed to total potential labor force is the same for migrants as for the nonagricultural sector, the total absorption of male farm migrants during 1950-65 would amount to 42,096. Under the assumptions I have made there would be no net migration of either farm or nonfarm workers into the case-study area. Even with this protective assumption, it is apparent that the study area's farm migrants cannot depend upon local opportunities alone for nonfarm employment, but will be forced, if they maintain the same rate of migration as during

1940-50, to seek employment outside the case-study area. The required absorption (99,859) under this assumption of migration is more than twice the absorptive power indicated above. To achieve the 1950 Iowa man-land ratio by 1965 would involve the absorption of at least 165,000 male workers.

Is there any sign that the rate of increase of nonagricultural job formation in this area is increasing sufficiently to absorb this number of farm migrants? The answer to this question must certainly be no. Data computed from the *Census of Manufactures* [12, 1947, *3*, Table 2] [12, 1954, *3*, Table 3] indicate an increase of about 20,000 manufacturing jobs in the area between 1939 and 1947, an increase of approximately 72 per cent for 8 years. Between 1947 and 1954, the increased manufacturing employment amounted to somewhat less than 9,000, a rate of about 19 per cent for 7 years. During the period 1948-54, there was an increase of at least 7,202 persons employed in the area's wholesale, retail, and service industries enumerated by the Census [11, 1948 and 1954]. Although the sum of these two items amounts to a "job-creation" of 16,000 for both sexes during a 6- or 7-year period and does not include all nonagricultural employment (significant exclusions are public employment and construction), it is quite clear that the rate of job formation in this area is not proceeding rapidly enough to absorb potential and necessary farm migration from this area's agricultural sector. Indeed, if a rate of nonagricultural job-formation no higher than that observable for the 1947-54 period is maintained until 1965, the area will do well to find nonagricultural employment for the offspring of the present nonfarm population within the area.

The lagging rate of growth for this area is particularly serious because, unlike many an area of equivalent dimensions in the South, it already has a well-developed urban-industrial base in the metropolitan area of Memphis with its strategic locational advantages. In addition, a considerable portion of the area is within the state of Mississippi, which has had an active and overt subsidy program to support local industrialization for more than twenty years [4, p. 1]. It would appear that if this area cannot absorb its farm emigrants, there are few areas within the South which would be able to do so, particularly if we exclude the peripheral states of Florida, Maryland, and Texas.

## III. *Concluding Remarks*

Although the solution of the low-farm-income problem largely by means of farm emigration has come under fire politically and the Congress has not implemented certain aspects of the Rural Development Program relating to farm emigration, there are a number of advantages

in unsubsidized migration. The primary advantage is that it requires no direct government intervention; it directly rewards individual initiative on the part of people whose resources are much too small to permit effective farm reorganization within agriculture or to establish a local nonagricultural business. The second advantage is that induced changes take place immediately without delaying for legislative or other governmental action. A third advantage is that there is no direct cost to any governmental unit.

The magnitude of the ameliorative influence of migration for the low-farm-income (and labor factor reward) problem obtained and obtainable through unsubsidized migration seems to have been considerably neglected. If we accept the estimates of net migration out of the South during 1940-50 as 1,500,000 persons and the net migration from farms to urban centers within the South during the same period as 5,000,000,[11] we find an appreciable explanation of rising farm incomes per worker within the Southern region. The migration of 6,500,000 people in 10 years constitutes a dramatic vindication of the position that the primary step in the solution of the low farm-income problem is the maintenance of full employment in urban centers. More specific and more direct programs aimed at encouraging migration face continuing political opposition in the labor-exporting areas, whether such migration be intra- or interstate. Efforts to encourage local industrialization within the case-study area have achieved results which are evidently far from the magnitude required to absorb natural increase within the area and to raise agricultural labor productivity level to the standards found in the country's nonagricultural industries.

[11] Estimates presented by D. Gale Johnson before the 27th Annual Conference of the So. Econ. Assn., Memphis, Nov. 7, 1957.

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# THE THEORY OF PUBLIC FINANCE[1]

## *A Review Article*

*By* CARL S. SHOUP*

The past thirty years have witnessed a notable growth in the pure theory of public finance, if we take that term in Edgeworth's sense: "deduction from received first principles," with "sufficient length of reasoning and strength of premises to deserve the title, 'pure'."[2] But the contributions have appeared, for the most part, as journal articles, papers in proceedings, essays, and chapters in books not devoted to public finance. The authors have been numerous. And they have not always sought to relate their findings to the pure theory of an earlier century or another school. For teachers and students in public finance the problem of extracting something more than a minimum benefit from the scattered material has become a discouraging one. The great task of assembly, organization, and critique has been so formidable, one hardly dared hope that it would be accomplished. But now the goal has been achieved, by Professor Musgrave, in a treatise that is a major contribution to public finance thought. We are afforded the opportunity of comprehending the scattered contributions and understanding their interconnections, guided by close reasoning and a precise but sympathetic evaluation.

Moreover, the analysis, to which Musgrave himself has contributed so much in the past, is extended still further in the present volume, chiefly through a remarkable power of combining questions that have hitherto been posed separately, and then pressing on to answers.

The four parts into which the book is divided can be distinguished by their degrees of normative content. Part I is a discussion of the goals of public finance policy, subsumed under allocation, distribution, and stabilization: decision-making is imagined to occur within a multiple budget organization with branches devoted to each one of these ends. The value of this device is intellectual, not bureaucratic; whether or not we can conceive of any country's trying to set up these three branches as distinct entities, we can benefit from the author's warning not to muddle our aims. Even if allocation, distribution, and stabilization are viewed as effects to be described, not goals to be pursued, intellectual benefits accrue from an insistence on continually distinguishing among them.

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[1] R. A. Musgrave, *The Theory of Public Finance—A Study in Public Economy*. New York: McGraw-Hill, 1959. Pp. xvii, 628. $12.50.

[2] F. Y. Edgeworth, "The Pure Theory of Taxation," in *Papers Relating to Political Economy*, London 1925, II, p. 100.

From Part I we get the impression that the rest of the book will be heavily normative: "we determine the optimal budget plan on the basis of initially defined conditions and see how it can be achieved" (p. 4); "the economist . . . must determine what expenditures [on social wants] should be made and what taxes should be collected" (p. 10). The allocation goals and the distribution goals are to be determined by the analysis in Part II, which applies "welfare economics to the issues of budget determination" (p. vii). For, assuming "a full-employment economy, the task of budget policy is to contribute to an efficient allocation of resources and a 'proper' distribution of income" (p. vii). The goal of the stabilization branch is of a different order: it is known at once, for it is simply to stabilize (at full employment). This goal is therefore accorded no such extended normative discussion as Part II provides for distribution and allocation. Consequently, Parts III and IV can be devoted to implementive analysis, designed to show how the three sets of goals can be achieved simultaneously, or if not, why not.

As it turns out, the rigorous analysis of Part II demonstrates that there are no very specific allocation and distribution goals that can be recommended for universal acceptance in the way that stabilization—or at least full employment (by definition alone)—can be recommended. Part III, therefore, may be read more or less detached from Part II. Part III is said to be devoted to answering the questions: "If a given tax or expenditure measure is put into effect, what will be the resulting changes and adjustments in the private sector? Or, allowing for these adjustments, what fiscal measures must be put into effect to reach the desired objectives?" (p. 205). Of these two questions, the first is much more the subject of Part III than the second.

Part IV is a fruitful mixture of the normative and implementive, or at least of implementive analysis directed to a fairly well defined goal. As Part IV progresses into problems of growth, however, distributive and allocative aims, especially the latter, again assume importance; "there is the . . . problem of choosing between various rates of growth" (p. 483). Thus Part C of Chapter 20, like the topics of Part II, necessarily lacks the specific objective that supports most of the rest of Part IV in a normative framework.

Within this four-part structure, what is it that Musgrave is attempting to do, and how well do circumstances allow him to succeed? At least three aims seem to emerge.

## I. *The Theoretical Structure*

First, of course, and overriding all the other aims when necessary, is that of supplying a body of theory, public finance theory. But theory in what sense? Not as an explanation of how we got where we are, and where we are likely to go; the "sociology of fiscal politics" is "interesting and important" (p. 4), but it is not the theme of the present treatise. Nor is the theory a series of generalizations taking the form of: the following data are not inconsistent with the hypothesis that . . . The book contains many tables, but no statistics. Does the theory deal with economic consequences that are at least defined, even if not in fact measured, in terms of measures that are operational, "that is, based on observable data?" (p. 206). Musgrave implies

that it does, yet he correctly points out that the method employed is that of comparative statics, where "incidence and output effects are determined by comparing the initial equilibrium position of the economy (prior to the change in budget policy) with the new equilibrium position (reached after all adjustments to the change in budget policy are completed)" (p. 209).

More precisely, we may say that the comparison is between two states of the economy, both at the same time and place; putting it this way would make it clear that at least one of the two states being compared must be imaginary, hypothetical, not discernible from any observable data. The implication that this is so is carried by the immediately succeeding sentence, "Time does not enter, and the adjustment is assumed to be instantaneous." But the next three paragraphs seem to reflect a different view. Noting that "The actual adjustment process takes time" (p. 209), Musgrave remarks that: "Instead of comparing the initial position of the economy with that after an infinite period of adjustment has passed, we may compare it with the position after a month or a year" (p. 210). But the appropriate comparison would seem to be between the economy as it would be a month from now if the budget measure had not been passed, all other things equal, and the economy as it actually is a month from now, under the budget measure. There is still no chance, following the methods of this book, to compare two sets of observable data. There is nothing, for example, on time series, nor should there be, given the general cast of the treatise.

The theory of *The Theory of Public Finance* is the familiar mixture of logic and introspection, grounded on assumptions that seem reasonably in accord with the economic life around us. For example, in Chapter 17 the analysis works with alternative combinations of assumptions about presence or absence of a money-wage-rate floor and an interest-rate floor (p. 416). The author thus presents us with a series of logically oriented worlds, some large and complex, some small and simple, in which sets of postulates are seen to imply certain consequences.

If the author's aim has been correctly interpreted here, he has succeeded to a high degree in the task he set for himself. Given the axioms—and it must be recalled that the sets necessarily shift somewhat from chapter to chapter, or even from section to section—the reasoning proceeds, incisive and almost always dependable, with scarcely a break for the full 615 pages. Indeed, no reader not already quite familiar with the materials covered here will find the volume anything but tiring after a bit, with its relentless demand on his attention and his reasoning power. The remedy, of course, is to take this concentrated nourishment in appropriately sized portions.

### A. *Characteristics of the Theory*

The main offering in this logical feast is the extraordinary combination of various postulates in a variety of patterns, with a consequent breadth of coverage not hitherto approached in any volume on theory in public finance. This is seen both in the large and in the small, both in the skill with which the great blocks of analysis are laid one upon the other to form a complex whole, and in the listing of all possible combinations within a particular problem.

The first of these features, giving the book its strong architectural sense,

can be noted most readily by reading the opening paragraphs of successive chapters (within a Part), and even of successive major sections within a chapter: for example, Part IV, where only Chapters 21 and 23 cannot fit easily into the orderly sequence. It is also seen in the recurrent appeal to the three norms of distribution, allocation, and stabilization.

Typical examples of the methodical exploration of a particular problem are the analyses on pages 348-55, Relationship between General and Discriminatory Taxes, and the table on page 527, classifying various budget policies by their effect on the structure of claims. Some reservation concerning this kind of elaboration will be offered below, but to begin with, let us consider the advantages.

First, we can be sure that the author is not neglecting certain combinations merely because they happen to fall outside his own policy predilections. So much of recent writings, especially on fiscal policy, can be adequately appraised only if the reader continually reminds himself, often at the cost of considerable effort, what it is that has been left out. Musgrave has his views, too, on policy, but they are presented only with the full range of alternatives in sight. A refreshing freedom from dogmatism allows the discussion to end up occasionally in a frustrating uncertainty, when that is just where it should end up. A sophisticated detachment from little maxims and secondary aims averts narrow prescriptions. A good illustration is the way in which Musgrave handles the question of preference among taxes that vary in their resource-releasing power per dollar of yield (pp. 520-22). The common assumption that the best tax to fight inflation is the one that cuts private spending the most per dollar of revenue is shown to be dependent on implicit secondary goals that may or may not be acceptable.

Secondly, we are required, by Musgrave's systematic approach, to recognize the full implications of a classificatory scheme to which we may have committed ourselves without a strong exploratory urge. A good example is the analysis already referred to, on pages 348-55. Musgrave here organizes our thoughts for us in a comprehensive classification of general and discriminatory taxes, according as the taxes are imposed on the purchase or on the sale, of all or some factors or products, by all or some buyers or sellers. And he places in each of the pigeonholes a well-known tax as an illustration, except that, as might be expected, the complete list of possible combinations outruns the real world, so that boxes 11 and 16 remain empty. The value of this intellectual exercise is considerable. For instance, even textbooks devoted to economic theory rather than public finance point out that a change in the legal impact of a tax from the buyers' side of the market to the sellers' side does not alter the net prices received or the amount of product transferred, but they do not extend the theorem to a tax on purchase of factors compared with a tax on sale of product. Musgrave leads the reader through the circuit of production and consumption and demonstrates the equivalence of the general-scope taxes levied at any one of the points in the circuit. He also reminds us that for such equivalence, the rate of an ad valorem tax must be higher when applied to the amount paid (exclusive of the tax itself) than when levied on the seller, and states the requirement neatly in mathematical form.

It all seems so reasonable, even so familiar, that some reflection may be

necessary before we recognize how substantial is this contribution coming from the author's powers of comprehending and classifying. The scattered taxes fall into place in a new and intellectually satisfying pattern. Even the limitation set to the field of combinations is characteristic. Some might wish that the area of classification had been extended in order to contrast the general turnover tax (*Umsatzsteuer*) with the national-income type of tax, *e.g.*, the pure retail sales tax; but such an expansion would have blurred the clear, precise borders within which the combinations are evolved. For Musgrave's purposes, at this point, the area covered and the classifications discovered are, one feels, just right.

Another illustration of this power of analytical grouping is supplied by the assembly of systems of income determination *a* to *f* in Table 17-1, and the analysis of each case in the accompanying pages. Still other examples could be cited, especially from Part IV. Indeed, the chapters on compensatory finance are probably the best in the book, in terms of what they set out to do. They begin, however, on a rather more advanced level than most of the chapters in Parts I to III, and the reader may want to complement Musgrave's analysis with the slightly different approach in the summary essay by Samuelson.[8] At any rate, Musgrave's Part IV is the epitome of the aggregative approach to business-cycle fiscal policy; and it must share the adverse comments on that approach: no details of business cycle theory or description, no attention to turning points, no consideration of almost inevitable instabilities in this or that sector of private enterprise. Thus the conclusion that the level of public expenditures on goods and services should not be changed to stabilize aggregate demand (p. 517) cannot be supported if, despite all measures short of inflation, the private construction industry is bound to be unstable. Indeed, the relative concentration of unemployment during the last recession in geographical and industrial pockets suggests that fiscal policy in the future may deal much more than in the past with measures aimed at particular segments of the economy. Musgrave is of course aware of these issues (see p. 518); but they do not fall within his framework of detailed analysis, or rather are considered to be only "important exceptions."

Now and then the material proves too refractory, the task of completing the structure too great; it appears that a chapter (promised on p. 348) to deal with incidence in a macroeconomic setting that "combines growth with the need for compensatory policies" did not get written, unless Chapter 20 is thought of as meeting this problem. More serious, one important class of budget effects is omitted purposely (see p. 214, note 1): the distribution of benefits represented by government services for which no direct charge is made. Incidence, defined as a "change in the distribution of income available for private use" (p. 207), does not include change in the distribution of income in kind received from government. Thus the attempts made by Barna, Brochier, Adler and Schlesinger, and Cartter to measure this phenomenon are not referred to (their names do not appear in the index). This omission weakens much of the discussion of measurement of global incidence on pages

[8] "The Simple Mathematics of Income Determination," in *Income, Employment and Public Policy, Essays in Honor of Alvin H. Hansen*, New York 1948.

223-26 and 229. In Chapter 13, the effects of budget policies on activity in the private sector are similarly analyzed without reference to households' or firms' reactions to the receipt of free services from the government; and only occasional references are made elsewhere (*e.g.*, pp. 254, 336). Finally, the tone of the discussion of public expenditures is profoundly affected by the refusal to consider that public services like free education owe their existence to a redistributive drive as much as to the motives Musgrave lists for the "Merit wants" (pp. 13-14). The intellectual gain from distinguishing among the three goals of redistribution, allocation, and stabilization can be converted into a penalty if it induces us to set off in a corner, as something to be achieved only through positive or negative taxes, a state of distribution that is "proper." For example: in an underdeveloped country where half the populace are illiterate, one of the initial steps for redistributing income will have to be the supplying of free education to all; the effects of any cash redistribution would soon be dissipated otherwise.

Musgrave's theoretical contributions in this volume are not limited to devising new problems from classificatory combinations, and solving at least some of them; he also fruitfully extends the analysis of some quite familiar puzzles. There are many instances of this; one of the best concerns the incidence of a general consumers sales tax (pp. 379-82). In just a few paragraphs Musgrave disentangles the issues neatly and demonstrates that the question of shifting forward to consumers or backward to factors resolves itself into one of consumers against savers under a differential incidence approach (stabilization at full employment is assumed at this point).

In addition, of course, there are the many theoretical issues on which Musgrave has already made significant contributions in journal articles and elsewhere, and which appear again in this volume, sometimes with an acknowledged change of view (p. 74, note 1), sometimes holding to the conclusions already reached (as in the risk analysis, to be discussed below).

B. *Some Problems for the Reader*

The consumer of this treatise is sure of reaping substantial rewards, if he is as careful and thorough as the author has been. But he will be faced with certain costs of consumption, some of which he may resent.

First, the repeated attempts to exhaust the possibilities of combination inherent in a set of postulates weary the reader; some will judge the price too high, in terms of time, effort, and persistence, as they find themselves looking forward to the end of the analysis rather than enjoying the middle. The price seems doubly steep when no data, not even rough estimates, are on the horizon, to give substance to the more complex combinations. Second, a great deal of the analysis concludes that no general theorem can be deduced after all, or that only well-known theorems hold. Such information is useful, but makes for tedious reading.

Third, where the reasoning is simple or at least familiar, and hence can be fashioned without much difficulty in concrete research at that moment when it is called for, not much is gained by working through the analysis in abstract terms; the workman on the site will not need prefabricated parts. And fourth,

the absorption of space and effort in setting forth all the possible combinations occasionally forces a degree of sketchiness in content that might have been avoided by a more selective and more intensive treatment. This last point can be illustrated by the paragraphs on compulsion in debt policy (pp. 586-87); the first three, by the elaboration of a section already noted above for its usefulness in setting up the classifications (pp. 355-64). The question at issue is whether the author has heeded sufficiently his own warning (p. 364) that "little would be gained by a mere cataloguing of possible results." Admittedly, it is hard to know where to draw the line. But in some instances a more useful procedure might have been to supply the reader with a tool kit, to show him how to formulate problems by bringing together hitherto unassociated postulates, then to turn him loose to work on his own. Instead, he is sometimes crammed, as it were, with a long series of formal answers in place of a sympathetic and imaginative counseling on the construction of problems and the use of tools.

An inevitable result, in these stretches of the volume, is a certain flatness of style and occasionally a taxonomic drive that produces a terminology too cumbersome for general use, as in Table 22-2. By the time the reader arrives at policy 22 (the identity with Chapter 22 is coincidental), he will probably conclude that he has benefited from the discipline of classification, but that he will prefer to speak, as does Table 22-1, of a deficit financed by new money rather than of a mixed fiscal and liquidity policy, fiscal-and-pure-monetary-policy variant. The three full paragraphs on page 530 illustrate the mechanical mode of instruction in its least appealing form (compare, for instance, Lerner, in his "Burden of the National Debt," in *Essays in Honor of Alvin H. Hansen*). The technique is appropriate enough if the aim is to supply a handbook for reference in the field, although some reservations on this score are offered below. But even this aim seems not well served by the excessive formalization that characterizes Chapter 3, "Problems of Implementation."

Another result is a sometimes disconcerting degree of compression of analysis, as statement of question is followed directly by answer. The reader is then left the task of unearthing a good deal of implicit reasoning. An example is the brief treatment given to correlated commodities under taxation (pp. 301-2). So too is the string of conclusions following the table on page 527. Sometimes the suppression of explicit statement extends even to the conclusions themselves, as in the treatment of monopoly taxation without restriction to linear functions (unit tax). We are left with the hint that "the monopoly case involves second-order terms as well" (p. 297); the reader must decide for himself whether this means that the increase in the monopolist's price, consequent upon the tax, still varies directly with the slope of the demand curve, as it is said to do in the linear case. (The answer is no; and in fact the statement does not hold even with a linear demand curve, since a change in the slope changes the pretax price, a fact to which the author appears to attach only an ambiguous importance in a paragraph of text and two footnotes, pp. 289-90.)

Disagreement with Musgrave on substantive issues will probably prove to be quite limited, in view of the evident care with which the theorems have been developed. A few questions, some perhaps more of terminology than substance, are offered here:

1. In Chapter 14, as in the well-known paper by Domar and Musgrave, the investor distributing a given amount of funds among two ventures and cash is viewed as finding what for him is the optimum combination of yield and risk. The analysis abstracts from degree of dispersion within the gain component of yield as long as the actuarial value of the gain is unaltered (the same is true with the loss, or risk, component). On the yield axis, one point will cover both a safe 4 per cent yield and a lottery bond that assures return of capital and gives 1 chance in 500 of 2,000 per cent interest Any single investor, however, would prefer one to the other. And if the investor were in fact indifferent as between these two opportunities in the absence of a tax, he would not probably remain so under a tax of say 50 per cent on gains. The text, terming $y$ first yield then income, moves on to discuss the income effect of a tax, but the implied analogy with the income effect of a wages tax seems inexact in view of the difference between income and yield. The terminology invites an inference that if a poll tax were levied on an investor (win or lose), he would be sure to respond by taking more chances.

2. The argument concerning the effect of investment income on work response to an income tax (pp. 246-48) is difficult to follow. If, to taxed wage income, there is added untaxed investment income, the marginal rate of tax on the wage earnings from another hour of work is no less than before, while the income effect is weakened by the presence of more income (total tax is unchanged). Hence the amount of work supplied would decrease (but cf. par. 3, p. 246, and par. 5, p. 247). If "the presence of capital income" is taken to mean that for some part of the wage income there is substituted an equal amount of capital income, the income effect of a given amount of tax is unchanged, but so too is the marginal rate of tax on wages, unless the tax schedule is progressive.

3. To obtain a desired increase in income, "On the one extreme, we may increase expenditures and taxes by an equal amount" (p. 432). But the true extreme lies beyond this point, in the region where the increment of expenditure is less than the increment of tax revenue. Income increases, provided the increment in expenditure is greater than the product of the marginal propensity to consume and the increment in tax revenue ($\Delta G > c\Delta T$).

4. The asymmetry prevalent in almost all discussions of depression and inflation reappears in Musgrave's book, though he comes close to pointing it out. In discussing depression policy, he says (p. 445), "we departed from a position of unemployment and then examined [1] adjustments designed to secure a given increase in income. . . . Thus for inflation control, we can examine [2] the necessary steps to arrest an inflation in process; . . ." (numerals inserted). But [1] and [2] are not parallel cases; in [1], Y is steady, and we want to raise it to a given level (full employment); in [2], Y is rising, and we want to steady it. The closest parallel to [1] would be knocking down prices to their pre-inflation level while maintaining full employment, but this goal is not considered even in theory by Musgrave, probably because of an assumption throughout, implicit or explicit, that money wages are rigid in a downward direction (*e.g.*, note 1, p. 448).

5. "While it may be possible to secure expeditious action by Congress when the need is for reduction in tax rates, it may not be possible when an increase

is needed" (p. 502). This statement may be classed with the other commonsense observation that tax increases are not voted in an election year: both, incredibly, are belied by evidence (the first, in the '50's; the second, in the '30's).

6. "If the statutory-rate structure is progressive, the ratio $t$ of tax yield $T$ to tax base $B$ rises as the tax base increases" (p. 506). This is so only if the increment in tax base accrues to the higher brackets in a greater proportion than obtained for the initial tax base. Thus it is conceivable that the elasticity $E_t$ may be less than 1 for a tax with a progressive rate schedule. But perhaps the only issue here is the definition of a progressive tax.

7. The balanced-budget theorem is presented as something that is now generally accepted for every-day use, and the almost parenthetical warning that this is "only a very special case" (p. 432) seems to be set to one side in later analysis (*e.g.*, p. 524). Moreover, entirely aside from the weighty conditions that Musgrave lists in his footnote, there remain some awkward questions of necessary change in money stock or transactions velocity that are inherent in the theorem, which, in some versions, calls for a kind of frictionless, instantaneous passage of funds from taxpayer to government. In addition, the approximately full effect of the balanced budget increase is not achieved until a number of periods have passed, except for the remote possibility that the government expenditure on goods and services may be wedged into the timestream quickly enough to put its recipients of wage and profits on the same time schedule of respending as the taxpayer himself.

The high level of theoretical argument in the treatise is unhappily marred by mechanical failure in many spots in the form of printer's errors or slips of the pen. Especially bothersome are the reversals of "decline" and "rise," "elastic" and "inelastic," and similar couples, and the occasional omission of the negative. Those who purchased the first printing will want to obtain the mimeographed errata sheet, but this list does not catch all the slips by a considerable margin. There are at least two dozen more each of typographical and author's errors, to which are added certain inaccuracies in quotation, to be noted below.

## II. *Problems of Practical Application*

A second aim of Musgrave's treatise, not perhaps easily to be separated from the first, appears to be to supply theory of a kind that can be directly applied to concrete cases, to help obtain answers for a given time and place. At least this aim is indicated in the preface, and in the passage to be quoted below. Here, the success achieved is severely limited. The difficulty lies in the choice of problems. The issue is a familiar one, and has already been touched on, above, but seems worth some elaboration.

If the author's aim is to supply us with a technical handbook, to which we may turn for an answer when we deal with problems in the field, one can only say that the difficulties encountered in making use of the volume in this way are insuperable. The world is too complex to allow us to be reasonably well covered even by the multitude of combinations offered in this book. Moreover, as already noted, many of the combinations of postulates are not yet readily observable, or at least not quantifiable, in the real world. We have only

to look at the testimony given in recent years before the Joint Economic Committee, or at the symposia on current depressions or inflations in scholarly journals, to observe how little use is made of the formulae available in the handbook. It seems not sufficient to invoke the fact that "considerations of incidence play an important part in the immediate needs of tax legislation, and properly so" (p. 364); there would appear to be little point in presenting to the legislator a list of "unverified hypotheses" just because they concern important problems. Nor does the argument seem to be well supported by the remainder of the same sentence: "this being the case, the social scientist, unlike the astronomer, cannot postpone judgment [answers?] until a wholly conclusive proof can be given." But although astronomers may, in some sense, feel free to act in this way, they in fact do not; and if they did, the admonition might better be reversed; for it may be argued that mistakes by astronomers (abstracting from the space age) have less serious consequences for mankind than mistakes by economists—except that the public listens to astronomers.

Thus, with respect to the second aim here attributed to the book, much of the powerful and sometimes intricate reasoning of Parts III and IV is largely unusable; it does not help us to help the legislator move closer to generally accepted goals such as full employment, a stable price level, perhaps a reduction in the degree of inequality of income, and so on.

Are the prospects better for the years ahead? The lack of usefulness, so far, of many of the propositions may be due to the fact that the task of setting up the theory is not yet complete; perhaps a few more years, or decades, of continued thought will finally produce the engine that we need. Or, the theory itself is reasonably complete, and it is only a matter of time before the quantitative material that needs to be fed into the analytical machine will become available. But there is another view, more pessimistic, which doubts that the bulk of the refinements in the present-day economics of public finance will ever prove usable in specific cases. The thesis may be illustrated somewhat as follows on the macroeconomic side. In the 'twenties and in the opening years of the 'thirties, the generally accepted economics of public finance was a weak mixture of unconnected conjectures, backed by a muddled state of mind that resulted from drawing implicit macroeconomic conclusions from microeconomic reasoning. Congress in 1932 enacted the largest increase in income tax rates that the country had ever known, not excepting the first world war, and no article appeared in any journal of economics or public finance condemning that action. Kahn, and Keynes, and Hansen, and others, then turned us around, and set us in the right direction. This was a sufficiently great achievement. Yet there is a tendency to believe that much more has been accomplished. Tax rates should not be raised in a depression, but by how much, and when, should they be lowered? There has been, for example, no open discussion in economic terms of why the entire tax system should not be suspended when recession develops, to be set in motion again only when full employment is regained. For the time being, there seems little prospect of reliable advice that will embody much of the more refined, implicitly quantitative, propositions, as distinguished from directional prescriptions, that make up so large a part of the present volume.

Nor are things much better, for policy purposes, on the microeconomic level, the subject of Part III. What is known, so much better than before—and this knowledge is clearly set forth in Musgrave's elegant presentation—is how many different outcomes are possible. Thus the income tax, as shown by von Mering and Cooper, among others, may increase or may decrease the supply of work, but we do not yet have more than a hint of the particular rate scale that will prove close to the border line, and no way is suggested of finding out how far the present rate scale pulls in one direction or the other. Again, what is actually happening today, in the United States, under the corporation income tax (relative to some other tax) remains about as great a mystery as ever.

If these familiar misgivings are aired once again, it is certainly not in criticism of Musgrave for having written the treatise he has. Who can be sure which road will be the most fruitful? And in any event, there are the indisputable advantages of thinking through this kind of theory. We can discard theorems now found to be erected on conflicting assumptions or with faulty logic, and ready our minds to seize on significant relationships as we scan the recorded data. The aim of these remarks is only to suggest that since the revolution in macroeconomics has now thoroughly penetrated public finance, and since the incisive work of Edgeworth and Pigou has been filled in around the corners and the bends, there is now occasion, more so than ever before, to develop some relatively neglected paths in the theory of public finance. Some trails may start deep in the tangled thicket of facts. The incidence of the corporation income tax, for example, may be studied, in a very tentative and even fragmentary way, by trying to link the phenomena of changes in the corporation income tax rate with the pricing practices of say, the fifty largest corporations in the country. Other paths may lead into new branches of fairly abstract theory: for example, the "shifting and incidence," or their analogues, of free government services. Still others may be explored jointly by economics departments and law schools in a truly intensive search for ways to restore horizontal equity to the structure of a tax system that has been warped by pressures from particular interests. There remains, too, the neglected task of isolating the forces that have shaped public finance systems, and predicting what the pressures ahead will lead to. All of these approaches will produce theory for public finance.

### III. *A Guide to the Literature*

Another aim of Musgrave's treatise is to indicate, sometimes to chart, the intellectual development of a theorem, without attempting a formal history of doctrine. The result is a notably useful guide to the literature; it enables those of us who lack the author's breadth of scholarship to turn at once to the relevant source for further study and comparison. Despite the large number of references, there is no overcitation; the reader's response is that, if only he had time, not to mention the language skills, he should read everything that this treatise calls to his attention. An understanding yet objective appraisal informs the volume throughout; issues are separated neatly and linked in an enlightening manner with Musgrave's own framework: see, for example, the

penetrating comment on Lindahl's conclusion regarding the ability-to-pay principle (p. 78). Independent discoveries of the same proposition are noted and distinguished (*e.g.*, Bowen and Lindahl, p. 75). These passages of critique, scattered throughout the volume, reveal the essence and demonstrate the limitations of doctrines and theorems. There is nothing in the literature of public finance, and very little even in that of economics, that matches Musgrave's combination of analytical appraisal and appreciative understanding of the theoretical contributions of others.

A scattered checking of quotations reveals that the reader who desires a formal, scholarly, exactness will feel obliged to go to the originals. Indeed, even the sense itself is occasionally distorted by typographical errors or carelessness in transcription. A few substantive slips or inadequacies must be watched for: Ricardo was in fact explicit in condemning a special tax on land, just on grounds of inequity (cf. p. 158); the brief comment on Shackle's theory does it much less than justice (p. 331); Edgeworth himself extended his paradox to the competitive case (see Vol. II, pp. 63, 122-24, in his *Papers*), though Hotelling's later independent discovery developed the extension much further (cf. p. 302); and the distinguished authors who argued against double taxation of saving do not deserve the aside, "strangely enough," for they chiefly objected on grounds of allocation (cf. p. 161).

### IV. *Concluding Remarks*

On the whole, the volume is addressed to the nonmathematical reader. For the first 400 pages, and in the last two chapters, geometry and algebra occasionally supplement or extend the prose analysis, without resort to the calculus save in a few instances, most of them in footnotes. The chapters on compensating tax and expenditure policy, and liquidity aspects of fiscal policy, make use of the differential calculus, but it is kept to as simple a level as possible; even without this equipment the reader will gain a good deal, if he is willing to take the conclusions on faith. This he may safely do, granted the postulates, for, in contrast to the literary inaccuracies commented on above, the statements in algebra and the calculus, as far as a scattered checking shows, are free of logical or typographical errors. One minor reservation may be offered, with respect to the system of equations on pages 411-12, where it might have been helpful to distinguish the labor-supply function (17-11) from the labor-demand function (17-6) by appropriate subscripts and then explicitly equate the two. Chapter 20 and in part Chapter 21, dealing with problems of growth, demand an understanding of difference equations.

Musgrave's book is so rich in analytical content, and covers so vast an area, that no review, even one as lengthy as this, can give an adequate impression of the author's achievement. The particular issues singled out for comment here could be matched many times over by quite distinct sets, depending on the reviewer's interests and range of competence. The treatise is one that yields continued intellectual dividends on successive readings.

# COMMUNICATIONS

## Soviet and American Inventory-Output Ratios Once Again

R. W. Campbell in a recent issue of this *Review* examines the claim that the Soviet system "is more economical of working capital than is the capitalist system." He comes to the conclusion that "the Soviet system requires higher stocks in relation to flows than does the U.S. economy" [1, p. 565]. However, it is well known that the USSR possesses a woefully inadequate transportation system, has to cope with a very unfavorable location of many basic raw materials, and suffers from a lack of skilled administrative personnel. Therefore, it is conceivable that the inferiority discovered by Campbell is really the inferiority of the USSR and not that of the Soviet system.

TABLE 1—SALES-INVENTORY RATIOS IN SELECTED BRANCHES OF RETAIL TRADE IN THE CSR, 1935–1937 AND 1954–1956

| Item | 1935 | 1936 | 1937 | 1954 | 1955 | 1956 |
|---|---|---|---|---|---|---|
| 1. Foodstuffs, department stores | 6.0 | 5.9 | 5.9 | 11.6 | 13.0 | 14.2 |
| 2. Cooperative stores | 6.8 | 7.6 | 7.4 | 5.4 | 5.6 | 5.4 |
| 3. Textiles, shoes | 4.5 | 4.4 | 4.5 | 3.9 | 3.9 | 3.9 |
| 4. Leather goods, toys, drugs, paper products | 3.2 | 3.2 | 3.1 | 3.5 | 4.0 | 4.2 |
| 5. Porcelain, glass, household goods, hardware goods | 2.2 | 2.3 | 2.7 | 3.6 | 3.8 | 3.8 |
| 6. Average ratio, 1956 trade structure | 5.7 | 5.9 | 5.8 | 7.6 | 8.2 | 8.7 |
| 7. Sales, line 1–5, as percentage of total retail sales | — | — | — | — | — | 69.4 |

*Sources:* Lines 1–5: [2, pp. 19–21].
Line 6: Weighted average of lines 1–5.
Line 7: [3, p. 188].

It may be interesting to check his conclusions by an analysis of the performance of some better endowed Soviet-type economy. The Czechoslovak Republic (further referred to as CSR) will well serve this purpose. Already before the war, this was an "advanced industrial" community, with an excellent transportation system and well distributed productive facilities.[1] Thus, in this case, the conditions will be somewhat more favorable for a fair test of the performance of the Soviet-type economic system and for a comparison with the United States.

Some of the official CSR figures (Table 1) indicate that the planners are better able to economize resources devoted to inventories than is the market economy. Leaving aside for a moment the absolute postwar level of the sales-inventory ratio, we may ask what is the explanation for the sharp increases in the ratio in 1954-56 as compared with 1935-37. Part of the explanation may be a change in the proportion of the total stock carried by the wholesaler.

[1] With a population of less than 14 million people Czechoslovakia today stands eleventh in the world in the production of electricity, hard coal, and pig iron; ninth in the production of steel and passenger cars [6, pp. 439-44].

While data necessary for direct comparison are lacking, the relative "inefficiency" of the Czech wholesale organization (Table 3) points in this direction. However, it is conceivable that at least part of the improvement shown in Table 1 is genuine. After the beginning of the war there ocurred a drastic reduction in the total number of retail enterprises, most of which was accomplished by the German occupants and part of which was accomplished by the Czech planners (Table 2). If the data in Table 1 can be trusted, this would suggest that economies of large-scale retailing may have been of some importance in the 44 per cent increase in the sales-inventory ratio.

Should the postwar performance reported in Table 1 be typical, the Czech ratios of retail sales to retail-held inventories compare favorably with both those of the USSR and of the United States (Table 3). However, Table 1 and the first part of Table 3 have two shortcomings. The concentrations on *selected* branches of the Czech retail trade and the omission of information about the remaining 30 per cent of retail sales give rise to a suspicion that the official statistician selected those branches which bear out his bias. In addition, only inventories held by the retail organization are shown. To make a judgment, one has to know in addition the ratio of retail sales to wholesale inventories and to total inventories. Fortunately, such data exist. I present them also in Table 3, along with the figures prepared for the United States and the USSR. The table suggests that the performance of Czech wholesale trade is sharply below the performance of Russian wholesale trade; sufficiently below to offset the advantage on the retail level and to lead to an inferior over-all performance, as shown in the last section of Table 3. Two explanations may be offered. Travelers in the USSR report recurrent shortages of consumer goods; such shortages are rare in Czechoslovakia. In so far as they occur, they tend to inflate the turnover of inventories. Similarly, reports indicate that the choice available to the Czech consumer is superior to the choice available to the Russians. If true, this would suggest that the turnover of inventories is lower because the task faced by the Czechs is more difficult.[2]

TABLE 2—NUMBER OF RETAIL STORES IN CSR, SELECTED YEARS

| Year | All Retail Stores | Food Retail Stores |
|---|---|---|
| 1930 | 200,233[a] | 72,345[b] |
| 1932–1936 | +122,731[c] | — |
| 1949 | 105,895[d] | — |
| 1952 | 94,258[e] | 34,417[e] |
| 1957 | 78,675[e] | 30,725[e] |

[a] [4, p. 175].
[b] [5, p. 239].
[c] [5, p. 239]. (Only data on absolute changes in the total are available.)
[d] [3, p. 183].
[e] [6, p. 294].

[2] This interpretation is supported by the ratios of the turnover of nonfood products to the turnover of food products. In the USSR, this ratio was .42 in 1950, .36 in 1945, and .28 in 1955 [1, p. 564]. In the CSR, the same ratio was .17 in 1952, .15 in 1953, .18 in 1954, .21 in 1955, and .21 in 1956 [7, p. 258].

TABLE 3—RATIOS OF SALES TO STOCKS FOR RETAIL, WHOLESALE, AND TOTAL TRADE IN THE UNITED STATES, USSR AND CSR, SELECTED YEARS

| Year | Retail | | | Wholesale[a] | | | Total | | |
|---|---|---|---|---|---|---|---|---|---|
| | USA | USSR | CSR | USA[b] | USSR | CSR | USA | USSR | CSR |
| 1939 | — | — | — | — | — | — | 3.2 | — | — |
| 1940 | — | — | — | — | — | — | — | 5.2 | — |
| 1948 | — | — | — | — | — | — | 3.1 | — | — |
| 1950 | — | — | — | — | — | — | — | 3.2 | — |
| 1951 | — | — | — | — | — | — | — | 3.0 | — |
| 1952 | — | 5.6 | — | — | 4.8 | — | — | 2.6 | 2.5 |
| 1953 | 4.0 | 5.5 | — | 16–14 | 6.6 | — | — | 3.0 | 2.1 |
| 1954 | 4.7 | 5.7 | 7.6 | 10–9 | 11.4 | 4.2 | — | 3.8 | 2.7 |
| 1955 | 4.5 | — | 8.2 | 11–10 | — | 4.5 | — | 3.6 | 2.9 |
| 1956 | — | — | 8.7 | — | — | 4.3 | — | — | 2.9 |

[a] Total and retail data are known. Wholesale ratios have been obtained simply by subtraction: (Dollar value of total inventories per dollar of sales) — (Dollar value of retail inventories per dollar of sales) = (Dollar value of wholesale inventories per dollar of sales).

[b] For the United States none of the total and retail ratios are available jointly for any one year; thus direct subtraction to calculate the wholesale ratio cannot be made. The total ratios for 1939 and 1948 were assumed to be limiting values to the (unknown) 1953–55 total ratios.

*Sources:* United States and USSR: [1, pp. 558, 561, and 563]. CSR: Table 1 and [7, p. 257].

TABLE 4—COMPARABLE RATIOS OF RETAIL SALES TO TOTAL STOCKS IN TRADE IN THE UNITED STATES, USSR, AND CSR, SELECTED YEARS

| Year | Food = 25 Per Cent[a] | | | Food = 59 Per Cent | | |
|---|---|---|---|---|---|---|
| | USA | USSR | CSR | USA | USSR | CSR |
| 1939 | 3.2 | — | — | 4.5[b] | — | — |
| 1948 | 3.1 | — | — | 4.4[b] | — | — |
| 1950 | — | 2.5 | — | — | 3.4 | — |
| 1952 | — | — | 1.4 | — | — | 2.5 |
| 1953 | — | — | 1.2 | — | — | 2.1 |
| 1954 | — | 2.9 | 1.6 | — | 4.1 | 2.7 |
| 1955 | — | 2.6 | 1.7 | — | 4.0 | 2.9 |
| 1956 | — | — | 1.7 | — | — | 2.9 |

[a] Adjustment is based on data shown in Table 3 and in footnote 2.

[b] Adjustment based on the assumption that the 1951 ratio of the turnover of nonfood to food products can be applied.

However, as Campbell points out, it is important to note that in the United States foodstuffs, with their notoriously high turnover, account for one-fourth of all retail sales, while in the USSR they account for approximately 50 per cent and in Czechoslovakia for 59 per cent of all retail sales.[3] Thus the data presented in Table 3 are really not comparable. To remove this difficulty I

[3] Food sales accounted for 58.6 per cent of all retail sales in 1953, 1954, and 1955, while in 1956 the percentage declined by one point [3, p. 188].

have recalculated the data using the assumption that in all three countries food sales account for one-fourth of all retail sales (roughly the U.S. share) and for 59 per cent of all retail sales (roughly the Czech share). The results are presented in Table 4.

Table 4 shows clearly that the performance of the two Soviet-type economies is substantially inferior to the performance of the American economy, regardless of whether one bases the comparison on the American or Czech trade patterns. Actually, the second Soviet-type economy considered here, in all relevant respects (degree of industrialization, transportation facilities, distances of the producers from markets, administrative personnel) superior to the economy of the USSR, is shown to be inferior in inventory performance even to the economy of the USSR. Thus, Campbell's conclusions are confirmed and, I believe, reinforced.

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## The Wage-Push Inflation Thesis, 1950-1957: Comment

In the December 1958 issue of this *Review*, Lowell E. Gallaway analyzes the following question [2, p. 968]: "What conditions would have had to prevail for the wage-push inflation thesis to be an adequate explanation of the inflation that has characterized the United States economy in the period 1950-1957?" In his answer he advances the following two propositions: First, a pure cost-induced (wage) inflation is compatible only with an aggregate demand for labor function that is perfectly inelastic; if I interpret him correctly he rejects this theory as an explanation of the inflation that occurred in the 1950-1957 period. Second, the "qualified" wage-push inflation thesis (this thesis, according to Gallaway, is that the full-employment goal enunciated in the Employment Act of 1946 gives assurance to organized labor that whatever unemployment results from excessive money-wage-rate increases will be eliminated by federal contracyclical policies) is only valid as an explanation of this inflationary period if it "... is presumed that the large increase in federal government expenditures was deliberately designed to support full employ-

ment in the face of excessive money-wage-rate increases" [2, p. 971]. Experience in the recessions of 1948-49, 1953-54, and 1957-58, indicates that unemployment, irrespective of its cause (cost-induced or otherwise), was not counteracted by governmental contracyclical policies. Thus the increased federal government expenditures ". . . resulted from factors exogeneous to the money-wage-rate employment-level relationship . . ." [2, p. 971]; this presumably means the economy did not have a cost-inflationary bias.

In this note I shall argue that Gallaway's interpretation of the qualified wage-push inflation thesis is too narrow for two reasons: first, he ignores the behavior of monetary policy; secondly, trade union leaders do not have to believe that ". . . any unemployment effects resulting from excessive money-wage-rate-increases will be compensated for by government contracyclical policies . . ."[2, p. 968] in order for a cost-inflationary bias to be present in an economy.

An economy can have a cost inflationary bias even though there is no presumption that federal spending will eliminate any unemployment resulting from excessive money-wage-rate increases. Given the government's commitment to the goals contained in the Employment Act of 1946, the Federal Reserve authorities may be expected to pursue a policy of monetary restraint cautiously. Otherwise the application of strong doses of monetary restraint will bring on the very decline of aggregate economic activity which it is the responsibility of the federal government to prevent.

In 1955 production expanded at a record rate. The index of industrial production, at 123 in August 1954, rose to 130 by December, and to 144 by December 1955. The gross national product rose in 1955, 1956, and the first 9 months in 1957. The consumer price index did not commence to rise until the beginning of 1956, and wholesale prices until mid-1955. Although the money supply (the total of paper currency plus demand deposits) expanded little from the first quarter in 1955 to the fourth quarter in 1957 (the expansion was $2.8 billion), a substantial rise in the velocity of money occurred. The rate of turnover in the money supply increased by approximately 15 per cent from the first quarter of 1955 through the fourth quarter of 1957. This is equivalent to a 15 per cent rise in the money supply with a constant velocity.

These velocity changes simply reflect an increase in the supply of close substitutes for money. The public was substituting savings deposits and short-term government securities for checking deposits; for this period, the level of time deposits expanded from $75.6 billion to $88 billion; balances in savings and loan associations increased from $27.9 billion to $41 billion; and the public's holdings in short-term government securities rose from $25.6 billion to $37 billion. The public found it advantageous to economize on its holdings of cash, and financial institutions which gained these funds loaned them out promptly to spenders. As a result, the turnover of the money supply was accelerated causing trouble for monetary policy [3, p. 180].

To be sure the government moved rather promptly to curb the inflationary forces generated during 1955. But if these anti-inflationary policies are viewed

in their entirety, it is clear that they were half-hearted, especially if viewed against the measures that had previously been adopted to resist recession. No increase in reserve requirements was undertaken; long-term bonds were not issued by the Treasury between the summer of 1955 and the fall of 1957; the discount rate was raised in a series of small steps; and the Federal Reserve system did not initiate a continuous deflationary open-market policy—the holdings of securities by the system were practically as large at the end of 1956 as at the end of 1954. In short the monetary authorities were reluctant to initiate a vigorous restrictive monetary policy to compensate for the rise in the velocity of money during this period [1, p. 38]. Under these conditions, a cost-inflationary bias was injected into the economy because the monetary authorities were unwilling to control the volume of bank credit.

In times of full employment, it is the action of monetary authorities which determines the stability of the price level. And a qualified wage-price inflation thesis that only emphasizes federal spending is inadequate. Excessive money-wage-rate changes can be compensated for by a rise in the quantity of money or its velocity. It is precisely the willingness of the monetary authorities in most countries to feed the stream of monetary demand sufficiently to accommodate excessive money-wage-rate increases that enables the wage-push process to operate; or alternatively, the unwillingness of the authorities to counteract the more active use of the existing stock of money induced by cost increases or mild credit restraints.

Whether trade union leaders believe that the unemployment resulting from excessive wage rate increases will be compensated for by federal contracyclical policies is irrelevant. (It is hard to believe that any American trade union leader believes this, especially in light of both Democratic and Republican administration reluctance to use such policies since the passage of the Employment Act of 1946.) Union leaders only have to be aware of the fact that wage-rate increases do not cause continuous unemployment; given this empirical observation, they will reintensify their upward pressure on wage rates. What is needed is to include in the Employment Act of 1946 the goal of reasonable stability of prices. If the control authorities were to feel as keenly about this as they do in regard to production and employment levels, the wage-push-inflationary thesis might become only of interest to economic historians.

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## The Wage-Push Inflation Thesis, 1950-1957: Reply

I will confine my remarks to what I consider to be Dudley Johnson's primary argument, viz., actions of the monetary authorities compensated for any potential unemployment effect implicit in money-wage-rate increases in the period 1955-1957.[1] If this position is valid, my interpretation of the recent price inflation would have to be revised.

Johnson's argument seems to rest primarily on his contention that, "In times of full employment, it is the action of the monetary authorities which determines the stability of the price level" (p. 1035). He argues that during the period 1955-1957 the cost-inflation process could operate without any unemployment effect because of a substantial increase in the velocity of money. This increase, he contends, was the result of (1) a permissive attitude on the part of the monetary authorities, and (2) changes in the price level which were induced by cost increases. I draw these inferences from his statements that (1) "a cost-inflationary bias was injected into the economy because the monetary authorities were unwilling to control the volume of bank credit" (p. 1035), and (2) the wage-push process may be allowed to operate because of the "unwillingess of the authorities to counteract the more active use of the existing stock of money *induced by cost increases* or mild credit restraints" (p. 1035; my italics). I must disagree with both counts of Johnson's interpretation of the increase in the velocity of money in this period.

With respect to his argument that the monetary authorities were "unwilling" to control the expansion of the effective money supply that resulted from the increase in velocity, I would suggest that rather than being "unwilling" to control this phenomenon, the monetary authorities found themselves "unable" to control it within the existing framework of controls. In the words of A. G. Hart, "if the public chooses to shift the form in which it holds its cash in time of credit stringency, the limits set by reserve requirements are by-passed. Since the rise in interest rates induced by such a stringency increases the relative attractiveness of time and government securities, and thus sets up incentives for such a shift, this weakness in the control system is very real, as we experienced in 1955-1957" [1, p. 182].

The argument that the pressures which produced the change in velocity originated in increased costs is, unfortunately, not buttressed by any empirical evidence. I suggest that a more accurate explanation of the forces which produced the change in velocity may be had by considering equation (1):

$$C + I + G = PO \tag{1}$$

where consumption expenditures ($C$) are some function of disposable income, $P$ equals the price level, and $O$ equals the level of output. During the period in question,[2] government purchases of goods and services ($G$) increased about

[1] Johnson's comments with respect to trade union leader attitudes merely broaden the scope of the qualified wage-push inflation thesis sufficiently to enable him to consider the role of monetary policy in the inflation process.

[2] Comparisons are between annual rates for first quarter 1955 and annual totals for year 1957.

$10 billion and gross investment expenditures ($I$) increased about $18 billion. In the meantime, net government receipts increased by about $16 billion. The net effect of these changes was a substantial increase in aggregate effective demand. This, while the economy is operating at full employment levels, will obviously increase the price level. According to the equation of exchange, an increase in $P$ with little change in $O$ will produce an adjustment in either the quantity of money ($M$) or its velocity ($V$). As Johnson points out, in the period 1955-57 the adjustment took the form of an increase in the velocity of money. While a possible case can be made for the increased expenditures resulting from cost increases, it seems highly unlikely that a rise in costs resulting from excessive money-wage-rate increases would stimulate an $18 billion rise in gross investment expenditures and a $10 billion increase in government purchases of goods and services.[3]

In summary, the period 1955-1957 was characterized by excessive aggregate demand which put pressure on the money supply ($MV$) through an increase in the velocity of money. This increase in velocity then permitted the pressures from the aggregate demand side to exert their impact on the price level. This pattern of events illustrates rather aptly the weaknesses of monetary policy (even at levels of full employment). Consequently, I contend that a qualified wage-push inflation thesis that ignores monetary policy is not too narrow.

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[3] Government purchases of goods and services ran at an annual rate of $75.8 billion in the first quarter of 1955 and $85.7 billion for the year 1957. If it is assumed that the cost to government of these goods and services moved in line with the wholesale price index (110.2 in first quarter 1955 and 117.6 in 1957), about half of the increased expenditures can be explained. However, this does not answer the basic question of whether these price increases were cost-induced or demand-induced.

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## Time-Preference and Economic Growth: Comment

In a recent note [4] appearing in this *Review*, Wassily Leontief provided "a relatively simple method of graphic presentation and analysis . . ." of time paths (involving many time intervals) of investment, capital, income and consumption that are said to be the consequences of maximizing behavior. Many years ago Irving Fisher, on whose pioneering analysis Leontief's paper is based, ended his brilliant graphical exposition of the two-period paths on a pessimistic note for the literary economist. Regarding the possibility of an extension of the diagrams to plans involving many periods, he concluded: "The truth seems to be that no complete visualization of this difficult problem is possible. The only complete symbolization which seems to be possible is in terms of mathematical formulas. . . ." [1, p. 287].

It is the purpose of this brief note to demonstrate that, much like Hayek [3] [2, Ch. 17-18] before him, Leontief has in fact not succeeded in solving the problem. Because he overlooks some crucial intertemporal relationships, his time paths of economic development do not have the maximizing properties claimed for them.

Leontief's graphical solution of how much to consume and how much to add to the capital stock at each point in time can be represented by what I will call Leontief's fundamental equations:

$$MU_C{}^t = MPP^t MU_Y{}^t \qquad t = 1, 2, \ldots \tag{1}$$

The term on the left is the marginal utility from consumption at time $t$. The first factor on the right is the marginal physical product from investment at $t$ in the production of perpetuities of income commencing at $t + 1$. This is postulated to diminish with increases in the capital stock. The second factor on the right is the marginal utility derived from the claim to such a perpetuity commencing at $t + 1$. Given an initial capital stock at $t = 1$, an income for this point in time is determined by the production function. As long as all the income is consumed—no more, no less—the capital stock remains unchanged and the same constant income will be produced. But how much should be consumed and how much added to the capital stock in order to maximize the satisfaction of the decision-maker? The first equation in the set (1) says that the income at $t = 1$ should be divided between current consumption and additions to the capital stock so that an extra unit of income will give the same extra utility if consumed at $t = 1$ as will the claim to the $MPP^1$ units of perpetuity that can alternatively be produced with such an extra unit of income. This determines consumption and capital formation at $t = 1$. It also specifies the capital stock at $t = 2$ and hence income for $t = 2$. Similarly the solution of the second of Leontief's fundamental equations determines the allocation of income for $t = 2$ between consumption and investment; and so on for succeeding time periods. Of course, we need not write any of the equations once the stationary state in which all of the income is consumed is reached, since the solution for every one of these is identical.

The clue suggesting that the plan sketched out by Leontief's process fails to maximize the utility function of the decision-maker is provided by the characteristic that decisions are made seriatim. The consumption-investment choice for $t = 1$ is made without regard to and quite independently of the allocation of the output at subsequent points in time. Similarly for the allocations at $t = 2, 3. \ldots$ Thus, the amount to be added to the capital stock at $t = 2$ is the same whether beginning with $t = 3$ *all* income each year is to be consumed or only 20 per cent each year is to be consumed. Surely it is impossible to decide how much output at each point is to be allocated to consumption and capital formation without taking explicit account of how high the marginal utility of consumption and the marginal productivity of capital will be at *each* later date. After all, each one of these decisions determines how large the capital stock and consumption at each of these time points will be. It almost seems as if Leontief's solution is a plan which determines consumption for each day under the assumption that on the following day *all* income

is to be consumed, when in fact the analysis involves the determination of how much is to be consumed each day.

An optimal plan through time must meet a crucial test. Regardless of what the total quantity of capital is to be at some particular future point in time, say at $t = T$ (and what this amount is to be is, of course, itself to be determined), so that the sum of the capital additions for all periods from say $t = 1$ to $t = T$ is specified, it must be impossible to raise what Leontief refers to as the "level of welfare," i.e., ordinal utility, by increasing capital formation at any one point in time and reducing it by the same amount at *any* other point in time. For instance, in the plan illustrated by Leontief's Figure 1, consider the capital stock at $t = 3$ specified at the stationary level determined by him. Since the capital stock at $t = 1$ is given, the total capital formation over the interval from 1 to 3 is determined. The crucial test is: Reshuffle (arbitrage) the total investment between the first two time periods; increase capital formation by a unit at $t = 1$ and decrease it by a unit at $t = 2$, or vice versa; this must *not* increase total utility. Leontief's solution fails this test.

The issue is clarified and dramatized if we associate some numbers with Leontief's example given in his Figures 1 and 2. We select a utility index so that each indifference curve has associated with it the utility:

$$U = .2 \ln C_i + \ln Y_{i+1}$$

where $C_i$ stands for the consumption scheduled for $t = i$ (measured on the horizontal axis in his Figure 1) and $Y_{i+1}$, for the perpetuity of income commencing at $t = i + 1$ (measured on the vertical axis). These are seen to be indifference curves with the conventional diminishing marginal rates of substitution ($MU_C/MU_Y = .2Y_{i+1}/C_i$), and the marginal utilities are diminishing. As production function (Leontief's Figure 2) we choose for simplicity the segmented linear function:[1]

$$\begin{aligned} Y &= .8K & 0 &< K < 8 \\ Y &= 6.4 + .6\ (K - 8) & 8 &< K < 14.755 \\ Y &= 10.454 + .35\ (K - 14.755) & 14.755 &< K < 16 \\ Y &= 10.889 + .2\ (K - 16) & 16 &< K \end{aligned}$$

where $K$ is the capital stock (measured on the horizontal axis of Figure 2). The coefficients of $K$ are, of course, the marginal physical products of capital ($MPP$), and these are seen to diminish by steps as the capital stock grows. We suppose that the given initial capital stock, $K_1 = 9$. This is seen to imply an initial income, $Y_1 = 7$.

By careful plotting or by applying Leontief's fundamental equations the capital-investment-consumption-income allocation through time can be obtained (see part A of Table 1). This is Leontief's solution. As in his own

[1] For the smooth production function illustrated by Leontief in Figure 2 (but not in Figure 1) it is logically impossible for his stationary state to be reached in three time periods. This state can be approached only in the limit as the number of time periods becomes infinite. Readers familiar with the theory of the cobweb cycle should have no difficulty in proving this.

graphical example, a stationary state is reached at $t = 3$. Part B of Table 1 gives an alternative solution which is also permitted by the technology. Part C illuminates the differences between the two paths of economic development. The two plans are identical beginning with $t = 3$. They differ in that in the alternative plan more of the total capital formation occurs at $t = 1$, requiring a corresponding contraction of consumption at $t = 1$. This leads to a higher level of income at $t = 2$, and consumption for this date is increased not only by the amount of investment released by added investment of the previous period, but also by the fruits (interest) borne by this additional investment. Of course, where the two plans differ, Leontief's fundamental equations are not satisfied by the alternative plan. Its consumption-perpetuity points are not given by tangencies in Leontief's Figure 1.

To show that the alternative plan raises "the level of welfare" above that

TABLE 1—CAPITAL, INCOME, INVESTMENT AND CONSUMPTION THROUGH TIME

| | A. The Leontief Plan | | | | B. An Alternative Plan | | | | C. Difference Between Alternative and Leontief Plans | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| t | K | Y | ΔK | C | K | Y | ΔK | C | K | Y | ΔK | C |
| 1 | 9.000 | 7.000 | 3.889 | 3.111 | 9.000 | 7.000 | 5.000 | 2.000 | — | — | +1.111 | −1.111 |
| 2 | 12.889 | 9.333 | 3.111 | 6.222 | 14.000 | 10.000 | 2.000 | 8.000 | +1.111 | +.667 | −1.111 | +1.778 |
| 3 | 16.000 | 10.889 | 0 | 10.889 | 16.000 | 10.889 | 0 | 10.889 | — | — | — | — |
| 4 | 16.000 | 10.889 | 0 | 10.889 | 16.000 | 10.889 | 0 | 10.889 | — | — | — | — |
| ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ | ⋮ |

reached by the Leontief plan it is only necessary to show that the total utility associated with the alternative plan is greater than that associated with the Leontief plan. This is a simple computation since the two plans yield identical utility except for that derived from $C_1$, $C_2$, and $Y_2$. Entering the magnitudes for these quantities for the alternative plan in the utility function and subtracting from this total the utilities derived from the magnitudes for these quantities in the Leontief plan, we get a positive number, namely:

$$.2(\ln 2.0 + \ln 8.0) + \ln 10 - [.2(\ln 3.11 + \ln 6.22) + \ln 9.33] = .04$$

Of course, the magnitude of this difference is of no significance; only the sign counts. Thus, we show that if we rearrange total investment in the Leontief plan among only the first two periods, leaving the plan unchanged in all other respects, the level of total welfare can be increased. In geometric terms this means that regardless of what particular ordinal utility index is assigned to the indifference curves of Leontief's Figure 1, the decision-maker can profitably go to a lower indifference curve for $t = 1$ by moving the point $P'_1$ in a northwest direction along the "exchange line" and thereby be on a higher indifference curve for $t = 2$ than is the point $P'_2$, and one which is sufficiently higher to make this worth while.

The alternative plan is itself not the one that maximizes the level of welfare —it is only *better* than Leontief's. Indeed for the same reason that total wel-

fare is increased by switching investment in the Leontief plan between the first two periods, so can it be improved even more by further arbitraging also between the other time periods. Thus it can be demonstrated that the stationary state of Leontief's solution slated to begin at $t = 3$, must in an optimal solution, in fact, not begin at $t = 3$. It can also be shown that the capital stock-income-consumption quantities of the Leontief stationary state are, in general, *not* those associated with the plan that maximizes total welfare.

Both Hayek and Leontief attempted to use the income perpetuity concept to solve the problem which Fisher could not handle without it. They both thought that this would turn the trick. Since it fails to accomplish its purpose and since, in the first place, it is not easy to comprehend why such perpetuities should enter the utility function in addition to the stream of consumption, it might be more fruitful for those interested in the considerations dealt with by Leontief's note to return to the study of the Fisher-Ramsey versions of the model in which welfare is derived from the stream of consumption alone.

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## Time-Preference and Economic Growth: Reply

In my note, economic growth is described as a continuing, unending process the path of which is determined by a never-ending sequence of choices. Particularly important, from this point of view, is the fact that the explicit time-horizon of each one of these successive choices is much shorter, in principle infinitely shorter, than is the span of time covered by the dynamic process as a whole. Thus while each step, being determined by a conscious act of choice, satisfies certain maximizing conditions, this sequence as a whole does not. Its path can be compared to the course of a dog running across a field toward his master, while the master walks along a road. The dog's path will usually describe a gentle arc, while the fastest way of joining his master would be to run along a straight, properly aimed intercepting line.

To demonstrate the general properties of my dynamic system on two-dimensional graphs, I had to base my simple illustration on the simplest possible kind of intertemporal choice with a horizon extended over only two distinct successive positions—"this year" and the "next year and all years thereafter." The second position also summarizes expectations concerning the consumption possibilities in all the subsequent years.

Westfield, on the other hand, considers the process of economic growth as

being determined over its entire length in one piece, by a grand act of one single choice. To be consistent, he must also assume that his choice is based on a full and a correct knowledge of all relevant conditions from now to the end of time; or, perhaps one should say, to the very end of the growth process. If my approach is right, a country could most likely profit, as Westfield's numerical example shows, by revising some of the choices made in the remote past; if his view is correct, it would not want to change, even in the light of later knowledge, any of the decisions made before. His is a sophisticated dog who follows the shortest, straight intercepting course.

Westfield's problem of maximizing utility—by planning the allocation of income between consumption and investment—over long intervals of time is certainly of considerable interest in itself. If it were not, why should Frank Ramsey have brought it up thirty years ago, and why should the operations researchers and dynamic programmers of today be solving it in so many different forms? I would also not deny that a model of economic growth more elaborate than that presented in my note should and could easily be based on a choice-horizon longer than a two-periods interval in time. Let me repeat, however, that the crucial difference between Westfield's assumptions and those which I preferred to make lies not in the absolute extension of that horizon, but in its length relative to the duration of the entire dynamic sequence that we analyse. If each individual choice is based on a forward view in time which is at least somewhat shorter than the process as a whole, the general properties of economic growth are correctly displayed not by Westfield's model but by mine. The one-shot, pure decision processes which he sets up—if one could visualize it in the context of economic growth—cannot possibly have stable or unstable equilibrium solutions as described in my note; there would be one, the best of all possible solutions, and that is all. As soon however as one choice is assumed to be followed by a second, a third, and so on, the problem of stability of any possible repetitive stationary equilibrium position must arise.

Westfield's treatment of his own problem of maximizing utility over time seems to contain a logical flaw. In constructing the numerical example shown on his Table 1, he uses a utility index of the limited-horizon type, as I do. Among the factors on which the level of utility is supposed to depend, this type of function usually contains a variable which summarizes the possible alternative effects of action taken within the fixed time-horizon on the events which will occur later on, and which the decision-maker cannot anticipate in full detail. In the utility function used in my note, the role of such a terminal variable is played by the magnitude of the (possible) perpetual annual income which could be earned beginning with the "next" year. (Considering the simplified production function which I assume, I could have used instead the stock of capital accumulated by the beginning of the "next" year.) In Westfield's formula,

$$U = 0.2 \ln C_t + \ln Y_{t+1},$$

the annual level of the perpetual future income stream is represented by $Y_{t+1}$, while $C_t$ stands for the amount consumed "this" year. But his decision-

maker—in contrast to mine—is supposed to base his choice on a full and accurate prediction of all possible courses of events not only in "this" and in the "next" year, but separately for each one of the future years beyond that and up to the very end of the process of economic growth. Yet Westfield computes his own Alternative Plan on the basis of the limited-horizon function shown above. The internal inconsistency of this procedure shows up when he goes on to demonstrate the superiority—in total utility terms—of the Alternative Plan over what he labels in the table The Leontief Plan. By substituting, for the numbers which he uses, the corresponding letter symbols, we find that the utility function on which Westfield bases this computation has the following general form:

$$U = 0.2\ (\ln C_t + \ln C_{t+1}) + \ln Y_{t+1}.$$

It is not only different from and incompatible with the utility function actually used by him to construct the numerical examples in the table, but it also makes little sense when considered by itself. This shows that a limited-horizon function with a terminal term cannot be used to formulate the problem of maximizing utility over time if the total length of the period over which utility is supposed to be maximized exceeds the length of the time-horizon covered by the utility function which one intends to maximize.

To interpret the process of economic growth as if its path were determined by one single and infallible rational choice, it is necessary to assume the existence of a utility function which shows the level of utility depending simultaneously on the levels of consumption not in two, three or even twenty years, but in all the years over which the process of economic growth extends. Such a function could, for example, be of the following form:

$$U = 0.2(\ln C_1 + \ln C_2 + \ln C_3 + \dots + \ln C_n).$$

For purposes of the actual application of his own theory, Westfield should be able to identify in an empirically meaningful way the "first year" in which the grand choice supposedly is made, as well as the "*n*th year" in which the process of economic growth is assumed to end. I can not see how this can be done.

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# BOOK REVIEWS

## General Economics; Methodology

*Economic Synthesis.* By BORIS ISCHBOLDIN. New Delhi: New Book Society of India, 1958. Pp. 543. $6.00.

This book covers more than one phase of economics. Occasionally it is a history of both familiar and little-known economic thought, sometimes a critical analysis of economic ideas both old and current, then often a summary and description of economic theory. All of these facets are treated from time to time on an elementary, intermediate, or advanced level. The author's stated objective is to contribute a genuine synthesis of socio-economic problems which will reconcile the "sound hard core of neoclassicism . . . and the modern popular and somewhat arrogant Keynesian train of thought." The synthesis should in the author's view combine "the natural-scientific method with the social scientific method," using statistics "as a supplementary tool for verification and illustration of non-universal generalizations." However, statistics are little in evidence and the author pointedly eschews a mathematical approach. An attempt is made discursively to integrate religion, natural law, genetics, and other immaterial factors into economic science.

The Ischboldin synthesis reaches its climax in the last three chapters, where he presents a socio-economic balance sheet (inspired by Manfred Berliner and Othmar Spann), his ideas on social rent, and arguments for an integrated rather than a narrow economic methodology (i.e, one which confines itself to explaining the subject exclusively from either an empirical, deductive, mathematical, or historical approach). The synthesis often fades from view, though there are intermittent references to the ethical or naturalistic aspects of economics.

The difficulty with the author's synthesis is that the real problems remain unsolved. Economists recognize that many metaphysical elements bear on the economic problem directly or indirectly. The difficulty has always been in quantifying and containing their influences within a meaningful framework. While Ischboldin believes this can be done—many economists would like to see it done—the author, perhaps inevitably, did not demonstrate in concrete terms how it might be done. It is the dependable measuring rod that will aptly fit these factors into the macroeconomic model for which many social scientists have vainly sought.

The book also has difficulties in style, format and organization. The writing is often awkward, pedantic, and frustrating. The ideas are listed by number throughout, which has a deadening effect. There is frequent reference to foreign economists without establishing the context of their ideas. Despite its impressive inclusiveness of names and ideas, the book has no index. More important than these, however, the author failed to conceive a unity of

purpose and design, either in subject or in the audience for whom the study was intended. History, analysis, criticism, original ideas, review, questions elementary and esoteric—all these are intermingled.

Notwithstanding these basic shortcomings— some the publisher's fault— this book has value. The chapters on Utility and Wants, Value and Valuation, Labor and Wages, Rent and Interest offer unusually exhaustive examinations of these concepts and some new alternately illuminating and controversal insights for student or specialist reading. The author contributes, though all too briefly, ideas of dozens of economists seldom if ever dealt with elsewhere. Moreover, Ischboldin's "Social Limits of Profitability," his "Socioeconomic Balance Sheet," "Social Rent," and his attempt at a synthesis give the book an unconventional and interesting flavor.

This work is also notable for stimulating thought about studies the author could fruitfully develop further. Russian economic ideas and practice, obviously alive and familiar to Ischboldin, and the Soviet social accounting system briefly described in Chapter 21 deserve full coverage. The many little known economists, fleetingly met here, warrant full elaboration. Theoretical differences between the author and Keynesian and Hicksian economics plead for resolution. The "Social Balance Sheet" lacks rigorous examination, particularly as to quantification problems. All of these are intriguing glimpses one gets from this work.

Thus the reader is frustrated between a desire to know more of these topics and the impulse to skip so much that is familiar in between. A more limited objective may have given us more efficient and penetrating use of the author's original and learned mind. Nevertheless, in its panoramic sweep of old and new terrain, its flashes of originality, and its fearless grappling with metaphysical phases of socio-economics, the book is impressionable reading.

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*Economics in Action*. By HARRY G. BRAINARD. New York: Oxford University Press, 1959. Pp. xvi, 441. $5.50.

Brainard has written a solid elementary text designed primarily for a one-semester terminal course for nonmajors. The book is of the nonencyclopedic variety and is therefore necessarily selective in its range of materials and in the depth of coverage accorded to the topics chosen for discussion.

Part I devotes chapters to scope and method, the economics of production, the conditions surrounding the factors of production in the United States and money and banking. The chapter on scope and method gives a conventional account of the circular flow and "the four basic problems which any economy must solve." The chapter on production, a refreshing reversion to the old-fashioned texts, is effectively developed through contrasting the U. S. economy with those of the underdeveloped countries. The chapters on the factors of production are a model of compact elementary presentation. The section devoted to U. S. agriculture is the best textbook discussion which the reviewer

has seen. Why the author chose to discuss money and banking in a part devoted to the setting of the economy rather than in conjunction with stabilization policy is unclear. The treatment, though competent, is thin, and a reasonably thorough discussion of monetary policy is not presented anywhere in the book.

Part II is devoted to the pricing of goods. Partial-equilibrium analysis is used, and the bulk of the discussion is of the competitive case. Supply and demand curves are developed and applied to the usual problems in the usual way. More attention is devoted to the cost curves of the individual firm and their influence on supply than has become customary in elementary texts. An instructive chapter is devoted to government interferences with the price system. There is no systematic discussion of the theory of income distribution. This part serves the purpose of the elementary course well, since the student should come away from it with a clear notion of the opportunity cost concept, and an adequate grasp of the principles of resource allocation.

Part III discusses, in turn, national income accounting, the theory of income determination, the business cycle and stabilization policy. The chapter on national income accounting is well done. In his treatment of the remainder of these topics, the author has chosen to economize on space. The wisdom of this choice is a matter of taste, and it may well be that the typical elementary course devotes too much time to these topics, but the potential user of this book must realize that the discussion of the pressing domestic problem of the joint pursuit of full employment and price stability is sketchy. The final chapter of Part III treats economic growth. In the experience of the reviewer, an appreciation of the problems of economic growth is extraordinarily difficult to convey to sophomores. Brainard seems to have solved this problem, and this outstanding feature of his book represents a real contribution to the teaching of elementary economics.

Part IV deals with the public economy, and Part V with the international economy. The choice of material is felicitous in both cases and the international section compares most favorably with other text treatments.

The reviewer's chief quarrel with this book, and with most of its competitors for the elementary course, is its lack of an explicit discussion of the issues involved in declaring one constellation of economic events superior to another. After all, a good deal of the elementary course is devoted to improving the student's basis for policy evaluation. It may be that the recent logical critiques of welfare economics have made textbook authors gun-shy in this area. Yet they should remember that economists are better fitted to discuss these problems than editorial writers, that economics is unique among the social sciences in having an extensive normative literature, and that it may well be that among the chief contributions that the elementary course can make is a clarification of the value judgments inherent in alternative policy courses.

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*Economics: Principles and Policy.* By ROYALL BRANDIS. Homewood, Ill.: Richard D. Irwin, 1959. Pp. xvii, 340. $6.00.

In style, this text is almost uniformly easy and direct. At his best, the author has a knack of presenting economic ideas in a clear, succinct manner. In organization, microeconomics precedes macroeconomics. The topical coverage follows, in the main, traditional lines; but the characteristic which makes the book unique is its brevity. The text's 340 pages are very short pages.

One reason for the above-mentioned brevity it the author's terse, concise style. A good example is the following, taken from the discussion of international economics: "Comparative advantage is a *relative* matter. If labor is 'expensive' in the United States, it is only because capital and land are both 'cheap.' If a foreign nation has 'cheap' labor it is only because capital or land or both are 'expensive' there. . . . If the Swiss have a comparative advantage in the production of watches relative to the United States, then the United States *must* have a comparative advantage in the production of some other product relative to Switzerland" (pp. 270-71).

Brevity is also achieved by the elimination of much descriptive material. The author has omitted topics which he feels do "not contribute significantly to the student's understanding of the major policy problems of our economy" (p. ix). Examples are: methods used in the selection of government committees, statistical methodology of government agencies, and statistics of certain kinds. While much descriptive material can with benefit be left out, there are definite limits to this practice. A case in point is the author's treatment of the general field of government and business (pp. 103-7). No attempt is made to use such apt material as basing-point practices, fair trade, and "fair return on the fair value" to illustrate economic principles and policies. These phenomena are of sufficiently widespread occurrence in our economic system to call for their presentation in an introductory text.

A third reason for the shortness of this book is the author's tendency to use brief, broad statements to cover topics which seem to require more extended discussion. Consider, for example, the following: "All the economists mentioned in the last paragraph were either English or German. This is not to say that a number of American economists have not made significant contributions to the subject. Yet in economics, as in the natural sciences and mathematics, the most original work, the most sweeping ideas have not come from Americans, but rather from Europeans. For any American reader who may go on to become an economist I leave this challenge." This statement in itself has a majestic sweep which should put it in the running.

In his prefatory remarks the author mentions some "fine points of technical analysis" which he has omitted "because they are not likely to be grasped by the average college sophomore." Among these are "numerical coefficients of elasticity, the Samuelson-Stolper effect, the interaction of the accelerator and the multiplier, and the geometry of indifference curves" (p. ix). Elimination of these subjects is not a particularly important factor in the comparative brevity of the text, since the more difficult theoretical

concepts are usually omitted by other introductory texts also. For example, the Samuelson-Stolper effect is not customarily discussed in such texts (although it is true that Samuelson mentions it in his); and indifference curves are (following Raymond Bye's example) usually relegated to an appendix. However, shortening the book by omitting the interaction of the accelerator and the multiplier and by the failure to discuss numerical coefficients of elasticity does not seem desirable. Both concepts lend greater precision to analysis, are general in their occurrence, and seem to be well within the "average" college sophomore's powers of comprehension.

Much the same can be said of the author's decision not to include a discussion of profit-and-loss statements as an addendum to the explanation of the balance sheet (pp. 182-83). The reason given for this omission is that this type of statement "contributes little or nothing to the analysis of the firm and, indeed, is likely to confuse the student because the accounting meaning of terms like 'cost' and 'profit' is not identical with the meaning of these terms in economics" (p. x). But "cost" and "profit" are such basic concepts that the difference in usage which exists with respect to them would seem to be an excellent reason for discussing them *via* profit-and-loss analysis.

In summary: The author has written an exceptionally brief text, presumably with the object of making it more manageable for purposes of instruction. The result has been good to the extent that this objective has been achieved by the use of a clear, direct style; by elimination of prolix excursions into description; and by the exclusion of more difficult analysis. But the clear, direct style has often become unduly broad and impressionistic; some exceptionally useful descriptive material has been left out; and there is some question whether all the omitted analysis was really so difficult as to warrant its exclusion.

PETER PALMER

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*Economic Principles and Public Issues.* By ALFRED R. OXENFELDT. New York: Rinehart & Co., 1959. Pp. xx, 618. $3.95, paper.

This volume is a "lineal descendant" of Oxenfeldt's earlier text, *Economics for the Citizen.* As in his earlier work, he has proceeded from the premises: (1) that a useful and effective introduction to economics must build upon students' existing interest in public issues, (2) that extensive abstraction, elaborate analytical apparatus, and high-level generalization are unnecessary for illumination of these issues, (3) that intelligent and useful consideration of economic issues must include related political, social, and ethical problems, and (4) that the author should state, explicitly, his own policy conclusions.

This text will be passed over by colleagues who, in an introductory course, emphasize theoretical structure and analytical rigor. Purists in the profession will object that some of the material in the book isn't even economics: for example, the treatment of religious, social, psychological, and political problems associated with the topics of economic growth, large-scale enterprise, and the distribution of income. Moreover, the author does not pretend to

scientific detachment from his work. He takes specific positions on controversial policy issues and even—sin of sins!—makes numerous value judgments. Oxenfeldt does these things deliberately. He writes, effectively, for those teachers of elementary economics who share his philosophical preconceptions about pedagogical priorities and about the means of making economics "come alive" to the student.

Although the author uses but six "public issues" as hooks upon which to hang his exposition, he is able to cover the topics generally included in standard introductory courses. As one might expect, the section on Big Business includes descriptive material on the legal forms of enterprise, the antitrust laws, and other legislation with respect to enterprise; included in the section on Economic Instability we find the topics of deposit creation, monetary control, and fiscal policy. The author demonstrates his ingenuity by his successful incorporation into the section on Elimination of Poverty such topics as the theory of consumer demand, the meaning and measurement of income, and the prerequisites for economic growth. In the section on Income Distribution we find the law of diminishing returns, the concept of marginal productivity, descriptive materials on taxation, some tax-incidence theory, and a discussion of union activity. For some reason, Oxenfeldt has chosen to eliminate from this revision of his work the section on comparative systems, a topic which surely recommends itself on an "interest" criterion. Nevertheless, it is organization, not coverage, which makes this text unique.

The section on Economic Instability contains a somewhat more extensive statement of employment theory than in the earlier effort. The section on Economic Efficiency includes a modest dose of price theory. In the context of such abstract models as pure competition and simple monopoly, the reader is exposed to the marginal calculus and to some simple diagrammatics. This reviewer had the feeling that the treatment of these topics represented a concession to the market-place rather than the author's conviction as to their importance. In fact the presentation seems somewhat forced. It somehow doesn't ring true to include in a list of important and controversial issues, the "hot" issue of "economic efficiency." Moreover, in the 50-odd pages of Chapters 14-16, the reader is reminded no less than ten times that all this abstract theory should not be taken too seriously. Such an attitude toward construction of unrealistic models is curious indeed, coming from an author who makes the assertion that purely competitive markets are desirable, ideal, and virtuous whereas monopolistic markets are objectionable, nonoptimal, and sinful (see pp. 291, 300, 302, 320). These conclusions are clearly derived, not from any empirical observation or application of common sense, but from theoretical, abstract, and unrealistic models—a practice which the author finds most hazardous (see p. 595). And unfortunately, they are not necessarily valid conclusions. The author, himself, in another place in another context cites a situation in which independent, competitive action is less (socially) desirable than cooperative, group behavior (p. 71).

More extensive analysis of realistic market structures—in Oxenfeldt's terms, "conventional" markets—would have helped to avoid a number of question-

able generalizations. For example, it is not necessarily true that monopolistically competitive markets are essentially compromises between the good of pure competition and the evil of simple monopoly (p. 320). One can argue, with equal logic and some empirical verification, that imperfectly competitive markets combine the best (or the worst?) characteristics of the extreme market types. Those economists familiar with Chamberlin's recent writings (*Towards a More General Theory of Value*) will be skeptical about the author's assertion that monopolistically competitive markets are characterized by excess capacity and higher than necessary average cost of production (pp. 300, 305).

In the remainder of the book there are other propositions of dubious validity and for which some rigorous analysis would be illuminating: Only by acceptance of Say's law (which the author rejects, p. 482) or by an implicit shift in assumptions of the problem, can one accept the author's conclusion that *both* capital export and repatriation will raise the level of domestic aggregate demand, income, and employment (cf. pp. 561-62). The author's conclusion that free trade is to the advantage of every nation (p. 556) or factor (p. 558) is patently false. It is not necessarily true, in fact it is unlikely, that the present wealthy nations of the world will realize economic gain from the economic development of the poor nations (cf. p. 18). There are a number of exceptions to the proposition that wage increases as a result of union activity or of legislation, will reduce employment (cf. p. 136). It is certainly hazardous, if not fatuous, to generalize from partial equilibrium analysis and conclude that general wage increases will reduce the level of total employment in all markets (p. 165).

This book is a highly differentiated product for which *Public Issues*, rather than *Economic Principles* is the more appropriate portion of the title. For instructors who find students frightened by economic theory but intrigued by practical public affairs, this will be an attractive and useful book. The subject matter of economics is covered tolerably well; the material is presented in a style which is occasionally pedantic, usually lucid, always enthusiastic, and frequently provocative.

FRANK C. CHILD

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*Economics, Theory and Practice.* By MELVILLE J. ULMER. Boston: Houghton Mifflin Company, 1959. Pp. xv, 639. $6.95.

In his preface Professor Ulmer advises that he has attempted to provide a beginning text which is "both complete and compact" and "on a level . . . suitable for beginners." To a considerable degree he has succeeded. His book is well written; and descriptive and statistical materials are intelligently chosen, up-to-date and interesting. Moreover, there is a significant, and I believe desirable, shift of emphasis toward the analysis of growth and the economic problems of the world community.

But there is less emphasis given to theory and to the application of theory than is implied by the title. This is in part the result of the author's attempt

to write a "compact" text wihout sacrificing coverage. It is also the result of an organization of topics which delays the formal presentation of theory for approximately one third of the text.

The book is divided into nine parts, each containing from two to six chapters. In part I, "Foundations," the concepts of scarcity, factors of production, functions of an economic system and the circular flow of income are introduced briefly along with other preliminary material, but little effort is made to establish these basic concepts as central themes to be treated in the chapters which are to follow. Part II, "Income and Growth," consists of a chapter on national income accounting followed by a discussion of some of the dimensions of economic growth and welfare. Two additional chapters treat the "roots of progress," population and the labor force, natural resources, capital and technology. The principle of diminishing returns is presented effectively in dealing with the effects of population growth, but the nature and difficulties of capital formation receive scanty treatment. In the light of the concern with which the problem of capital formation is viewed throughout the world today, this lack of emphasis must be regarded as a serious weakness.

Part III, "The Financial Organization of Society," is comprised of chapters on the organization and financing of business, consumer finance, and, quite surprisingly, public finance. Since the latter chapter appears prior to the discussion of income, value and distribution theory, there is little opportunity to demonstrate the contributions which economic analysis can make to the solution of problems in this area. It is true, of course, that a subsequent chapter deals with fiscal policy, but this provides only a partial correction and in no way renders the chapter itself more effective. Part IV, "Money and Banking," suffers from the same general defect. The institutional material is well treated and the explanation of multiple expansion of bank credit lucidly made. But preceding as it does the discussion of monetary theory and the business cycle, neither the strategy nor the significance of Federal Reserve measures can be brought home to the student.

It is only with Part V that a formal attack upon theory is begun. Parts V to VII treat income theory and the business cycle, price and distribution. The presentation of income theory is thorough and well supported by graphical analysis. It is by far the longest and the most effective of the chapters which deal with theory. The business cycle is treated principally within the framework of the multiplier and accelerator mechanisms, and there are short but clear chapters dealing with fiscal and monetary stabilization measures.

The treatment of price theory is briefer, too brief to provide anything like a thorough coverage of the theory of the firm, although all of the usual points are made in dealing with pure competition and monopoly. Pricing in administered markets receives relatively more attention than is usually accorded it. Separate chapters are devoted to the monopoly and farm problems.

A marginal productivity theory underlies the discussion of distribution, but this is at no time made explicit by the author. Major emphasis is given to wage determination and the role of labor unions. The treatment of rent, interest and profit is relegated to a single short (22 pages) chapter.

Parts VIII and IX are devoted to international economics, the economics

of underdeveloped countries and comparative economic systems. These chapters are both interesting and informative, especially those which treat the difficulties encountered in increasing world trade and stimulating growth in the underdeveloped nations.

Those who regard the beginning course as essentially a course in principles will not find in this book an answer to their textbook problem. But the teacher who is prepared to place additional materials in the hands of the student (the text is rather short for use over two semesters unless such provision is made) and who prefers to emphasize institutional aspects will wish to consider this book. Those who offer a one-semester course may even find that the limitations of this text become desirable features.

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*The American Economy*. By GEORGE W. ZINKE. New York: Ronald Press, 1959. Pp. xvi, 704. $6.75.

Professor Zinke believes an introductory book for the newcomer to the discipline of economics should seek to present those principles that will be "operationally useful" in the next few decades, since "there can never be one set of economic principles that will be valid for all times." In presenting those principles, his stated objective is twofold: "To familiarize the student with the principles and institutions that are vital to the American economy today, and to provide a solid basis for more refined speculation in advanced courses" (p. v).

Part I includes first, a description of the economic system in terms of the major flows of economic transactions. There follows a chapter on "National Income and Prosperity," in which income concepts are encountered, and a brief and useful comparison of these concepts is made with money-flow economic accounting. This section closes with a private accounting view of the economy, noting in passing the relationship between national and private accounting.

In Part II, Zinke devotes almost 100 pages to an inventory of the U. S. resource base. Included are chapters on the natural, human, and man-made resources. The material on the resource base introduces in an interesting manner the basic economic problems of "abundance and scarcity," on the one hand, and efficiently using and developing resources on the other.

The student is introduced to employment theory in Part III. Of the five chapters, two are discussions of the determinants of the level of employment, and the remaining chapters indicate ways in which the prime determinants of employment are influenced, directly and indirectly, by government action.

Practical observations on commercial and central banking, governmental expenditures, taxation, and debt management are related to the earlier theoretical discussion and institutional framework. There is an attempt to "balance" theory and its applications which at times limits the theoretical analysis. This should lead instructors to utilize fully the excellent bibliography found at the end of each section.

Part IV, "The Efficient Use of Resources," is easily the best-written section of the book. Beginning with a summary explanation of price analysis, the

chapters move to price determination under the usual market structures. The assumptions are clearly spelled out, concepts carefully defined, and the conclusions carefully weighed. An interesting feature, found in the chapter on monopoly pricing, is an excellent summary of federal antimonopoly legislation from 1898 to 1958 with excerpts from the principal antitrust laws.

The concluding section is devoted to "Income and Wealth Distribution." One chapter describes the quantitative aspects of the U. S. income and wealth distribution, and the remaining six chapters are evenly divided between the determination of income shares through the price mechanism, and the determination of wages through individual and collective bargaining.

Even though the author insists, "This book is an introductory one whose aim is to explain the systematic aspects of the American economy rather than to emphasize the problematic aspects" (p. 341), he does manage to raise and discuss several political and ethical problems. This is particularly apparent in his discussion of fiscal policy and income distribution. He is careful to give conflicting points of view equal weight and to leave the decision as the "proper" policy up to the student.

In general, the level of analysis of this book is satisfactory, and one cannot quarrel with the claim of "realistic qualifications of abstract theory in the light of current developments." Definitions and concepts are introduced throughout the course as they are needed for immediate application. Suggested readings at the end of each section are rather well selected, and will be welcomed by the serious student. For each chapter there are questions for discussion, utilizing the principles and institutional materials found in the chapter. The frequent use of direct quotations from original sources is to be commended. This may whet students' appetites and encourage them to seek out and read the primary sources in the economic literature. This reviewer somewhat questions the use of over 80 graphs and more than 100 tables in an introductory textbook. Only too often an introductory text is a potpourri of topics treated descriptively without much unity or depth, and this should and has been avoided. However, there would have been some justification for using a smaller amount of illustrative materials and in their place including a chapter each on comparative systems, international trade, and economic growth. On the whole, this is a rather good book, and should have wide use.

ALFRED L. EDWARDS

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## Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

*Output, Employment, Capital, and Growth.* By HANS BREMS. New York: Harper & Brothers, 1959. Pp. xiii, 349. $6.00.

This volume represents another serious, scholarly attempt to generalize, dynamize, and mathematize Keynes' *General Theory.* Let us look critically into the details of the exposition with a view to making some general observations.

Use of "the autonomous-consumption multiplier" (pp. 21-22) is refreshing,

but its economics is less clear than if the consumption-multiplicand had been specified as that due to a social-security program which is nowadays the most significant factor affecting minimum consumption with zero income. Unfortunately this lack of concrete exemplification characterizes all the mathematical models of the volume, to the possible exasperation of the student (even at the graduate level). The multicountry open model of Chapter 4 adds realism without increasing the substantive understanding of the international spread of income fluctuations. Brems' elasticity discussions here are not coupled with the necessary caution that the explicit consideration of trend factors would render the elasticity factor largely irrelevant. The linearity of the Keynesian model is made to depend on the elimination of the "nonlinear" relation between output and the interest rate (p. 35), but the aggregate functions of that model could be made nonlinear by dropping the usual assumptions of a constant accelerator and other constant behavior parameters, regardless of the interest-rate variable.

In his criticism of "lack of generality" in the *General Theory*, Brems echoes J. R. Hicks' complaint about Keynes' neglect of the price mechanism and Harrod's lament over Keynes' dictum that "in the long run we are all dead." But Brems does not mention that aspect in which Keynes' short-run theory of employment must be supplemented by J. Robinson's long-run theory of structural underemployment. Chapter 7 rightly stresses the need for "disaggregation" in relation to policy, yet Brems' overenthusiasm should perhaps be tempered by J. Tinbergen's caution against getting lost in the maze of automistic data (in his "The Significance of Keynes' Theories from the Econometric Point of View" in *The New Economics*, S. E. Harris, ed., New York 1948) and by J. A. Schumpeter's warning against the scholastic inability "to say more than that everything depends upon everything," until nothing at all stands out as strategically the most important (see his "The Analysis of Economic Change," *Rev. Econ. Stat.*, May 1953).

Brems' notion of "cost-push" inflation (p. 71) as originating in "labor's pressure for higher wage rates and management's insistence upon 'full-cost' pricing" is of limited validity, for his model based on that notion not only depends on the restrictive assumption of "a monopolistic setting" but abstracts from the income-distribution and technological-demographical variables affecting factor costs. His disaggregative fiscal-policy model (pp. 71-72) is not disaggregative enough, since it ignores such fiscal-policy parameters as progressive and regressive tax rates, the government's separate marginal propensities to consume and to invest, and the explicit transfer-payment and social-security parameters. Brems' consideration of the possible effect of monopolistic competition (especially advertising) on autonomous consumption (p. 84ff.) is interesting, but he makes no allusion to J. Robinson's analysis of the distributive effect of monopoly on induced consumption or to Schumpeter's notion of monopoly-financed innovations and inventions on autonomous investment.

The further application of the balanced-budget theorem to an open economy is contemplated (Ch. 10), but the preclusion of a marginal propensity to invest and an explicit transfer-payment rate make less than conclusive Brems'

demonstration that the unbalanced foreign-trade multiplier is smaller than the balanced foreign-trade multiplier. Chapters 12 and 13 offer the disaggregative Leontief input-output model presumbly as a qualification of the Keynesian aggregative model, though without throwing much light on their relation. Some micro material (e.g., linear programming in Ch. 16) is inappropriately included in an expressly macro-economic volume, and it does not call for comment here. It is interesting that Brems' "modern restatement" of capital theory (Ch. 19) does not incorporate J. Robinson's recent contribution (*The Accumulation of Capital,* Homewood, Ill. 1956).

Part III dealing with "dynamization of the Keynesian model" may appeal most to the growth-conscious reader. There Brems refines and complicates the familiar Harrod-Domar models by introducing the foreign-trade sector and variable technological parameters, but then he, like Harrod and Domar, pays no attention to the increasingly important role of fiscal policy in the growth of a mixed economy. Finally, the stability conditions of dynamic equilibrium are considered in a valiant effort to integrate cycle and growth theories, but there is no critical examination of the concepts of the ceiling and the floor as the nonlinear constraints to an explosive linear system taken as an "approximation." The technological-behavioral content of his suggested hypothesis of "the length of the investment-planning period" as a stability factor is left as vague as that of the something-will-turn-up explanation of the turning points of the cycle.

The contribution of the book *as a whole* seems to lie in furthering modern "operationalism," that is, in giving quantitative precision and disaggregative emphasis to the intuitive and aggregative theories most closely associated with the name of Keynes. This book gives more credit to non-Anglo-Saxon (especially Scandinavian) economists and pays greater attention to methodological matters than do most American writings. Despite these over-all merits, the book is as long on manipulative technique as the comparable books of W. Baumol and other mathematical writers, but shorter on economic insight than those of, say, L. R. Klein and E. Domar (not to mention Harrod, Hicks, and J. Robinson). For this reason some readers might come away from this book with the same feeling as Harrod once expressed: "It is useless to refine and refine when there are no basic ideas present at all" (see his *Towards a Dynamic Economics,* p. 81). It remains to be hoped that future econometricians will, between the equations, show themselves more aware of the institutional-psychological complex of our economic society that might significantly alter theoretical and practical conclusions.

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*The Allocation of Economic Resources; Essays in Honor of Bernard Francis Haley.* By MOSES ABRAMOVITZ AND OTHERS. Stanford, Calif.: Stanford University Press, 1959. Pp. ix, 244. $5.00.

Writing a review of this collection of essays in honor of Bernard Haley is a bit like reviewing a volume of the *American Economic Review,* which is also a kind of continuing *Festschrift* in his honor. There is a fortunate difference,

however. The contributors to the book, though not the journal, have addressed their papers to general economists rather than to fellow specialists in their own fields. Since, moreover, the thirteen contributors constitute a group of leading U.S. economists in a wide range of specialties I am intrigued by the possibility of regarding the collection as an indicator of the current state of economics in the United States.

To particularize. Welfare economics is represented by two papers: Abramovitz' judicious evaluation of the problems of measuring welfare change when social conditions change concomitantly with measurable output, and Lindblom's consideration of methods for taking account of social values and moral standards in analyses of economic policy. There are three papers in the field of price theory: Alchian's re-examination of the notions of cost and cost functions, Arrow's discussion of neglected difficulties in the competitive price adjustment mechanism, and Houthakker's analysis of the economics of futures markets. Labor economics is also represented thrice: by Cartwright who considers whether unemployment insurance distorts or improves resource allocation, by Hilton who views the wage increases obtained by unions as taxes on payrolls which will be shifted fore and aft in such wise as to erode most of the gain in the long run, and by Reder who analyzes time series on labor's share of national income and related data to show that the marginal productivity theory of wages coupled to a Cobb-Douglas production function fits the facts (as Hilton's theoretical argument led us to expect) but that Kaldor's recent suggestion that labor's share is determined by the differing propensities to consume of wage-earners and nonwage-earners also fits. Chenery and Scitovsky write in the field of economic development: Chenery presenting an example of circumstances in which several sectors of an economy must expand either *pari passu* or not at all; and Scitovsky arguing contrariwise, in favor of unbalanced growth which permits an aspirant economy to reap the advantages of economies of scale by placing its few investible eggs in a comparably few industrial baskets. There is one paper on aggregative analysis: Baran's lively presentation of the familiar position that under capitalism, especially monopoly capitalism, so small a proportion of national income is disbursed to consumers and so large a proportion goes to the owners and managers of giant corporations that aggregate demand can be maintained at prosperous levels only by sedulous waste. Shaw contributes the only paper on money and banking. He claims to be fed up with the Federal Reserve Act under which we have suffered from an alternation of monetary leniency, when Treasury Department policy prevailed, and stringency, when the Federal Reserve Board was in the saddle. Monetary conditions would be sounder if we replaced the Federal Reserve Act by a legal requirement that the supply of money be expanded by, say, 3 per cent per annum, regardless of current conditions. Finally there is one paper on international trade: Tarshis reporting on an empirical study that confutes Leontief's paradoxical results and confirms the orthodox expectation that in any country goods that require relatively high proportions of factors that are relatively scarce in that country will be relatively high in price.

There it is, in somewhat less than a nutshell. One striking note throughout the volume is the determined embrace of orthodoxy. Reaffirmation of received

doctrine is the explicit thesis in Hilton, Reder, Scitovsky and Tarshis: 30.769 per cent ( I carry five significant figures so as not to be outdone by Cartwright). I can increase this orthodoxy rating to 38.462 per cent if you will permit me to include Marx' theory of the declining rate of profit in received doctrine, so that Baran can be counted as a reaffirmer.

A second group of four papers are exegeses or technical refinements of marginalist competitive doctrine. Abramovitz's, Alchian's, Arrow's, and Lindblom's contributions fall in this class. Two more papers, Cartwright's and Houthakker's, are applications of this doctrine to special institutions, namely unemployment compensation and futures trading. Is input-output analysis received doctrine by now? If so, Chenery falls into this class also. This leaves Shaw as the only potential radical or innovator but (1) he doesn't write as if he were in full earnest, and (2) I'm not sure that a proposal to supplant the Federal Reserve Board by a fully automatic money market counts as radicalism. In short, neither in substance nor in technique does this sample of recent work suggest important innovations or substantial revisions of the accomplishments of preceding generations.

At the same time, several of the papers display technical skill amounting to virtuosity in the use of the inherited tools in spite of the inhibiting effect of writing for an unspecialized audience. In my judgment, the outstanding papers from this point of view are Alchian's, Cartwright's, and Hilton's, and any one of them could serve as an exemplar of economic argumentation at its best. Hilton lays out with beautiful clarity the forces that press real wages and working conditions toward their marginal productivity levels (the same in all employments, making due allowance for nonpecuniary advantages and disadvantages), including not only the pressure of workers excluded from favored employments but factor and product substitution, and leads the reader to the ineluctable conclusion that unions cannot withstand these forces. Cartwright establishes that competitive wage rates do not reflect the costs to the worker of the varying degrees of instability of employment in different industries, Hilton's argument on equilibrating forces notwithstanding. Therefore, he shows, unemployment compensation schemes, which equalize these costs among industries, can improve the allocation of resources at the same time that they relieve hardship. This paper is a first-rate display of the application of classical tools to a significant policy issue and should be referred to as (1) an important contribution to the economics of ameliorative economic policy, and (2) a leading example of the use and power of rigorous economic analysis.

Alchian's purpose is to correct our slovenly habits of thought with respect to costs and cost curves. He advocates that we explicitly recognize that cost depends on much more than the instantaneous rate of output: in particular, on total volume, the length of the period of preparation for production, and the length of the period over which production extends. The chief omission from his catalogue of neglected influences on cost is the one that Modigliani, Hahn, *et al.* emphasize, namely the cost of changing the rate of output which can be approximated by the variance of the instantaneous rate. My major reservation about Alchian's recommendation is that in equilibrium conditions, which is about all that most economic analysis is competent to deal with, all these factors

except the old-fashioned instantaneous rate drop out. I wish I had space also to discuss and summarize Alchian's very stimulating, but not entirely acceptable, proposal for defining costs. Just between the author and myself, it seems to me that his definition amounts to the usual one but is less intuitive and more obscure.

In one respect this volume does not represent fairly the current state of economics. We can take pride in the high standards of conscientiousness and skill in handling statistical data that the profession has attained in recent decades, but they are not reflected in these essays. Only Cartwright, Reder, and Tarshis invoke empirical data. Tarshis's analyses are an extract from a more complete study as yet unpublished, so his statistical procedures cannot be judged. But Cartwright's and Reder's papers are both self-contained and their statistical sections are disappointing. Let me raise only one point against each. One of Cartwright's problems was to measure the instability of employment in various industries in the period 1948-57. His method was to determine, for each industry, the peak monthly employment in the first and last year of his series and to draw a straight line connecting them. Then his index of instability is the ratio of actual total employment during the nine years to what it would have been if employment in every month had been on this "full employment trend line." The inefficiency of this "trend" line in industries which grew along a linear trend is evident. In industries whose growth trend was not linear this concept has the additional drawback of incorporating in instability the consequences of the deviation from linearity. And, of course, it didn't work when applied to industries in which employment in some months exceeded the "trend." Why not measure instability by variance or average deviation around some more carefully estimated trend line?

Reder's statistics were at their weakest when he tested the hypothesis that Kaldor's theory is an adequate explanation of variations in labor's share in the period 1909-56. This is a classic problem in fitting and testing a set of simultaneous linear equations, and the canons for performing such a test are well established. Reder, however, instead of applying textbook techniques, pulled estimates of two critical parameters out of his hat, as far as I can make out. This is just unfair to the hypothesis under test. If the hypothesis barely squeaks through with the hat parameters, which it does, it could conceivably perform much better with properly estimated ones.

Taken as a whole this collection of essays is a readable, stimulating, brain-clearing draught for a professional economist who wants a few days off from his own little field but is wary of becoming enmeshed in other folks' arcana. He will learn a good deal, as I did, about what is going on in a variety of fields. The impression it gives of economics in the United States today is that we are fascinated by the power of inherited tools of economic analysis and highly skilled in their use, but are not making additions to the tool kit comparable in significance to, say, the Keynesian revolution, the development of national income accounting, or the invention of linear programming in the two previous decades. Nor are we very daring in our policy proposals.

ROBERT DORFMAN

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*Toward a Geography of Price.* By WILLIAM WARNTZ. Philadelphia: University of Pennsylvania Press, 1959. Pp. 117. $5.00.

The author notes that space and time have long been recognized by physical scientists as of the utmost significance in their disciplines. He suggests the similar need for these dimensions in economic analysis, proposes three hypotheses to explain the variation of prices over space, and tests his hypotheses in terms of the farm prices of various agricultural commodities.

The first hypothesis is called the gross economic population potential. This function relates the number and income of people at one place to all other places so that a land surface or map is visualized with contours connecting all points of equal magnitude of potential demand. Basic to the hypothesis is the postulate that every person influences demand but that his influence is smaller the farther away he is from a given place. To derive this empirically, the mean population in each state is computed and weighted by the average annual per capita income, the geographic center of each state is used as a control point, and 2304 measurements are said to be made of the distance from each control point to all others. The demand potential at any point is then adjusted to include the influence of nearby control-point demands. The resulting contour map shows that the greatest intensity of demand exists in the middle-Atlantic states and drops off from there in all directions with California a locally elevated "highland." Because Warntz is proposing a general model rather than special ones, he ignored such demand factors as income elasticity, custom, tastes, climate and their like. Also, because of the lack of detailed data, he ignored the impact of time variations on demand.

His second hypothesis holds that price over space varies not only in accordance with the demand at a point (as described above) but with product supply space potentials. Here Warntz again takes the state centers as control points using the production of the commodity in that state and the distances from one place to another as the basic cornerstones for his computation of the total supply potential at any and all control points in the country.

His third hypothesis covers the matter of time, by giving explicit recognition to the conditions of production of a given commodity at different times in different places. He divides the size of the output by the number of the units involved and asserts that the intensity of supply diminishes with time.

Warntz tests his hypotheses with wheat, potatoes, onions, and strawberries. The multiple correlation coefficients obtained yield estimations of spatial prices which carry predictive quality well beyond that (e.g., three standard deviations around the mean) of some less inclusive system. He asserts that the economic geographers' contribution to the analysis of economic phenomena lies in such general hypotheses as "price tends to vary directly with demand space potential and inversely with both supply space potential and supply time potential (p. 41). He goes on to test by beta coefficients the relative importance of each independent variable in determining farm prices and concludes that the population factor is more important than the supply space and time factors. He draws his inquiry to conclusion by suggesting certain other uses to which his theory may be put.

Warntz infers that, via his model, economic analysis now includes space

along with time in its framework. Unfortunately, of course, the full inclusion of space entails much more than observing differences in demand and supply at various places. Space impacts involve transport rates and practices, different spatial price systems (e.g., f.o.b., basing point, single, multiple, plenary, equalizing, delivered, etc.) and, as a consequence, different industrial markets with forms of competition and economic results which may or may not conform to classic conclusions. To include space in theory is a complex, far-reaching matter which, notwithstanding suggestions to the contrary, has not been pursued in the subject book. Hence the book will not satisfy those concerned with an all-inclusive space-time economic theory. In contrast, economic geographers in general and students studying agricultural prices will find many sources of ideas in Warntz' book.

MELVIN L. GREENHUT

*University of Richmond*

*Histoire de la pensée économique et analyse des théories contemporaines.* By ANDRÉ PIETTRE. Paris: Librairie Dalloz, 1959. Pp. 517.

This is an undergraduate textbook in which the history of economic thought and an analysis of modern theory are combined. Approximately half of the book is devoted to each subject. Although one might at first suspect otherwise, there is really no dichotomy between the two parts. As interpreted by the author, modern theory represents a revolution in economic thinking brought about by the latest developments in the commerical, agrarian, and industrial revolutions. He therefore presents modern theory simply as an accelerated aspect of the development of economic thought as a whole.

The writer considers that each civilization goes through three distinct economic stages. In the first stage traditional prerogatives, custom, and ethical concepts govern the form of the economic activity. The individual is subordinated to the system. In the second or middle stage the spirit of free enterprise emerges. Restrictive elements are cast aside, or at least subdued, and a climate of rationality, in which individualism flourishes, is fostered. In the third or final stage state regulations multiply until individualism and free enterprise are suppressed in the interests of state welfare. A lengthy and decadent period of state planning ensues. Is it not true, the writer contends, that civilizations are born amidst religious fervor and die in the throes of collectivism?

After briefly tracing the three stages in the Grecian and Roman civilizations, the author concentrates on the Western culture. The medieval period in Europe is, of course, an ideal example of the first stage. Also the spirit of liberalism, culminating in the doctrine of *laisser-faire,* clearly illustrates the second. It is when he turns to the socialist school of thought, and to communism in particular, that his Toynbeean interpretation encounters rough weather. He insists upon regarding communism as a reaction to liberalism, and, by implication, as a forerunner of the third stage. Yet Russia moved from the first stage directly into communism, and has experienced her greatest economic growth during the communist phase. Evidently a somewhat more sophisticated conceptual interpretation is needed to fit all the facts.

The discussion of modern theory in the second part of the book is remarkably comprehensive and clear. Indeed, the final section on economic dynamics and growth is outstanding. Students interested in getting a balanced perspective and a reasonably detailed knowledge of this important field would do well to study the section carefully.

Comprehensive treatment and clarity in presentation are really the main features of the entire book. The concepts of each notable economist are presented in their proper setting, and in a straightforward, pithy style. Moreover, the current French writers in economics appear to have a unique over-all insight which English and American writers lack.

This is certainly a book which students majoring in economics should read. The reviewer feels strongly that we are allowing the language barrier to cut us off, at least in part, from a valuable source of intellectual stimulation. Some ready facility for providing translations would undoubtedly enrich our economic thinking.

HENRY GRAYSON

*University of Maryland*

## Economic History; Economic Development; National Economies

*The Foundations of Capitalism.* By OLIVER C. COX. New York: Philosophical Library, 1959. Pp. 500. $7.50.

The author, a sociologist who is also a student of economic theory and of economic history, was led to undertake this study by the belief that the major social problems of contemporary society are essentially problems of capitalism, "the most powerful and dynamic form of social organization ever created by man." Capitalism is currently "undergoing such vital changes that an understanding of its nature becomes an immediate prerequisite to any meaningful interpretation of the subordinate though critical social issues involved." Finding available surprisingly little transmissible knowledge about its characteristics, he has sought in this volume to describe the origin and development of the social organization of the capitalist system. In a foreword contributed by H. E. Barnes, he states that this "is not a traditional economic history, with capitalism as the major theme, but a social history of the capitalist system and the spirit which engendered it, motivated it, brought about its integration as a system." The work is not addressed to scholars, although they will find its perusal interesting and often provocative, but to "a wider audience." Two more volumes are to follow in which the author proposes to analyze the "theoretical aspects of the social processes in the system."

As a form of social organization, the author believes capitalism to be constituted with a peculiar economic order, government, and religious structure. Economically, the capitalist city or nation is viewed as one in which expanding production and trade are stimulated by foreign markets. "Capitalism tends to form a network of national and territorial units bound together by commercial and exploitative relationships in such a way that a capitalist nation is inconceivable outside this capitalist system." Government in a capitalist society is controlled by or responsive to business men who operate

it to promote the welfare of the foreign and domestic commerce and industry of the state. The religious structure is one in which business and government are not subject to limitations imposed either by ideology or by a clerical hierarchy.

The emergence of these conditions required an urban setting. Such ancient cities as Rhodes, Athens, Carthage, Delos, Alexandria, Rome or medieval Constantinople did not meet all the requirements and the author believes that the favorable conjuncture was first achieved in early medieval Venice. Capitalist techniques were fairly rapidly diffused but not all urban centers in Western Europe made significant contributions to the growth and spread of capitalism. Special attention is given to Florence, Genoa, the cities of the Hanseatic League, staple towns and the fair towns. Holland is examined carefully as the nearest approach to a modern capitalist nation before England became so organized.

In the latter part of the seventeenth century the center of capitalist innovation shifted to England, an area that developed more rapidly than any other state into a modern capitalist nation. Among the steps in its development and unification discussed are: the gradual abolishment of all trading concessions to foreign merchants, the establishment of the supremacy of the secular power over the clerical and of Parliament over the monarchy, the organization of merchants in regulated and joint stock companies for the securing of trade abroad, the organization of domestic commerce and industry for more effective competition abroad, e.g., by the domestic system in the woolen cloth industry.

One of the most interesting sections of the book discusses the role of mercantilism in capitalist development. The author believes that the writings of the mercantilists appeared at the stage in the evolution of a capitalist nation when the merchant class saw the possibility of developing their nation to leadership in the capitalist system if only the noncapitalist authorities could be won over to the adoption of policies that had accomplished such an aim elsewhere or seemed likely to do so there. Hence mercantilistic writings in the author's view never appeared in countries so backward that leadership seemed unattainable nor in a political situation in which there was no opposition to be won over. For similar reasons they tended to fade away when leadership had been attained.

By the middle of the eighteenth century, England had developed all the major characteristics of a capitalist society, none of which had originated there. In the industrial revolution, however, England is visualized as adding an element of its own, a revolution in the technique and methods of manufacturing. Industry was a *dependent* factor in the capitalist economic organization, but England, having developed an effective capitalist system and having attained leadership in foreign trade, became the natural milieu for inventive activity, the principle reservoir of scientific and industrial knowledge as well, and thus achieved industrial supremacy.

Finally, the decline of England's leadership is attributed to the rivalry of younger and more resourceful capitalist nations, to the movement of colonial peoples to throw off the foreign grip, and to "the stagnation of the system itself." Aside from the statement that "the capitalistic nation can develop and

expand only as it expands its economic role in a 'wider world of sophisticated and backward countries" and "tends to decline and decay as opportunity for such expansion recedes or becomes limited," the explanation of this stagnation of the system is left to the future volumes.

JOSEPH A. BATCHELOR

*Indiana University*

*The Design of Development.* By JAN TINBERGEN. Baltimore: Johns Hopkins Press, 1958. Pp. vi, 99. $2.50.

*The Strategy of Economic Development.* By ALBERT O. HIRSCHMAN. New Haven: Yale University Press, 1958. Pp. xiii, 217. $4.50.

In these studies, two well-known economists outline principles to guide policy-makers in underdeveloped countries. Tinbergen's brief essay was written at the request of the International Bank for Reconstruction and Development for use in its Development Institute and hence is addressed primarily to government officials. Hirschman's work is aimed mainly at his fellow economists, since he calls into question the type of advice they are wont to give to governments of underdeveloped countries.

Although many of their recommendations are directly contradictory, Tinbergen and Hirschman start from similar premises as to the desirable institutional framework in underdeveloped countries. They seek ways in which governments can improve the working of an underdeveloped economy by a mixture of judicious planning of public investments and incentives to private producers. Their disagreements arise from different diagnoses of the basic limitations to growth and the potentialities for public and private action.

Tinbergen takes the view that deficient market mechanisms in underdeveloped countries produce "fundamental disequilibria" in which capital and often foreign exchange are undervalued, while labor is surplus. To make better use of resources under these conditions, an over-all development program which includes an analysis of the use of currently scarce factors and commodities is needed. Within this framework, governments should assign priorities to projects that directly and indirectly produce the greatest increase in national income.

One of Tinbergen's main contributions to the development of programming methods is the use of "accounting prices" for the assessment of investment priorities. The procedure outlined in Annex 3 follows the rationale of linear programming in using prices ("shadow," "accounting" or "equilibrium") designed to equate supply and demand for scarce commodities and factors in the long run. The valuation of projects using these equilibrium prices shows their true contribution to the national income and, in principle at least, takes into account all indirect effects—use of unemployed labor, improvement in the balance of payments, external economies, etc.

Hirschman objects to the formulation of over-all development programs in general, and in particular to the use of the productivity of investment, however calculated, as a basis for priorities. In his view, the rate of growth in the least-developed economies is limited not by the availability of savings but by the *ability to invest.* Although Hirschman admits that at some unspecified

point the savings capacity will replace the ability to invest as the factor limiting growth (p. 39), his analysis is almost entirely concerned with the first phase, in which he assumes that capital scarcity can be ignored. On the other hand, Tinbergen assumes that the capital supply is one of the main limitations. Many of the contradictions in their policy recommendations can be traced to this difference in assumption.

Hirschman considers the basic problem of the less-developed economies to be the expansion of the ability to make and carry out investment decisions. This limitation has been called "absorptive capacity" by other writers. It includes the perception of investment opportunities, the promotion of new enterprises, the overcoming of institutional obstacles, etc. Hirschman defines the ability to invest to include government as well as private decision-making; the former is assumed to be as much in short supply as the latter.

Making the best use of the limited ability to invest is the key to development policy for Hirschman. Since some decisions are more difficult to make than others, priority in the use of scarce entrepreneurial ability should be based on the extent to which investment in one project or sector will induce investment in others. For this and other reasons, Hirschman gives primary importance to the development of interrelated industrial sectors. To other writers, interindustry transactions affect the desirability of investment through the creation of external economies and diseconomies. To Hirschman, the establishment of one or more links in a chain of producer relations may also induce or even compel investment in other sectors by reducing uncertainty and making the decisions easier. In this respect, pressure on the government to build power plants or railroads to serve newly established plants is no less important than market inducements to private investors. The basis of development strategy is therefore the discovery of the key links in the chain which will create a maximum pressure for further investment. From this point of view, Hirschman suggests that it may be more effective to deliberately create shortages of power and transport, within limits, than to invest in anticipation of them.

Hirschman's emphasis on the ability to make investment (or development) decisions leads to a theory of development which is more applied psychology than economics. Since in this view capital and other input limitations are illusory, the economist is left with nothing to economize except the elusive quality of decision-making. Numerous paradoxical results follow from this premise. Investment in capital-intensive methods may be preferred because they provide greater penalties for lack of efficiency, because "process-centered" industries, such as chemicals or metallurgy, require less managerial skill, or for other reasons that are alleged to economize on the scarce ability to invest.

Whatever the merits of including motivational factors in a theory of economic development, Hirschman surely overstates his case. Even if all his speculations about the responses of investors, managers and bureaucrats are correct, the scarcity of decision-making ability cannot be made to serve as the sole guide to development policy any more than can capital scarcity or the other criteria that he rejects. A policy-maker maximizing induced investment

would run into shortages of foreign exchange, saving, or skilled labor unless he had taken them into account in setting his priorities. It is therefore hard to see why Hirschman makes the virtues of unbalanced, investment-stimulating projects a basis for rejecting the over-all calculations implied by a development program, which he declares to be "often quite unhelpful" (p. 205). He apparently is willing to attribute great psychological insight to policy-makers but only very limited ability to them as economic analysts.

To this reviewer, the case presented by Tinbergen for an over-all development program is not materially affected by the recognition that the sequence in which projects are undertaken may affect the total volume of investment. It would not be hard, in fact, to modify Tinbergen's programming framework to take account of Hirschman's innovations. The calculation of accounting prices for assessing priorities can include differences in the entrepreneurial requirements of different types of production as well as of other scarce factors. An over-all analysis would avoid the overemphasis on one set of factors to which Hirschman, like other innovators, is prone.

Despite the one-sidedness of his analysis, Hirschman has performed a real service in going beyond the conventional acknowledgment of the importance of external economies and probing the implications for investment behavior of interdependence in production. His suggestion that some growth sequences are likely to be more effective than others is a promising subject for further empirical study.

HOLLIS B. CHENERY

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*Stability and Progress in the World Economy*. The First Congress of the International Economic Association. Edited by DOUGLAS HAGUE. New York: St. Martin's Press; London: Macmillan Co., 1958. Pp. xv, 268. $5.00.

Is economic growth compatible with stability? What are the special problems facing the richer economies in reconciling these two goals? What about the problems of the poorer economies? What are the real, and what are the monetary factors that condition growth and stability? Finally, how are the results modified by reason of the fact that the individual economy operates in an international setting? These are the problems to which the participants at the Congress, held in Rome in September 1956, addressed themselves. This is a pretty big order, even for an International Congress which can tap the wisdom of the whole world—in principle, anyway. And it often, under different circumstances, seems to invite not much more than windy generalization, and a complete absence of communication. Luckily, considering the importance of the topics, the Congress seems to have been unusually successful. The five main speakers and the invited discussants addressed themselves to the questions, and the record of their remarks provides a very useful treatment of these problems.

It would be too much to expect that the remarks of all the other participants would add anything like as much in value; and they did not. In fact some of these pages are as funny as anything published: but since this sort of

entertainment was presumably not intended by those who organized the conference, they do no more than to illustrate the danger of allowing everyone who can pay the fare to get his remarks into print. Stephen Spender surely got some of the raw material for his short novel, *Engaged in Writing,* from parts of the discussion. Charity, extolled by Pope Pius XII in a thoughtful address to some of the participants, forbids my saying more on this matter.

To return to what *is* useful; Dennis Robertson described the problems of the richer economies in a felicitous and provocative account of the factors conditioning growth, the obstacles to growth, and the reasons why it is likely to proceed jerkily. Jacob Viner covered the same questions for the poorer countries in a stimulating paper which was, apart from the conclusion, pessimistic about growth, and less so about stability. François Perroux and Gottfried Haberler then covered the same range of questions from a somewhat different standpoint: the former dealing with the real factors that bear upon stability and growth; while the latter concerned himself with the monetary factors. Both accounts are decidedly worth while: Perroux's for the novelty and scale of its ideas, and Haberler's for its judicious and scholarly survey of contemporary thought. J. R. Hicks' comments on Perroux's paper are also worth careful attention. Finally, Erik Lundberg addressed himself to the problems of international stability, and the implications for growth and internal stability.

There is much more of value in this book--as, for example, W. A. Lewis' discussion of Lundberg's paper; Erik Lindahl's and Abba Lerner's comments on Haberler's; Erich Schneider's remarks on Perroux's; and several others. And while no one would claim that the basic puzzles have been answered, the volume will be a useful one to all those who are looking for answers.

LORIE TARSHIS

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*The Growth of Industrial Economies.* By W. G. HOFFMANN. New York: Oceana Publications Inc., 1959. Pp. xiii, 183. $6.00.

One of the achievements of the Weimar Republic was the establishment of a number of economic research centers, including the Institut für Weltwirtschaft at the University of Kiel. The volume under discussion is a translation of a revised and updated monograph published by the Institut in 1931. In its original conception, the investigation owed much to the then popular distinction by Böhm-Bawerk between primitive and capitalist production. The general argument can be stated simply. On the basis of an extensive review of empirical information, Professor Hoffmann holds that, as the process of industrialization progresses within a country, the composition of the output of its industrial sector gradually shifts from overwhelming emphasis on consumer goods to a larger and larger portion of capital goods. The process is continuous, highly perceptible, and universal in its application to industrial economies, no matter when industrialization starts, although the rate of progress has differed substantially from country to country. He works with data on the industrial patterns of twenty-odd countries. For purposes of elaboration, three "stages" of industrialization are discussed. Stage I relates to that period

when the net output of consumer goods industries is roughly 5 times that of capital goods; Stage II, when the ratio is in the neighborhood of 2½ to 1; and Stage III, when the outputs of the two industrial groups approach a balance.

The United States was the first nation to enter this third stage of industrialization about 1890, followed by Britain and Germany in the first decade of the present century. The industrial composition of the economies of Brazil, New Zealand, and Argentina by the second world war were reaching the second stage, which the United States had achieved in the mid-19th century. In contrast, Australia, Canada, and South Africa, where the initial phase of industrialization occurred during the latter part of the last century, had by the early postwar period passed into the third stage. An investigation of dominant industries during the various stages of industrialization is not too instructive, but it indicates a tendency for the textile or food industries to dominate in the early phases, whereas among the capital goods industries the machinery industry tends toward dominance in the third stage.

Hoffmann employs his data, censuses of production or employment series, with reasonable caution and establishes his principal thesis without question. The logical problem posed by industries not easily grouped into one or the other of the two categories is disposed of satisfactorily by a demonstration that the margin of possible error cannot significantly influence the results. While it is possible from a later point of vantage to ask whether or not the consumer durable and residential building industries might more properly be considered capital goods industries, since their products provide a flow of services over a period of time, such a change would not alter, but in fact enhance, Hoffmann's conclusion. Despite the adequacy of his handling of the descriptive material, one finds less satisfaction in the explanations of the phenomena observed. Perhaps this is due in part to the crudeness of the theory examined —the effort to discuss systematic changes in industrial composition independently of the rate of industrial growth or the magnitude of the industrial economy by itself or relative to the rest of the economy. Certainly the contemporary reader will immediately ask what was the relationship between industrial composition and the periods of most rapid output growth for various countries.

Similarly, the reader is left to his own resources to interpret the significance of the industrial pattern so ably charted. Possible implications for the underdeveloped areas immediately suggest themselves. The modest industrial beginnings of Britain, the United States, or, more recently, New Zealand ought to have a sobering effect in those countries where the initial impulse is to erect an integrated steel mill. However, when Hoffmann does occasionally venture a policy appraisal, as the statement that "On the whole, government action has tended to foster rather than hamper the normal trend of industrial change," it is sometimes unwarranted. If the pattern of industrialization does behave in systematic fashion, the question logically arises as to the costs or benefits of altering or hastening its development. The materials for a partial answer are at hand, but not without venturing into the measurement of growth in net output and its attendant problems of aggregation.

After the passage of almost three decades, was it worth undertaking the

translation of a work somewhat dated in theoretical conception, notwithstanding the current interest in the topic? This reviewer answers with an emphatic affirmative and commends the English translators, W. O. Henderson and W. H. Chaloner, for their judgment and efforts in making the work more accessible. The exposition is exceptionally good and the author's grasp of the relevant materials comprehensive. Economists in general should be interested to find in the third chapter an excellent brief summary of world-wide industrial history. Those interested in the process of economic growth will find, as this review indicates, that the book will stimulate many questions. Not the least of these is as to the extent and effectiveness of the distortion of the consumer goods/capital goods ratio in those nations devoted to rapid industrialization at forced draft.

ROBERT S. ECKLEY

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*Lectures on Economic Development.* By JOHN H. HABAKKUK and others. Istanbul: Faculty of Economics, Istanbul University and Faculty of Political Sciences, Ankara University, 1958. Pp. viii, 200. 7.20 TL.

This volume is made up of lectures delivered in the Spring of 1957 under the auspices of the Faculty of Political Sciences, Ankara University, and of the Faculty of Economics, Istanbul University, in cooperation with the Rockefeller Foundation. The lecturers whose work is published were: Professor John H. Habakkuk of the University of Oxford on "The Entrepreneur and Economic Development" and "Population Growth and Economic Development"; Alfred Sauvy, director of the National Institute of Demographic Studies, Paris, on "Progès technique, emploi et chômage," and "La population des pays sous-developpés"; Professor Pasquale Saraceno of the Catholic University of the Sacred Heart, Milan, on "Initiative privée et action publique dans les plans d'industrialisation des regions sous-developpées"; Dr. Hal B. Lary, director of research, Economic Commission for Europe, on "Economic Development and the Capacity to Import—National Policies" and "Economic Development and the Capacity to Import—International Policies"; and Professor Ragnar Nurkse of Columbia University on "The Conflict between 'Balanced Growth' and International Specialization" and "Some Reflections on the International Financing of Public Overhead Investments."

As in most symposia, the quality of the individual contributions varies considerably; in this instance from the scrappy, confusing, and quite unconvincing dicta of Lary in favor of a "systematic but judicious intervention in their foreign trade" by means of protective tariffs and government rigging of multiple exchange rates which somehow or other will fenagle the unfortunate operation of the principle of comparative advantage which he believes operates against the economic development of the primary-producing countries to the brilliant and all too brief observations of Nurkse. The volume is marred by a fantastic number of misprints in both English and French. In spite of these drawbacks, the book is important and deserves careful study by all economists concerned with problems of economic development. It would be desirable to

have the book reprinted by one of the university presses with a critical and synoptic introduction, removal of the misprints, and translation of the French lectures so as to make it more widely accessible for student assignment.

Habakkuk's initial lecture is a stimulating discussion of the importance of "imported entrepreneurship" for the underdeveloped countries. Interesting as it is, it is far surpassed by the stimulating observations in his second lecture on population growth and the importance of the "kinship" family as opposed to the "nuclear" family as a key factor in economic growth. He concludes that one of the main reasons for the disappointing results of private enterprise in developing these countries is that labor-intensive techniques in countries of surplus labor will offer most profit to the entrepreneur but capital-intensive techniques offer the best prospects of progress. Thus the entrepreneur's search for profit maximization works against rapid economic growth.

Sauvy warns that whether technical innovation will cause employment or unemployment depends on the type of innovation; labor-saving innovations will merely add to the employment problems of countries with surplus labor. He is pessimistic about either an economic or a demographic solution to the problems of the under-developed countries without a preceding cultural revolution. He concludes that whether Russia or the United States succeeds in dominating the world, the victory will contain the seeds of inevitable defeat unless the population problem is first solved.

It is impossible to summarize in a few sentences Saraceno's detailed and thought-provoking analysis of industrial development in the older industrialized countries with their willingness to adjust to the social revolution implicit in economic revolution and the lessons to be drawn from this experience for the underdeveloped countries. He emphasizes the danger of considering the economic problems of these countries in terms of a Western type free-market model when they are structurally and culturally so different from Western economies. He also warns economists to remember that government development plans are political ammunition as well as economic projections and should not always be taken too seriously as statements of government intent.

Nurkse brilliantly throws new light on international trade as a transmitter of economic growth and on the enormous importance of the infra-structure of social capital necessary before industrialization is possible. He also observes that expansion of national income does not necessarily result in balance-of-payments difficulties for underdeveloped countries if the expansion is the result of a change in their economic structure.

The first effect of these lectures is depressing. The problems seem so complicated; past experience so irrelevant; and traditional theory so in need of reinterpretation if it is to be helpful. For this reason, however, the book is challenging. It would be the sheerest defeatism for economists *not* to attempt the basic re-examination of Western-type theory needed to make it helpful in the solution of these problems—for unless these problems are solved there may well be no Western economics in another fifty years.

J. TAYLOR

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*The Economics of Underdevelopment—a Series of Articles and Papers.* Edited by A. N. AGARWALA and S. P. SINGH. New York and Bombay: Oxford University Press, 1958. Pp. viii, 510. $4.65; Rs 17.50.

The appearance of a collection of readings in the field of economic development of underdeveloped countries may be taken as a sign that the literature at least can no longer be termed "underdeveloped." The editors have selected 21 papers from the vast output of contributions to the economics of growth and grouped them in 6 sections. The first, titled "Approaches to the Problem of Underdevelopment," contains papers by Jacob Viner, Colin Clark, Gerald Meier, Paul Baran and Hla Myint. The second section is historical and contains papers by Simon Kuznets and W. W. Rostow. The third section, primarily theoretical, is designed to provide side by side the Schumpeterian, the Keynesian and the Harrod-Domar conceptual frameworks. These are discussed in papers by Henry Wallich, V. K. R. V. Rao and Henry Bruton. The fourth section contains 5 selections all dealing with "External Economies and Balanced Growth" and includes contributions by P. N. Rosenstein-Rodan, Ragnar Nurkse, J. Marcus Fleming, Tibor Scitovsky and Celso Furtado. Two papers, one by Alfredo and Ifigenia Navarette, the other by R. S. Eckaus, constitute the fifth section; both papers concern themselves with underemployment and factor-proportions problems. The last section presents four models of economic development, by H. W. Singer, W. A. Lewis, Hollis Chenery and Martin Bronfenbrenner.

In their preface Agarwala and Singh admit that their selections can be criticised but say that they hope to publish two additional volumes to include important contributions which have been left out. The purpose of the editors was to give economists in the underdeveloped countries a ready access to widely scattered material and they hope "that the studies presented in this volume will stimulate those in the underdeveloped countries to think further for themselves." There can be little question that the purpose is a good one and that the majority of the selections are excellent in their own right. At the same time, one is struck by the fact that, with the possible exception of the Viner article, all the contributors take for granted the desirability of fostering economic development and they implicitly assume that the progress of underdeveloped nations will require some degree of planning, international aid and cooperation. Viner, though somewhat more caustic and skeptical than the others, focuses specifically on obstacles to growth and he too is committed to the value premise that economic development is a good thing and has to be fostered somehow. One would wish for the inclusion of a paper by Frankel or Bauer if only to provide a more rounded exposure to Western thought.

More serious, perhaps, is the fact that the "selectors and editors" have discharged the former function to the detriment of the latter. There is a good deal of duplication of material that a little judicious editing could have minimized. Such topics as "hidden unemployment," "social overhead," "vicious circles" and others are discussed in several readings, often in very similar terms. Moreover, some of the readings deal with topics that are equally well if not better treated in others. This is especially true in Section IV where the

contributions by Rosenstein-Rodan and Scitovsky would seem to be adequate by themselves. The contribution by Eckaus leaves little room for those by Rao and Navarette.

Of special interest to Western readers are the three articles written by economists from India, Mexico and Brazil. In a paper entitled "Investment, Income and the Multiplier in an Underdeveloped Economy" Rao takes to task those who would apply Keynesian analysis to the problems of hidden or disguised unemployment. He points out that when the supply elasticity of capital and other inputs is low an increase in the aggregate demand for goods and services leads to inflation rather than to expansion of output and employment. His prognosis, even in the face of some unemployment, is not deficit financing but rather the classical prescription of "work harder and save more." The Navarettes also address themselves to the problem of disguised unemployment that they consider peculiar to pre-industrial countries. When the term was first introduced by Joan Robinson it referred to the situation where a fall in aggregate demand led to withdrawal of labor into occupations with low or zero marginal productivity. In the underdeveloped countries this form of unemployment takes place during expansion and manifests itself in the multitudes of peddlers, vendors and beggars in the cities of the East. It is interesting that the Navarettes, whose analysis follows closely that of Rao, do nevertheless think that some degree of inflation is not only inevitable but actually to be welcomed in facilitating the shift from "rural leisure" to industrial employment. They would like to control and contain the degree of inflation but they deem it a positive contributor to economic growth if only through its effect on real wages.

The article "Capital Formation and Economic Development" by Furtado takes issue with several of Nurkse's contentions. In particular Furtado argues that only an external stimulus, more precisely a change in the terms and volume of foreign trade, is likely to provide the necessary impetus to economic development and that the policy of "lifting oneself by one's bootstraps" is doomed to failure. The article is analytically rather uneven but it contains two discussions that show insight. In his criticisms of the market as the optimizer of resource allocation, Furtado insists that this is the fundamental theoretical problem of economics of underdevelopment. Toward the end of his article he calls the provision of incentives to save rather than to invest the central policy problem of pre-industrial countries. It is one that is nowhere discussed in the book.

ALEK A. ROZENTAL

*Saint Louis University*

*Economic Development: Principles, Problems, and Policies.* By BENJAMIN HIGGINS. New York: W. W. Norton & Co., 1959. Pp. xvii, 803. $7.50.

This is clearly the most satisfactory textbook yet published in the field which is rightfully coming to dominate our interests. Professor Higgins opens his preface with the frank recognition that a textbook, as ordinarily conceived, presenting received doctrine in a form understandable to students and intelligent laymen without previous training, cannot yet be written in this field.

But he goes on to point out the urgent need for answers to pressing policy questions, and adds: "so we must try to meet the demand for policies—and for textbooks—before we are altogether ready." Fortunately, the author is well equipped for this attempt, being a soundly-trained economist, interested in theory, with something like a decade of field experience in four underdeveloped economies. His work is thoughtfully eclectic, drawing on the work of many others, especially that of his associates at the MIT Center for International Studies.

With a breadth appropriate to its subject, the book is divided into 30 chapters grouped into 5 parts. The 2 chapters of Part I are introductory. The 6 chapters of Part II provide a tightly organized review of developmental concepts in the work of the major classical writers, Marx, Schumpeter, Hansen, Harrod and Domar. Two short chapters in Part III offer a hasty survey of lessons to be drawn from the capitalistic record. This first third of the book is preliminary. In Part IV, 7 chapters present selected theories accounting for the undeveloped state of many economies. A wide range of literature is cited, and noneconomic factors are given considerable attention. Finally, in Part V, the last thirteen chapters (343 pages), offer a wide-ranging series of discussions on the policy issues that are confronted in attempts to initiate and sustain economic development.

The emphasis on theory is to be commended. Everyone sees that extensive descriptions of actual situations, together with reviews of historical developments, are necessary in this field, but one also quickly becomes aware of the dangers inherent in these quicksands of detail. Higgins has had his share of immersions, but he here presents a coherent set of analytic generalizations resting upon the field evidence.

Another strong point of the book is its attention to psychological, sociological, and political aspects of economic development problems. He draws especially on the recent work of Everett Hagen and David McClelland, along with more familiar earlier notions. It has become commonplace to call for consideration of these related problem areas, but Higgins goes further than most in drawing such variables into his account of the development process.

The strong emphasis on policy reflects Higgins' background and concern. There is, as he says, an element of the treatise in his work, and at times also an element of the mission report to a host government. Some economists may shy away from these more operational parts of the book, but most, I suspect, will find them extremely rewarding.

If it is a reviewer's privilege to nominate particular chapters as his favorites, I would draw attention to Chapters 2 and 29—where Higgins first gives thumbnail sketches of the situation in six countries: Libya, Indonesia, the Philippines, India, South Italy, and Mexico, and then, at the end of the book, reviews "some lessons of experience" with developmental programs in five of these countries. In Chapters 22 and 23, Higgins discusses the tax problems that arise in fostering development and proposes an ingenious mechanism for minimizing tax evasion and maximizing growth, while raising government revenues substantially. His long chapter on the planning of economic development is outstanding. For instructors seeking quick summary statements,

Chapters 17 and 18 bring together the elements of Higgins' development theory and his policy approach to the fostering of economic growth. Finally, his concluding chapter of proposals for further research would stimulate discussion toward the end of a graduate course.

We now have a number of valuable works in this field, and without stressing invidious distinctions, comparisons between them and Higgins' text may be useful. First, there is a vital matter of methodological bias. Higgins begins his final chapter:

> The major thesis of this book is the need to break out of the traditional molds of economic thought when we come to analyze the process of economic growth. Both scope and method of development economics must be different from those of equilibrium economics, and policy recommendations based on misapplication of the methods of traditional equilibrium economics are likely to take us far astray.

This conclusion is basically different from that of Meier and Baldwin in their 1957 *Economic Development* (p. 448):

> . . . the study of economic development should not be confined to poor countries. The problem of maintaining development in rich nations is equally relevant. To restrict the subject to poor areas alone is to lose an essential sense of continuity in the concept of economic development. For, although there are many political, sociological, and economic differences between rich and poor nations, the fundamental economic forces and processes involved in development are basically similar in the two cases. There is no sharp dividing line between theories of development as applied to poor and rich countries.

Higgins believes strongly that Western tools of analysis can be applied to the economies of non-Western societies (see his discussion on pp. 288-93), but he insists on the importance of recognizing differences in degree, especially in the surrounding environment. This is what aligns his work with that of Hirschman or Myrdal, and differentiates it from the Meier and Baldwin approach, exemplified also in the 1955 Buchanan and Ellis study, *Approaches to Economic Development*. Higgins' text draws on more recent evidence and is more sharply focused than W. A. Lewis' *Theory of Economic Growth* (1955); and it provides far fuller coverage on all topics than C. E. Kindleberger's *Economic Development* (1958), though both these works are better written than the Higgins study. On issues of public versus private ownership, the effectiveness of market forces, and the proper scope of state-initiated development programs, readers will find a far more balanced treatment in Higgins than in the somewhat apoplectic approach of Bauer and Yamey in their *Economics of Underdeveloped Countries* (1957). Higgins' analysis is considerably more up-to-date and advanced than that in Williamson and Buttrick's pioneering *Economic Development* (1954).

For all its achievements, the volume under review cannot be said to have accomplished fully its author's objectives. He hopes that it "will be intelligible even to people with no previous training in economics," but this does not seem likely. Even for those who have had a good introductory course, the book will be difficult. Partly this is because extraordinary craftsmanship is required

to present complex, technical material in compact, introductory terms. The analysis must be unfolded with painstaking lucidity, anticipating all the puzzles standing before the beginner. Moreover, innumerable details must be handled with precision, and on both these counts, Higgins' text is deficient. Abundant use is made of statistics and of a wide literature, but sources are incompletely and carelessly identified. Consequently it will be unnecessarily difficult for an interested reader to follow up the leads Higgins provides. Frequently terms are used without definition, and some of them are real mysteries. Ordinary proofreading slips are numerous. Finally, in attempting to present the work of others in compressed form, Higgins appears to me to have produced several opaque passages. It would be well, for example, wherever feasible, to put students into direct contact with the recent writings of Hirschman and of Leibenstein. For students with a good background, in fact, use of Higgins as a text, plus a reading list of selections from such authors, may be the best available thorough introduction to this subject.

HOLLAND HUNTER

*Haverford College*

*Economic Development*. By HENRY H. VILLARD. New York: Rinehart and Co., 1959. Pp. xiv, 229. $2.50.

For those who feel that the introductory economics course should present a substantial block of material on economic development, this simple and stimulating essay may provide an attractive answer. Its main theses are that technological progress should be speeded up in the West, and that population growth should be checked all over the world, especially in underdeveloped areas. The argument is laid out in a form both temperate and tentative, while at the same time the author's value judgments are sharply defined. There is a refreshing emphasis on the long-run factors that underlie economic growth, and this emphasis should do much to attract thoughtful students to the field of economics.

After an introductory discussion (meaning, measurement, and comparison of income levels), Part II is devoted to a lively analysis of American and British experience since 1875, designed to show that effective research and innovation have been the keys to our economic progress. Villard compares the performance of the United States and the United Kingdom in this respect, evaluates the relative merits of oligopolists and purely competitive firms in developing technology, and discusses the functions of government in stimulating technical progress. He distinguishes among fundamental research, practical research, and innovation, illustrating their importance with a variety of case studies and statistical data. Parts III and IV offer brief surveys of Soviet economic growth and the situation confronting underdeveloped areas, especially India. The section on the USSR is reasonably accurate and reflects good judgment in the selection of topics for discussion. The material on underdeveloped areas is, in my view at least, less well balanced. Two-thirds of the discussion concerns the dangers of continued rapid population growth, with only some 11 pages devoted to problems of increasing the output of goods and services. Here the emphasis is on the great difficulties involved (see p. 201). And yet as the

author remarks in another connection (p. 189): "Because the problem is difficult and not entirely economic, there has been a tendency in economics to state the difficulties and let the matter drop. This is unfortunate."

Recent population trends cannot fail to give a dispassionate observer pause, but Villard weakens his case (p. 180) with unnecessary exaggeration in his statistical example of "possible world population" if recent trends continue. The 1950 world total of 2.5 billion people would not be 1526 billion in the year 2350 with a doubling every fifty years; it would only (!) be 640 billion. The author's conclusion is that the United States can best contribute to the economic development of underdeveloped areas by developing simpler and more effective contraceptives. This one-sided emphasis on the population side of the problem, while it will undoubtedly awaken students to a serious issue, carries with it the danger that students who have only this one brief exposure to the matter may develop distorted lifetime judgments on American public policies. Consequently instructors who assign Villard's discussion may want to couple it with something like Wilfred Malenbaum's recent 67-page paperback, *East and West in India's Development.*

HOLLAND HUNTER

*Haverford College*

*The Inflationary Spiral, The Experience in China, 1939-1950.* By CHANG KIA-NGAU. New York: The Technology Press of Massachusetts Institute of Technology and John Wiley and Sons, 1958. Pp. xvii, 394.

This is an important as well as a good book. It represents a thorough, workmanlike, and lucid contribution to our knowledge and understanding of one of the classic and most catastrophic inflations of modern times—an inflation with more far-reaching political consequences than any of its kind in Hungary, Germany or Greece.

The author, one of the incorruptible elder economic statesmen of contemporary China, is eminently well qualified to write on this subject. As a long-time banker, former Minister of Communications, and Governor of the Central Bank of China, he was both a participant in and a close observer of the events and policies he discusses. The central thesis ably developed is that hyperinflation in China was primarily brought about by chronically huge and growing government deficits. The deficits in turn were preponderantly financed by note issue or government bank credit largely due to enormous military expenditures well beyond the capacity of the economy to bear. This was reinforced by easy credit policies designed to encourage expansion in wartime industrial production in the greatly shrunken area of unoccupied China. In wartime, military and government expenditures in general were rising when the country's revenue and supply base was greatly narrowed as a result of Japanese occupation of the coastal and some inland areas of China. The inflationary process continued unabated and with even mounting speed in the postwar period, fed by largely the same factors. Government deficits continued to rise owing to military expenditures incurred in waging the civil war against the Communist armies. The inflationary pressures were further aggravated by the postwar release of

long pent-up demand financed by dishoarding and easy bank credit. At the same time, owing to war devastation, Communist-engendered disruption of communication, and dismantling of Manchurian productive capacity by the Russians, the country's capacity to supply expanding postwar demand was seriously undermined. Moreover, all of these conditions were critically accentuated by maladministration at all levels and in all fields, by technical failures in fiscal and monetary management and by an incapacity to administer direct controls. The author finally tries to show that in general terms underdeveloped areas are much more vulnerable to inflation and less capable of containing it than more developed, commercialized, and monetized economies. Precisely for this reason, he attempts to draw some lessons from the Chinese inflationary experience with a warning to the governments of underdeveloped areas to heed this experience so as to avoid a similar economic and political disaster in their own countries.

This reviewer finds himself in general agreement with Professor Chang's over-all thesis with two reservations. First, it would be my inclination to attach much less importance than the author does to failures in tax administration, the implementation of direct controls, and to irrational banking and credit policies. Not that these were insignificant in and of themselves, but as fuel feeding the inflationary flames, they were so overwhelmed by the fantastically high government deficits and by wasteful military outlays that even with the most exemplary policies in these fields, it is most doubtful that the hyper-inflation could have been stemmed.

Second, although the analysis of inflation potential in an underdeveloped area (Chapter 5) is interesting, it is less than convincing when applied to China. It is of course perfectly true that for the reasons outlined by the author, it may be more difficult to stem the inflationary tides in underdeveloped countries. However, the forces which propelled the Chinese inflation were so potent that they would have been sufficient to overwhelm a sturdier and more modern economy.

The general theme of the book is developed in four parts. Part I contains a historical account of the Chinese inflation divided into five chapters: the period up to 1939, war inflation, wartime changes in income distribution under the impact of inflation, postwar inflation, a theoretical model of inflation in underdeveloped area and its application to China. Each of the three historical chapters describes the contribution made to rising inflationary pressures by the government deficit, expansion of bank credit, supply responses, behavior of the foreign exchange rate and government policies.

Roughly the same ground is covered in greater detail in Parts II and III. In Part II, the author describes and appraises prewar, wartime, and postwar government finance. Each of these chapters in turn analyzes the over-all budget, the growth of different types of expenditure, and the revenue situation. Additional chapters deal with the state of the money market and private credit, and with aggregate supply conditions during the 1937-1949 period, i.e., trends in production, net imports and distribution.

Part III is an exposition and critique of government policy with a chapter each devoted to the following fields: fiscal and monetary, foreign exchange,

foreign trade, and direct controls. Finally, Part IV attempts to draw some lessons from the Chinese inflationary experience.

While this pattern of organization contributes to the thoroughness and detail of the book, it also makes it quite repetitious and tedious. Essentially, each of the three parts goes over the same material from different points of view. Therefore, one cannot help but feel that the book would be much better if it were considerably shortened.

The contribution which this book makes is also seriously undermined by the author's failure to discuss and appraise the validity, reliability and limitations of the statistics on which the narrative and analysis is based. The 95 tables in the text and the 12 in the Appendices have only passing and cursory source notations which do not tell us how the original data were compiled. The author's obligation in this respect is the greater in the face of his dubious claim that "the figures I quote and some estimates I make are based upon statistics which are accessible only to me."

ALEXANDER ECKSTEIN

*University of Rochester*

*The Commonwealth Economy in Southeast Asia*. By T. H. SILCOCK. Durham: Duke University Press, for Duke University Commonwealth-Studies Center, 1959. Pp. xvii, 259. $4.00.

The literature on economic development is pouring off the presses so rapidly that it is more than ever important to classify the contribution of each new work. Professor Silcock, of the University of Malaya, in these Duke University lectures gives a picture of the diversity of economic development in an area starting with Malaya but swinging out to include Singapore, Sarawak, Brunei, North Borneo, and in a few instances going as far as Hong Kong. No attempt is made to develop new theories or precise models, but students wanting to learn more about the process of economic development will find many penetrating insights based on the author's intimate knowledge of Malaya, Singapore and Sarawak. A 34-page classified and annotated bibliography is included to guide scholars who want more information on various aspects of the Malayan economy.

Students interested in the critical relation between economic development and various stages of colonialism and independence will find much material in these lectures delivered just after Malaya had become an independent member of the Commonwealth. During these political changes Silcock believes that discontinuity in economic growth seems likely to occur because the problems of political and economic development are very different for a self-governing country and a colony run by the British. Unfortunately, in both business and government, a great many Asians have been trained as substitutes for Europeans in jobs designed for Europeans and not for the new jobs required with independence.

No defender of British colonialism, though he points to many special institutions developed to meet the economic needs of a particular state and to the higher incomes in the areas he studies than in neighboring countries in Asia, he recognizes many of the problems of *ad hoc*, unplanned, nonuniform

political decisions for economic growth. He admits that it may have been impractical to have worked out a thoroughgoing integration of Malaya, Singapore and the three British areas in Borneo, but at least the three Borneo territories might have had a uniform fiscal system so that oil-rich, skills-poor Brunei might not have idle surpluses while Sarawak and North Borneo had insufficient funds for needed social and economic programs.

Rubber plays a major role, both in the entrepôt cities and in the areas where the plantations and smallholders collect the latex. Malaya's subsidized replanting program with clones that yield three times as much rubber per acre as the old trees is analyzed in terms not only of the small, less profitable estates it aids but also of those least helped—the big, progressive estates and the Malayan smallholders. Some might want to argue how much smallholders' rubber, particularly in Sarawak, as compared to synthetic rubber, has contributed to the rather greater stability of rubber prices since the second world war.

Silcock urges American social scientists and foundations to engage in more research in this area on such diversified topics as migration, motivation of rural peoples and the systems of capital formation used by the Chinese businessmen. He rightly points out that because Americans do not stay long in underdeveloped areas they must rely on research, more of which is needed to avoid waste in aid programs.

His fourth chapter on Malaya and the Free Trade Area was written for another audience and therefore seems less pertinent and more conjectural. In his final chapter on the Determinants of Economic Development he discusses many policy considerations: the role of taxation in encouraging development, the importance of training in business management and the rapid rate of population increase which requires a country to have much capital formation to maintain present levels, let alone grow. Earlier in the book he stresses that the importance of transport is often underestimated in underdeveloped agricultural countries and that the opening of new land in the region depends on the availability of capital. He warns that planning should not overstrain administrative abilities by too elaborate control techniques.

In a real sense the book reflects the commonwealth approach to development. It contains no detailed policy blueprint but a series of pertinent observations by an economic expert on the area.

EVERETT D. HAWKINS

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*An Economic History of the United States*. By GILBERT C. FITE and JIM E. REESE. Boston: Houghton Mifflin, 1959. Pp. ix, 714. $6.75.

The principal focus of this textbook is on the rise in personal income and standard of living of the American people. The book would seem to be directed toward use in courses at the freshman-sophomore level and particularly where students have had no prior introduction to economics. It has the customary textbook equipment such as an adequate index, an annotated bibliography,

brief summaries and suggestions "for further reading" at the end of each chapter, illustrations and maps, and a great many statistical tables and charts. A student's manual is available which presents a summary of, and review questions for, each chapter of the text. The style is clear and concise and the text is packed with factual detail. Case histories or hypothetical illustrations are presented to clarify the operation of economic principles or relationships.

The authors follow what has become a fairly customary approach in the organization of their book. The material is presented topically within four time periods. Part I, after an introductory chapter on the "Roots of American Economic History," deals with the colonial period through the formation of the constitution. Part II takes the reader through the Civil War. Part III encompasses the period from the Civil War to the first world war; and Part IV, in which the topical approach is abandoned for a more chronological presentation, carries the student through the second world war and concludes with a chapter on the "American Economy at Mid-Century." In order to treat more recent developments in greater detail approximately 60 per cent of the text is devoted to Parts III and IV. Each major section is introduced by a brief discussion of the principal political events of the period.

The book is distinctive for its emphasis upon agriculture and agricultural problems. In the author's words: "Because of agriculture's over-all economic importance in the nineteenth century economy, it has received somewhat more attention than customary. In contrast, when industry began to surpass farming as an income producer, emphasis is shifted to the reasons for, and results of, American industrial predominance." Thus of the six topical chapters in Part II, two (more than 25 per cent of the text) are devoted to agriculture. Similarly in Part III, of nine topical chapters, two (some 20 per cent of the text) concern agriculture and its problems.

It is unfortunate that the very attribute that imparts distinction to the book seems likely to detract from its usefulness. This is likely to be so particularly for those economists who look to economic history not so much to describe the economy of the past as to trace the development of institutions and problems of current interest and to illustrate the operation of economic principles. From this point of view, developments in agriculture in the first half of the nineteenth century, dominant though agriculture then may have been, may be of no greater interest than the emergence of the corporate form in manufacturing, the experiments with government participation in canal construction, the early attempts at labor organization, or the performance by the Second Bank of the functions of a central bank. Shall the criterion for emphasis be importance at the time or significance for today's problems? Most economists are likely to make the latter choice. Nor do the authors keep their promise of shifting emphasis, once agriculture surrenders dominance. For example, in their two chapters on the 1920s, 15 pages are devoted to agriculture while only 24 are left for discussion of the postwar boom and recession, government and business including the tariff, general industrial growth, growth of individual industries, marketing, concentration and consolidation of business, foreign trade and the balance of payments, foreign investment, and transportation. To devote nearly 4 pages to a discussion of the McNary-Haugen bill and the Agricultural Mar-

keting Act but only slightly more than one to the automobile industry and nothing to the capital markets hardly constitutes a de-emphasis of agriculture.

The emphasis upon agriculture militates against a very rigorous approach to the other areas treated, and economists will find the level of economic analysis fairly elementary. The authors seem more intent upon describing the growth of income and living standards than upon analyzing the causes for it. Apart from a footnote adventure in Fisher's equation of exchange there seems to be a reluctance to make explicit any but the most rudimentary economic principles. For this reason the text is not likely to prove useful in courses offered at the advanced level or where students have been exposed to much economic analysis. However, where the course is offered at the freshman-sophomore level and where the instructor prefers a strong emphasis upon agriculture, the book is worthy of serious consideration.

H. JEROME CRANMER

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## Economic Systems; Planning and Reform; Cooperation

*The Economy, Liberty and the State.* By CALVIN B. HOOVER. New York: The Twentieth Century Fund, 1959. Pp. 445. $5.00.

For more than 25 years Calvin B. Hoover has been studying and observing at first hand the operation of different contemporary economic systems. At various intervals, beginning with the New Deal's Agricultural Adjustment Administration, he has served as economic adviser in agencies of the federal government. The present volume, which climaxes his experience in this field, examines the relationship between the different types of economies that have developed since the two world wars: the centrally directed economies of nazi Germany, fascist Italy, and the Soviet Union; the mixed systems of Britain and Western Europe; the economic organization of Yugoslavia and the Russian satellite countries since the second world war; and the transformed capitalism of the United States. Hoover finds a trend in all countries away from traditional *laissez-faire* capitalism and toward either central planning or less direct and nonauthoritarian forms of control. He is concerned not only with the productive performance of the different economic systems, but also with the effect the general expansion of the economic province of government has had upon human liberties.

In a brief chapter on German Nazism and Italian fascism he describes the resemblances and differences between these defunct forms of totalitarianism and the soviet system. He quickly disposes of the Marxists' contention that Nazism and fascism were created by reactionary industrialists and landlords who feared an imminent communist revolution. The lesson to be learned is, he thinks, that the overthrow of capitalism and the suppression of freedom it makes possible need not be brought about, as Marx supposed, by revolutionary engineers acting in the name of a discontented industrial proletariat. It may be brought about by political demagogues who direct their appeals to a respectable lower-middle-class frightened by the prospect of depression and of being reduced to the level of jobless proletarians.

Hardly any one, except Leninist-Marxist adherents, will take exception to Hoover's view that "a socialism which embodies liberty is not to be attained by the Soviet route." A momentary dictatorship is inevitably supplanted by the dictatorship of a party to be followed in turn by an unstable "collective" leadership of a small group and, ultimately, by the authoritarian rule of one man. The minor relaxations of repressive measures and the slight loosening of controls that have followed Khrushchev's de-Stalinization campaign indicate no fundamental change in political principles and methods of the regime. Public criticisms or protests that threatened the intrenched party dictatorship will be put down just as ruthlessly as in Stalin's day. Hoover comes to conclusions that have been stated many times by orthodox economists: that the abolition of private ownership does not necessarily insure greater freedom or equality to the workers, or place political and economic control in the workers' hands, but gives rise instead to a new ruling elite deriving large incomes and special advantages from the management of state property.

In the past it was widely questioned whether a collectivist system could function successfully. "We know now," Hoover states, that such a system "can function over a huge area with a large population at a level of production high enough to maintain a tolerable standard of living." He doubts, however, that the productive superiority of collectivism has been definitely demonstrated by Soviet experience. Thus far the system has been free of the cycle phenomenon characteristic of capitalist economies; but there is no assurance that this will continue to be the case once that country is fully industrialized. He does not believe that the rate of Soviet economic growth has been twice that of capitalist economies during full employment. Discounting Soviet statistics and estimates by some specialists in the field, he places this rate at 6 per cent, and the level of production at 40 to 45 per cent of that of the United States at the present time. He finds that inefficiencies supposedly eliminated by planning have been perpetuated in Soviet industry by its cumbersome bureaucratic management. Khrushchev's radical decentralization program reflects, he thinks, a general dissatisfaction with conditions that had become intolerable. The outcome of Khrushchev's reforms is uncertain. But if the outcome is uncertain, one is inclined to consider as exceedingly optimistic Hoover's forecast that "the present giant industrial combines in the Soviet Union might somehow evolve towards a form more like that of capitalistic corporations and that new small-scale business might be permitted to operate privately." He does add, however, that such a development cannot take place without the emergence of representative government, of which there is as yet no sign.

Hoover is among those economists who hold that the American economy is increasingly monopolistic. He believes, accordingly, that monopolistic trade unions have been able to secure for labor a larger share of the national income than it would otherwise have received. But parts of his discussion lead one to conclude, correctly I think, that the important problem is the distribution of personal incomes rather than the relative shares going to labor and property. And the increase of personal incomes in the lower brackets—in other words, the decline in income inequality—has resulted from causes other than the monopoly power of organized labor.

In Hoover's judgment the increased role of the state in the modified capitalism of England, Western Europe, and the United States has not caused any decrease in personal liberty. General nationalization of industry and hence a complete "statization" of the economy has been avoided in these countries. Governmental controls have been directed towards achieving two ends: (1) full employment and economic stability, and (2) creating and maintaining "countervailing" power (a new term for bilateral monopoly) between large aggregates of economic power—monopolistic corporations in which management is divorced from ownership, and labor organizations practising industry-wide collective bargaining. In the popular mind, Hoover states, the liberties that have been curtailed, for example, by New Deal legislation, are business rather than personal liberties. But as he himself suggests, rather timidly I think, the representatives of the people, when confident of popular support, are quite as ready to encroach upon the civil liberties of individuals as they are to attack business or contractual freedom. Furthermore, when the state takes on the role of creating and equalizing bargaining power between rival economic blocs it is eventually confronted with the choice either of limiting their exactions and demands upon the community by regulating or suppressing them, or of becoming the pawn of the politically stronger side.

Hoover thinks that there is little likelihood at present of the modified capitalist systems of the West being replaced by collectivism by the conscious will of the majority of the electorate. Nevertheless, he concludes, Western capitalism is faced with two serious problems: The first is the vulnerability of the system to extreme inflation and depression. The second, which merits greater consideration than it can be given in this review, is the nonsupport of the system by the intelligentsia.

The book is a comprehensive description and balanced evaluation of the world's leading economic systems. It is well documented and contains an excellent bibliography. It is written with simplicity and should be useful either as a text or as general reading by the public.

ABRAM L. HARRIS

*University of Chicago*

*The Falling Rate of Profit: Marx's Law and Its Significance to Twentieth Century Capitalism.* By JOSEPH M. GILLMAN. New York: Cameron Associates, 1958. Pp. xi, 172. $5.00.

According to Dr. Gillman, "in Marx's theory of capitalist development [the] law of the falling tendency of the rate of profit occupies an especially crucial position," and poses the to-be-or-not-to-be question for the capitalist system. Should the tendency be operative and not checked by countertendencies, the breakdown of the capitalist order would be inevitable. On the other hand, if it be neutralized, or overcompensated by countertendencies, there would be no room for concern about the viability of capitalism, "and the advocates of a socialist solution to the ills of capitalism [would] become but Don Quixote's tilting at windmills." Although the attribution of such a view to Marx is highly questionable, Gillman has set himself the task of testing

the validity of this theory by tracing the behavior of the rate of profit in the U.S. economy in the course of the last hundred years. On the basis of extensive and most interesting statistical studies he concludes that until about the first world war the rate of profit was actually declining, but that thereafter it was either stable or even rising. In Gillman's opinion, this development since the first world war represents but an optical illusion. If unproductive expenditures of business (taxes, selling costs of all kinds, and the like) were to be subtracted from the gross profit numerator or alternatively added to the capital outlay denominator, the rate of profit would be seen to have continued to decline also during the last four decades. The growth of unproductive spending thus becomes in Gillman's view capitalism's principal enemy—causing the decline of the rate of profit and accelerating the breakdown of the system.

This is, indeed, a paradoxical way of looking at the matter, and I doubt that Gillman himself fully realizes its implications. For what follows from it is that U.S. capitalism today would be *better off* in the absence of the unproductive outlays to which Gillman refers. Both the volume and the rate of profit would be higher. Clearly, for prosperity to prevail, these larger profits would have to find sufficient outlets in investment (at home and abroad) and/or in capitalists' consumption lest they be "offset" (and eliminated) by massive depressions and unemployment. Since it cannot be Gillman's view that depression and unemployment would constitute a blessing to the capitalist system, he must believe that investment would respond to higher rates of return and rise automatically to the level required for the maintenance of reasonably high employment.

That this confidence in the elasticity of investment with regard to the rate of profit is hardly compatible with the far-reaching (and growing) monopolization of the U.S. economy, correctly observed and stressed by Gillman, apparently leaves him undisturbed. Nor does he seem to be impressed by the undisguised dismay with which business circles react to any reduction of the government's unproductive expenditures, or by the anguish caused in Wall Street by every outbreak of a "peace scare." Having properly noted the growth and proliferation of unproductive resource utilization and waste under monopoly capitalism, Gillman fails to seize the opportunity offered to him by his own insight. What is in fact a large and increasingly indispensible outlet for the overflowing economic surplus, and a powerful counteracting force to the tendency to underconsumption (and underinvestment) characteristic of monopoly capitalism, appears in Gillman's treatment as a drain on profits and a depressant of income and employment.

This fallacy of ignoring the forces determining the level of effective demand under capitalism and of taking full employment for granted is fatal to Gillman's effort to arrive at a meaningful theory of twentieth century capitalism. While the development of such a theory is, in my opinion, impossible without a systematic application of the tools provided by Marxian thought, Gillman's work can hardly be considered to be a step in the right direction.

PAUL A. BARAN

*Stanford University*

*The Roots of Capitalism.* By JOHN CHAMBERLAIN. Princeton, N.J.: D. Van Nostrand Co., 1959. Pp. xiv, 222. $5.50.

Mr. Chamberlain immediately sets the pace for his book in the introduction by saying that this "is not a scholarly, detached thesis but a spirited defense of the institutions of capitalism." Thus he proceeds to the task he has set himself with the easy style and the carefree approach of a journalist.

According to the book the roots of capitalism can be found in the ideas expressed by Adam Smith and in the political setting spelled out by the American constitution. They take hold in the sympathetic environment of the nineteenth century and they mature under the impact of the political and economic events for which they are, in part, responsible.

Much that was valuable has however been lost in the process, says Chamberlain, and the welfare state we have today is the road to eventual servitude. But, "there are ways to dismantle the welfare state without turning old people out to stony pastures." Some of these ways involve charitable donations and the use of foundations or other semipublic business organizations. Most of them are simply good business practices applied to the necessities of the community. In the "welfare society," which he would like to see instead of our present system, individuals would provide for their own retirement and security through private insurance, employers would accept the principle of a guaranteed annual wage so that unemployment insurance would become unnecessary and the owners of blighted properties would pool their resources to eliminate slums. The farm problem would disappear as marginal farmers left their farms and accepted temporary help from the government while they resettled and trained themselves for occupations more in current demand. These and other changes would not come abruptly but would appear in the course of a gradual withdrawal of governmental activity in every sphere of economic life.

The author arrives at these conclusions without going through a rigorous logical reasoning process; instead he tells anecdotes about the men who sparked the industrial revolution with their inventions and about the men who expanded it. He tells of Watt, Wilkinson, Hargreaves and Arkwright, of Henry Ford, Eli Whitney, Ransom Olds and Frederick Taylor, about Robert Owen, the Fabians and about other attempts in the cooperative movement. Interspersed with these stories are personal interpretations of the views of economists and philosophers.

In many respects this book is a plea for less governmental interference with business and with the economic freedom of the individual. It tries to show that the right to private property, coupled with freedom of contract in a democratic state, can and does provide the matrix for vigorous growth. In many respects it is a departure from Chamberlain's last book, *The American Stakes,* in which he seemed far more willing to see advantages in the application of state power, at least when used to increase the average individual's "social power," i.e., his power to create wealth for himself by applying brain and brawn to raw materials. At that time he thought that the New Deal, on balance, had been beneficial.

*The Roots of Capitalism* is far more conservative; it ignores most of th unpleasant aspects of *laissez-faire* capitalism and it seems to say, almost, tha men and institutions have changed sufficiently so that much is possible no that would have been impossible half a century ago. It ignores cyclical fluctua tions, crises and depressions, and the other unpleasant by-products of ur controlled business freedom. In painting the rosy picture of its ideal, i shows an almost innocent willingness to assume that men will join, unse fishly if necessary, to provide the social needs of society. Or at least that the have found the way to provide for public necessities while pursuing purel personal goals. It is a moot question how, in such a society, taxes and tarif would be treated. There is no mention of a conflict between personal cor sumption desires of the citizens and public investment demands to keep pac with them; nor is there even a hint of the incentives which would be necessar to induce the masses to provide the bulk of the resources for such investmen

Chamberlain is not an economist; it would not be fair to expect an ecc nomically sound thesis from him and he frankly says that this is not his pur pose.As a man skilled in journalism with a good knowledge of history he ha expressed his strongly rooted faith in free enterprise. There are certain face of his argument which certainly have merit and, since his style is pleasan the book makes enjoyable reading even if one cannot help but note the man inaccuracies and contradictions which it contains.

ERWIN RAUSCH

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## Business Fluctuations

*Les fluctuations économiques*. By MENTOR BOUNIATIAN. Paris: R. Pichon e R. Durand-Auzias, 1959. Pp. 133. 1,335 fr.

Professor emeritus Bouniatian here reassembles, with only minor change five articles which originally appeared in the *Revue d'économie politique* an *Revue des sciences économiques,* and which were devoted chiefly to denoun ing the heresies of the new economics. There is no indication that he ha examined any of the voluminous technical discussion of these issues, publishe in the last two decades.[1]

The first chapter, on the illusions of credit, strikes at what he considers th root error: the idea that an expansion of bank credit can increase economi activity. This is an illusion, he feels, because the effects of bank deposits ar not added to those of monetary circulation, but merely replace them. A ris in bank deposits can not, therefore, have the same effect on prices as an ir crease in the quantity of money. "It is the quantity of monetary gold and th public's need for it which, finally, determine the value of money." Ban

[1] The key chapters contain no references to literature published later than the mid-'thirtie though there is one reference to an unpublished book on banking. Later chapters refer t annual reports of the National Credit Council, and Regie Renault, to two (quasipopula estimates of the damages of inflation, and to two postwar books on the history of econom doctrines—but these latter are referred to solely to complain of inadequate recognition o Bouniatian's own contributions.

credit reflects, on the one hand, the amount of money—itself limited by the gold reserve—and, on the other, the economy's objective need for the means of payment, based on the level of economic activity. Bank credit is then a passive, not an active, element in the economy.

The second chapter attacks the notion that public works can increase economic activity in a depression. Actual experience in this field is inconclusive: "The question can be settled only by a theoretical analysis." In the light of theory, it can be seen that the hope of increasing employment by this means is chimerical. Capital used to create public works must be taken from other alternative uses, and hence can add nothing to the total of employment opportunity. Bank deposits can not be used for this purpose, for they too would be displaced from alternative uses; and as previously demonstrated, the creation of additional bank credit can not add to activity.

A third chapter attacks Keynes' theory of employment, particularly for supposing that an excess of intended saving in conjunction with rising liquidity preference and hoarding might depress economic activity. Such hoarding, it is here alleged, has never been empirically established, and is theoretically impossible because "The quantity of money in circulation is determined by the need for means of payment and the normal need for cash. . . . It is evident that if one person increases his cash reserves he can do so only at the expense of the cash reserves of someone else."

The fourth chapter, which is of more recent vintage, attacks French postwar inflationary credit policies, which are attributed in part to the influence of Keynesian ideas. It is denied that an increase in monetary demand can stimulate the economy even when substantial resources are unemployed. Production cannot expand immediately—necessary preparations must first occur, and this takes time (e.g., new plants may have to be built). But an increase in monetary circulation merely raises the prices of the goods which are here *now*—it can not add to the real purchasing power of the community.

The last chapter is devoted to the thesis that Albert Aftalion has appropriated and been given credit for some of Bouniatian's ideas on business cycles. This is a matter about which the reader will, I suspect, feel less concerned than does Bouniatian—though issues of justice and scientific integrity are raised which can hardly be ignored.

If this book has a value it is chiefly as a sort of museum piece. It takes us back, at a bound, to an earlier stage of economic thinking—which, it is sometimes hard to remember, is only a single generation removed, and in some minds lingers on still. It is healthy for younger economists, particularly, to see how today's economic commonplaces looked like dangerous heresies only a quarter of a century ago, and how stubbornly the new insights were resisted by the entrenched orthodoxy of the time. If this demonstration can help us to be even a trifle more openminded and imaginative when we first encounter the disturbing new ideas of tomorrow, then Bouniatian's little book will not have been published in vain.

EMILE BENOIT

*Columbia University*

## Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

*Monetair Evenwicht en Betalingsbalansevenwicht.* By G. A. KESSLER. Leiden: H. E. Stenfert Kroese, 1958. Pp. xiii, 496. Fl. 20.50.

In recent years two new statistical devices have been introduced to improve the analysis of monetary developments. One of them is the flow-of-funds data published by the Federal Reserve; the other is the "liquidity analysis" (to use a term proposed by M. W. Holtrop) of the Netherlands Bank. The man who, as deputy director and long-time chief of research of that Bank, has (together with Holtrop) been responsible for developing the latter device, now takes it as the basis of a thorough analysis of monetary and balance-of-payments equilibrium.

The author starts with a somewhat elementary discussion of monetary equilibrium in a closed economy, introducing the fundamental terms of the system: liquidity deficit and liquidity surplus, and inflationary and deflationary impulses and reactions. He continues with a brief explanation of balance-of-payments equilibrium. In the third and most important part of the book, he deals with the relations between monetary and balance-of-payments equilibrium, including especially the problems of restoring disrupted equilibria. An appendix briefly but completely describes the technique of liquidity accounting used by the Netherlands Bank. A summary, with an English translation, presents the most important conclusions reached by the author, but unfortunately without most of the underlying reasoning and in an order different from that followed in the body of the book.

In the course of his discussions the author touches on virtually every important problem of monetary and balance-of-payments theory and many fundamental questions of monetary and balance-of-payments policy. Particularly useful are the tables showing, for instance: the relations between changes in the supply of and demand for capital and in liquidity conditions (p. 90); the relations between internal, external, and "global" equilibrium conditions (p. 273); the effects of various stabilizing policies on the domestic business cycle and the balance of payments (p. 306); the interrelations between current and capital transactions in two interconnected countries (p. 334); the effects of inflationary and deflationary impulses from abroad under different cyclical conditions (p. 353); and the relations between such impulses and the balances of payments of the countries involved (p. 387).

Conforming to the Netherlands tradition the author stresses liquidity rather than cash balances, including (in addition to money) time deposits and short-term Treasury paper, whose holders can transform them into money without market transactions that would simultaneously reduce the liquidity of another economic unit. The author concludes that a central bank can reduce or increase the liquidity of the economy by its open-market transactions, without changing its net holdings of government securities, merely by selling long-term and buying short-term securities, or vice versa (p. 205). This conclusion should be carefully studied by those economists and politicians who want a central bank to operate with securities of varying maturities in order

to influence interest rate levels without paying attention to the unavoidable effects of such operations on liquidity.

The author freely concedes that liquidity analysis must be supplemented by income-flow analysis since liquidity analysis only reveals changes in liquidity positions without indicating the flows that lead to those changes (e.g., p. 162). The flow-of-funds data of the Federal Reserve seem therefore to be superior to the liquidity analysis data. However, I do not know of any major American work that has made use of the flow-of-funds data; in contrast, every recent annual report of the Netherlands Bank has discussed the problems of the Netherlands internal and external economy on the basis of its liquidity analysis, and now a respectable theoretical work has been built on the same foundation. As long as American economists are unwilling or unable to make similar good use of their own tools, they can hardly deny the championship in this field to their Netherlands brethren.

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*Volkswirtschaftliche Saldenmechanik: Ein Beitrag zur Geldtheorie.* By WOLFGANG STÜTZEL. Tübingen: J. C. B. Mohr (Paul Siebeck), 1958. Pp. viii, 270. DM 26,-.

This book constitutes a greatly expanded version of some ideas that first appeared in the author's article on "Konjunkturtheorie" in the *Enzyklopädisches Lexikon für das Geld-, Bank- und Börsenwesen* (Frankfurt/Main. 1957). The author is concerned with money flows between economic units and with net changes in the money holdings of these units as the result of such flows. While Stützel does not define such units further, they may be households, firms, governments, groups of households or firms, the industries of a Leontief matrix, or the economic atoms of the Walrasian universe. Starting with the rather obvious proposition that money flows between units are in perfect balance if the expenditures of each unit are equal to its receipts, Stützel points out that, even if the volume of money flows should increase or decrease, the money holdings of individual units will not change so long as the receipts and expenditures of each unit change *pari passu*. He holds that the significant variables in monetary analysis are therefore not changes in the volume of money flows, but departures *(Abweichungen)* from the equality between receipts and expenditures of economic units. Stützel considers the thorough investigation of this *Saldenmechanik*, or mechanism of net balances, one of the "most important contributions to a better understanding of monetary and economic phenomena" (p. 50). In this thesis there are of course strong echoes both of the Robertsonian period analysis and of Copeland's explanation of economic fluctuations in terms of money flows, although Stützel does not go so far as to derive a theory of business cycles from his basic framework.

Stützel's book represents primarily variations on his main theme. After discussing problems of aggregation and disaggregation (Ch. 1) and stating his thesis with the help of numerous theorems and postulates (Ch. 2), the author

illustrates his proposition with a condensed flow-of-funds analysis of the German economy in the 1950-56 period (Ch. 3). In Chapter 4, he attempts to apply his ideas to the balance of payments by treating balance-of-payments disequilibria as a special case of "departure," in the broader context of international payments, from the equality between receipts and expenditures. In the process he formulates his own definition of the dollar gap—somewhat in the fashion of Machlup's second concept, relating to the "program balance of payments"—as the current-account deficit resulting from planned expenditures by nondollar-area households and firms that did not appear sufficiently attractive or creditworthy to dollar-area lenders and investors. Finally, in Chapter 5, the author abandons the simpler analysis involving only pairs of transacting units and branches out into a network of money flows that takes into account the credit-creating potential of the banking system.

While the author's basic proposition—if extended sufficiently—bears some traces of cycle theory, the big difference is that Stützel stops short of any inquiry into motives. He fails to inquire why even the "simplest" of his economic units, the individual consumer, might wish to retain part of his money income, i.e., increase his cash balances. Nor does he deal with investor behavior and the making of investment decisions. Secondly, as already mentioned, Stützel does not take the next logical step which would lead him to explore the causal relationship between his "departures" and economic fluctuations, and hence the possible implications of these departures for economic policy. In fact, Stützel's treatment makes for an essentially nondynamic approach which relates to single undefined time periods taken in isolation and his sequence does not appear to be sufficiently complex to serve as a basis for a complete business cycle theory.

JOHN HEIN

*Federal Reserve Bank of New York*

*Money and Banking*. By LELAND J. PRITCHARD. Boston: Houghton Mifflin Co., 1958. Pp. xiv, 770. $6.95.

This is a new addition to the already intractably long list of texts designed to meet the needs of the first course in money and banking. The primary objectives of the book are to give the student a firm grasp of (1) the nature and origin of money, (2) the principles and interrelationships of the monetary and banking system, (3) the relationship of money to prices, production, and employment both in the domestic and international spheres, and (4) the effects of monetary and financial policies on the economy as a whole.

The organization of the volume does not depart in any essential way from the conventional arrangement of the subject matter found in most of the better known texts in the field. It is divided into seven parts. Part I deals with introductory and historical aspects of money. The chapter in this section on "The Money System of the United States" seems to be unfortunately titled since it is mainly descriptive of the various types of currency rather than the complex set of interrelations between the commercial banks, the Federal Reserve and the Treasury which we normally think of as comprising the monetary system.

The nine chapters of Part II are concerned with commercial banking, operation and structure. These chapters are excellently developed, making this probably the most teachable section of the book. Parts III, IV, and V are titled "Banking History and Development, "Central Banking and Monetary Management," and "International Financial Relationships," respectively. The chapters in these sections are standard treatments except for the separation of the discussion of bank failures and the insurance of bank deposits (a chapter in Part III) from banking structure (a chapter in Part II). In another instance, it is hard to see the need for a whole chapter on "The International Gold Standard" (in Part V) after the subject had been discussed in the chapter on "Monetary Standards" (in Part I).

Part VI introduces monetary theory by way of the "value of money" and the "income-expenditure" approaches. The author puts particular emphasis on the complementary character of these ways of looking at the influence of money rather than viewing them as necessarily alternative approaches. The concluding Part VII on "Monetary Policy and Debt Management" is perhaps the most striking departure from the usual treatment of this subject. It is here that Pritchard clearly succeeds in showing the relevance of theory to policy; and his discussion of debt (both public and private) and debt management is more lucid than one is likely to find in most other competitive texts.

Two major criticisms of the text must be made. The first relates to the desirability of forcefully arguing for one side of very controversial monetary issues in an introductory text when there is neither time nor space for fully presenting the case for the other side. Every serious student of monetary economics has a bag of monetary reforms and Pritchard is no exception. His chapter on "Monetary Management and the Banking System: an Appraisal" is devoted mainly to argument for the following reforms (among others): (1) national incorporation of all commercial banks, (2) limiting legal reserves to deposits in the Federal Reserve Banks, (3) extending the Board of Governors' power to fix the reserve ratios of banks within a range of 0 to 100 per cent, (4) eliminating the distinction between demand and time deposits, (5) extending the power of the Board to fix margin requirements to include the commodity markets. It should at least be added that some of these reforms involve such powers as the present Reserve Board would probably be loath to have.

The second criticism pertains to a possible confusion which may arise because of a type of analysis which the author describes as "flow-of-funds." This is a framework, developed by Pritchard, within which various sources and uses of funds may be viewed. However, this framework should not be confused with the flow-of-funds analysis which most students by now have come to identify with the social accounting system developed by the Federal Reserve Board. Pritchard does not acknowledge the existence of the Federal Reserve studies, to say nothing of contrasting his framework with the better known flow-of-funds system.

There are many points throughout the book where the careful reader will pause to dissent (for example, his attack on Keynes, 265-68), but these are for the most part minor. They may well add spice to the text, and be used to

stimulate student interest. A few topics were omitted or inadequately developed, which some teachers of money and banking might like to have had treated in a text. There is no systematic discussion of the money market, and in particular the important government securities and federal funds markets, and the central role of the commercial banks in these markets. Moreover, no account is taken of the growth of nonbank financial intermediaries and the problems they may create for effective credit control by the Federal Reserve. Indeed, Pritchard states emphatically that "while an understanding of our monetary system is impossible without a thorough familiarity with the nature and operations of the commercial and the Reserve banks, this does not apply to the intermediaries either individually or collectively."

Pritchard's text has much to recommend it. It contains and employs many attractive charts, tables and figures, and it makes extensive use of T-accounts to illustrate particularly troublesome points. On the whole, the volume is well conceived and presented, and it should be well received.

EMMETT J. RICE

*Cornell University*

*Money, Credit, and Public Policy*. By LAWRENCE SMITH. Boston: Houghton Mifflin Co., 1959. Pp. ix, 756. $6.75.

This new text on money and banking is long on history and the institutional approach and short on monetary theory and discussion of modern Federal Reserve policy. The traditional topics in money and banking are completely and accurately handled by Professor Smith, but there are woeful inadequacies in his discussion of recent Federal Reserve policy. Also, Smith treats traditional monetary theory topics briefly.

The strong points of this text are clearly the rich detail and loving attention paid monetary and banking history, particularly in the United States. The discussion here moves well and is handled in an objective manner. The strengths and weaknesses of the traditional gold standard, for example, are very well covered. The monetary controversies of the 19th century in the United States come to life under his expert treatment. One might speculate that Smith has a particular affection for this aspect of his subject matter. Certain it is that the historical discussions, of which there are many, are handled with a sure and deft hand. Smith demonstrates his considerable literary ability in these sections particularly well.

Commercial banking is also handled in an adequate manner, though one might wonder why a discussion of branch and unit banking, including particular mention of California banking, omitted any footnote or bibliographical reference to the standard work here by David Alhadeff, *Monopoly and Competition in Banking?* But it is particularly Smith's discussion of central banking that is open to criticism. At the very beginning of his chapter called "The Federal Reserve System" he asserts (p. 499): "A central bank is a bank which holds the reserves of the commercial banks of a country and uses them in the interests of the economy as a whole. To elaborate further, the holding of bank reserves gives to a central bank substantial lending power, a power to lend not

merely an amount equal to its own holding of reserves but a multiple of these reserves." Surely it should be obvious that no bank, including a central bank, can loan a multiple of its liabilities. The commercial bank reserves held by the Federal Reserve, of course, are liabilities and not assets of the Federal Reserve, so the statement as made is incorrect.

The great importance which the Federal Open Market Committee (FOMC) has in determining monetary policy is not recognized in Smith's discussion. Since the Executive Committee of the FOMC was abolished in June 1955, the FOMC has been meeting regularly at least every three weeks in Washington. Smith asserts: "In general, it may be said that the Board determines policy and makes regulations while the Reserve banks carry out the actual process of central banking . . ." (p. 503). In fact, it would be more accurate to say that the FOMC, which *de facto* comprises the Board of Governors and the twelve Reserve Bank presidents, determines monetary policy. Not only open market operations but discount rate changes and changes in reserve requirements are discussed in this key policy group. The most important part of the annual report of the Board of Governors consists of the summary minutes of the FOMC meetings. It is here that flexible monetary policy is fashioned. In order to guarantee speed and flexibility, numerous telephone meetings were held in 1958, for example. In five of these telephone meetings important policy decisions were made.

In discussing open market operations, Smith falls into the common error made by so many money and banking texts when he states: "When the Federal Reserve wishes to tighten credit it sells government bonds . . . The Federal Reserve buys in the open market when it is deemed wise to stimulate the expansion of credit. Bonds purchased . . ." (p. 515). Actually, Smith makes two errors here. One is that, since 1952, the Federal Reserve in its open market operations has worked under a self-imposed "bills only" doctrine, so that government "bonds" are purchased or sold only under conditions involving an extremely disorderly government securities market (e.g., July 1958). Secondly, the Federal Reserve may be purchasing government securities when it is tightening credit and it may be selling government securities when it is actively easing credit. The paradox in this statement is resolved when one keeps in mind that the Federal Reserve engages in regular seasonal operations of a large magnitude wherein it *always* buys government securities prior to Christmas and similarly *always* sells Government securities in January. If it wishes to tighten credit in the fourth quarter of the year, it does *not* sell government securities, but rather purchases somewhat less than the amount normal for that time of the year. Similarly, in a recession period, such as 1957-1958, the Federal Reserve still sells governments in January, but it sells less than the amount normal for that season of the year and in this manner accomplishes credit ease.

In his definition of the money market, Smith uses a broad concept so that what he is referring to is the series of markets in which all loanable funds are exchanged. Many specialists prefer a narrower definition which would restrict the term "money market" to the over-the-counter market, largely concentrated

in New York City, which trades in very-short-term highly liquid assets, such as Treasury bills.

It would be possible to use supplementary readings in the area of the money market and Federal Reserve policy in connection with Smith's text. This text would be particularly suited for those courses in money and banking emphasizing the historical and institutional approach.

JOHN A. COCHRAN

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## Public Finance

*Public Expenditures in Australia.* By B. U. RATCHFORD. Durham: Duke University Press; London: Cambridge University Press. 1959. Pp. xvii, 320. $7.50.

The literature of public finance contains few book-length studies of public expenditure. The techniques of economic analysis have not been applied in this area, and we have few useful generalizations about public consumption. The principles which should govern the use of resources for public as compared with private purposes, as well as their use for one public purpose rather than another, are underdeveloped.

The study being reviewed, restricted to one country, may serve to produce such principles as one building-stone in a large edifice. Certainly public expenditure in Australia has interesting features: government ownership and operation of enterprise is extensive; welfare expenditure, especially in the last two decades, has grown very rapidly; the federal nature of Australia has induced creation of unusual intergovernmental financial devices; local government has been unimportant, and its expenditure amounts only to 6-7 per cent of total public expenditure (compared to 16 per cent in the United States).

When Australia was united into a federation in 1900, its constitution was modeled on that of the United States, the intention being to limit the powers of the Commonwealth government. In fact, this has not happened; instead, the Commonwealth government has grown very strong. Possession of constitutional authority did not furnish the states with the financial and administrative power to cope with the exigencies of two world wars and the great depression. The process by which centralization went forward could hardly have been forecast. Little or no formal transfer of power was made, both because the states were unwilling to yield their position and because voters, when confronted with the chance to amend the constitution, refused to do so. Yet the Commonwealth government acted as if it had power, sometimes being confirmed later through court decision and sometimes going unchallenged. The Commonwealth has taken over taxation of income; it has developed an extensive system of federal grants, of which 70 per cent are unconditioned. Grants received by the states are more than twice as large as state tax collections, and, in the author's opinion, they have promoted political irresponsibility on the part of the states. The generalization is amply supported that Australia "has a more elaborate and complex machinery for main-

taining financial relations between the states and the federal government than any other federal state" (p. 226). Another war or severe depression might bring the end of federalism in Australia.

Ironically enough, the centralization of expenditures has not strengthened the role of expenditure in fiscal policy. In the postwar years, 1949-1952, when inflationary forces were strongest, public expenditure (excluding defense) rose by 91 per cent. The author believes that in Australia "the normal course is for government expenditure to rise during a period of inflation" (p. 263).

While Ratchford is aware that international comparisons are tricky, he offers figures which indicate that, in Australia, government expenditures for health and welfare and for roads and transport, are relatively high (6.7 and 3.5 per cent respectively of GNP, compared to 4.7 and 1.9 per cent for the United States), whereas expenditures for education and for defense are low (1.8 and 3.7 per cent compared to 3.1 and 15.3 per cent). The low figure for defense indicates Australian reliance on its membership in the British Commonwealth; the high figure for roads and transport reflects public ownership. Transportation has probably been the most unfortunate governmental activity in Australia. Rail transport suffers from the existence of three gauges and operation of railways by the states has held back development of highways and actually led to discriminatory taxation of highway use. The transportation system of Australia has been and is inefficient; it has put a drag on economic development.

Ratchford has made a valuable addition to the economic literature on Australia. His appraisal of events is judicious, and he has provided new and extensive statistics to buttress his generalizations.

JAMES A. MAXWELL

*Clark University*

*Federal Budget and Fiscal Policy 1789-1958*. By LEWIS H. KIMMEL. Washington D.C.: The Brookings Institution, 1959. Pp. x, 337. $5.00.

This book would better have been titled "what some people have thought about some aspects of federal budget and fiscal policy," for its concern is with the evolution of the fiscal theory of aggregate revenues, expenditures, and debt. Kimmel documents with extensive quotation the 19th century preoccupation with limited government and economy, the antagonism to deficits and debt in peacetime. The depression of the 1930's paved the way for a great transformation of thinking, but only, as Kimmel clearly shows, after both political parties tried to outdo each other in pledging allegiance to the traditional dogmas in the election of 1932. Franklin Roosevelt, in the early years of the New Deal, remained generally committed to the traditional dislike of deficits, until the recession of 1937-38 and the spread of Keynesian ideas brought fuller acceptance of their use in depressions. Kimmel concludes with a description of the adoption of the Employment Act and its implementation, and contemporary attitudes toward continuous use of fiscal actions for stabilization purposes.

The book has numerous individual sections which are interesting and valuable. President Grant displays a sophisticated recognition of the way a

growing national income reduces the burden of the national debt, and the fiscal insights of Simon Patten are impressive. The financial trauma of the Hoover administration is well presented, with interesting attention to the views of dissenters from the orthodox position. And it is worth the price of admission to read Bernard Baruch's complaints in 1933 about the three previous years of inflation.

But, as a whole, the book suffers from several serious deficiencies. Although Kimmel states at the outset that "an appreciation of the environment in which budget and fiscal theories were developed is essential for their understanding," he has neglected several vital aspects of this problem. Discussion of the fiscal ideas of presidents, Treasury secretaries, and others is not usually related to any description of what the government really did. Kimmel seldom uses Congressional source materials, an omission which leaves a void in the history of ideas. Further, he gives little attention to the political process of fiscal policy-making or to the short-run economic factors which have influenced fiscal policy from year to year. Without these, description of prevailing fiscal ideology lacks perspective, for we do not know how much it affected actual policies. Presidential statements, extensively quoted, too often lack the context needed to interpret them.

Kimmel concludes that "the modern conception is that the only sound fiscal policy is a flexible one. " No doubt public opinion has shifted a lot. But the *practice* of fiscal policy has displayed impressive flexibility in the past. Kimmel ignores pre-1776 experience, and thus neglects the fiscal aspects of American colonial "currency finance" so ably analyzed by Ferguson, Lester, and others. In the 19th century, the federal government incurred deficits in every major depression but one. Despite the prevailing ideological hostility, there were contemporaries who appreciated the economic benefits of these deficits, and plenty of pragmatic politicians who gave lip service to the idea of a balanced budget while avoiding the tax increases or spending cuts needed to balance it. By ignoring these factors, Kimmel exaggerates the shift in actual policy since the 1930's.

Although the style is clear, the selection and arrangement of materials are sometimes puzzling. Topics of major importance are treated superficially, while extensive space is devoted to quoting presidential platitudes or trivia from secondary sources. Only casual reference is given to the theory and practice of wartime finance. The link between fiscal policy and the money supply, which was especially important in 1862-1914, is virtually ignored. Discussion of tax policy after 1932, which is crucial to an appreciation of the confusion of New Deal fiscal thinking, is lacking. The relative space devoted to topics is also puzzling. For the pre-1860 period, six pages are given to internal improvements, only four to war and depression combined. Kimmel spends six pages on the balanced-budget multiplier, although the multiplier concept itself receives only passing reference and no attention is given to empirical studies of the propensity to consume.

Kimmel does a good job on the evolution of academic opinion, although he stresses ideology rather than analysis in premodern thought. His discussion of contemporary thought follows his source materials rather closely. Unaccount-

ably, he ignores the contribution to history or theory of such prominent names as M. S. Kendrick, J. M. Clark, H. H. Villard, D. R. Fusfeld, and S. S. Alexander. He makes no effort to use the historical record to evaluate the effects of fiscal policy or to judge its relative desirability, and his discussion of the political and administrative problems of fiscal policy is skimpy.

This book may prove useful for collateral reading for undergraduate students of public finance. The professors will probably find it disappointing.

PAUL B. TRESCOTT

*Kenyon College*

*Steuerinzidenzlehre—Grundlagen und Probleme.* By HORST CLAUS RECKTENWALD. Berlin: Duncker & Humblot, 1958. Pp. 164. DM. 14,00.

This volume testifies to the international scope of the recent renaissance of interest in the theory of tax incidence. Engendered in part by the existence of large budgets, the driving force is nevertheless a more basic one: this is the need for applying the modern tools of economic analysis, especially of macro-theory, to a difficult problem which heretofore has been treated in none too satisfactory a fashion. Dr. Recktenwald's brief but compact study renders a valuable contribution to this endeavor.

The author approaches his study with a healthy sense of eclecticism. There is no one single methodology that will give the true answer. Deductive as well as empirical work is needed; the analysis of comparative statics must be combined with a dynamic view, and even though much work remains to be done at the general equilibrium level, the partial approach must be retained as an important point of departure. Proceeding accordingly, Recktenwald examines problems drawn from this variety of approaches. His main concern is with the basic issues of incidence theory rather than the detailed analysis of specific types of taxes.

The study begins with methodological considerations, in particular a discussion of how the concepts of incidence and shifting are to be defined, but the author prefers not to commit himself to any one view. A distinction is drawn between (1) immediate incidence and resulting income effects, (2) resulting price changes or shifting, and (3) substitution effects or tax avoidance. This distinction is held useful particularly for partial analysis, it being recognized that it is difficult to distinguish the interplay of these forces in general equilibrium analysis. Here, the author is sympathetic to the reviewer's suggestion that incidence be thought of in terms of the differential effects of various taxes upon the distribution of income in the private sector.

Perhaps the most interesting chapter deals with the general equilibrium aspects of incidence theory. This problem is developed by means of income-expenditure matrices, in which households, firms and government sectors are related to each other. Similar to the reviewer's own approach, the analysis proceeds in two steps, one involving an all-consumption system, and the other an economy with capital formation. The matrix approach is an interesting and suggestive device, but it appears to be used here primarily for presentational purposes.Functional relationships (even in the crude form of Leontief's fixed input-output ratios) are not introduced, so that the application of

input-output analysis to a positive theory of incidence still remains to be done.

There are a few points on which the reviewer would differ. Thus, certain conclusions regarding the effects of progressive taxation seem to involve the rather unrealistic assumption of a marginal rate in excess of 100 per cent (pp. 122, 134) and the same assumption seems to be involved in the rate structure implied in the illustration on page 135. But these are relatively minor matters. In all, Recktenwald has presented us with a valuable and stimulating contribution, making—we hope—for a continued and vigorous discussion of these issues on both sides of the Atlantic.

RICHARD A. MUSGRAVE

*The Johns Hopkins University*

*Nationalbudget und Oeffentlicher Haushalt.* By ULRICH SCHUMACHER. Stuttgart: Gustav Fischer, 1958. Pp. 101. DM. 11, 50 (paper).

This brief and scholarly work, highly concentrated and written in a rather heavy style, will be of interest less for its contents than for the spirit underlying its presentation. The approach is one of detachment, and the author appears unconvinced as to the efficacy of the more elaborate tools of analysis. The contents are largely conventional, in Galbraith's words "conventional wisdom." Analysis of the government budget in macroeconomic terms, i.e. the Keynesian approach, is the basis of the study. In order to integrate the government budget into a market economy, the author makes use of the now familiar National Budget that would create a rational relationship between the two and, moreover, establish "fiscal dynamism"—the latter a rather simplified interpretation of the term with which we usually associate the entrance of the time element into an otherwise static analysis. By then placing the government budget, in all its aspects of size, composition and economic impact, under the tutelage of the National Budget, the author goes a long way toward adopting the "functional finance" approach as presented by A. P. Lerner over 15 years ago. However, he quotes Adolph Wagner as having in the last century called attention to the close connection between fiscal policy and national income, and thereby implies a much earlier origin of this basic idea.

The National Budget, the author then asserts, may aim at either *orientation* or *planning*. Under both assumptions it would anticipate future developments. But in its first use, expressed in general terms it would apply to a market economy; in its second, with precise data it would be the instrument of a regulated economy. There is a world of difference between these connotations. Yet in practice, with combinations of elements of both types employed in various countries, this differentiation becomes more a matter of degree than of principle.

The presentation of the National Budget itself, in accounting terms and using current "input-output" mechanics, econometric model construction equations and variables, makes interesting though finally inconclusive reading. It is, however, just this result that may give the study its particular significance. For many countries—and that includes the United States—have taken up the National Budget idea at some stage in their fiscal procedures without really probing thoroughly into the question of its analytical justifi-

cation. Here the author's approach may be helpful, even if its contribution is more negative than positive. He employs all the instruments of macroeconomic analysis developed in the decades when German economic theory was in a Rip van Winkle sleep; but he remains basically critical. That he then devotes an entire section to a possible application of his national accounting principles to the fiscal activities of the German Federal Republic may seem somewhat inconsistent. But to have expressed his doubts more bluntly than others before him should be a service to economic thinking.

One of his critical views fastens on the value attributed to the use of figures and calculations in general, however reliably obtained. There is no denying the immense aid supplied by statistics and their mathematical exploration in making past and present developments *visible*. But the economic process itself, he states, cannot be understood through figures (p. 40). With data thus put in a more modest perspective, some interesting consequences emerge: if the National Budget with its numerical appearance pictures facts but does not explain the economic implications, this in turn holds true also for the set of data in the government budget and, finally, for the interrelationship between the two accounts. Their presentation in monetary terms is replete with well-known methodological obstacles. But even assuming these problems were solved, the figures would still lack explanatory value (*Erkenntniswert*).

Accordingly, arithmetic totals of such stereotypes (the reviewer's, not the author's expression) as balanced and unbalanced budgets or smaller and arger volume of expenditure and revenue, would become economically less neaningful if not meaningless. As the importance of such aggregates for the nacroeconomic analysis fades, individual transactions on the expenditure as vell as on the revenue side claim the center of attention. Such conclusions vith their stress on itemized inquiry are also reached in fundamental studies n this country, though on different premises (see R. A. Musgrave, *The Theory of Public Finance—A Study in Public Economy*, New York 1959).

Altogether, we are presumably moving away from the habit of using data and their mathematically combined versions as if they were self-explanatory —a sort of Gertrude Stein way of saying that "a budget is a budget is a budget." In view of such progress, other alternatives for analysis mentioned by he author, such as the resort to "metaeconomics," seem less promising. This erm was coined in German postwar economics, apparently in analogy to 'metaphysics" and should indicate the attempt to explain the economic process by factors beyond its own sphere—a rather vague and frustrating idea for he economist.

HEDWIG REINHARDT

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*Readings in the Economics of Taxation.* Edited by RICHARD A. MUSGRAVE and CARL S. SHOUP. Homewood, Ill.: Richard D. Irwin, 1959. Pp. ix, 581. $6.50.

"The decision of the editors was to focus on the economics of taxation. This was influenced by the need to avoid duplicating material covered in other

readings, particularly an earlier volume in the present series, *Readings in Fiscal Policy*, and a volume recently published by the International Economic Association, *Classics in the Theory of Public Finance*, which contains readings on the theory of tax and expenditure determination. Beyond this, the economics of taxation appeared the most appropriate topic for the present series of readings, since it represents the area of public finance in which the major body of analysis has been developed. At the same time, we do not want to suggest that other areas, such as public expenditures, debt, and the budget, as well as the more specialized problems of tax structure, are of lesser importance. On the contrary, we hope that companion volumes in these fields will be made available before long."

This book is highly theoretical. It comprises a selection of leading articles in the field, but in many cases they cannot be understood if one does not have a grasp of mathematics. I am impressed by the variety of conclusions to be found in the various selections. They range from the most conservative to the more liberal. As an example of the former one can cite Edwin Cannan, "Minutes of the Royal Commission on Local Taxation, 1899." He gives four tests to be applied in the consideration of the equity of an alteration in the means of raising or ways of spending public revenue: (a) Will the change make any difference in the distribution of wealth? If not, whether it should be introduced or not is purely a question of production, not of equity. (b) Will the difference in the distribution be in the direction of greater or less equality in distribution? If in the direction of greater inequality, it is inequitable. (c) Will the greater equality be brought about without unfairness as between the various individuals who lose by the change and as between the various individuals who gain by it? If not, it is inequitable. (d) Will the greater equality be brought about in a fair and proportionate manner, stopping short of disappointing what are regarded as the legitimate expectations of the rich?

As an example of the more liberal approach we find that Richard Goode, in his paper "The Income Tax and the Supply of Labor," states that some may work more in an effort to raise their incomes before taxes in order to maintain an accustomed or attain a desired standard of living. His thesis is supported by recent Harvard studies.

John F. Due, in his article "Toward a General Theory of Sales Tax Incidence," disposes of the Brown-Rolph case. He points out that it requires, first, perfectly inelastic factor supplies, and a given quantity of money; second, perfect competition in commodity and factor markets with completely flexible prices and complete mobility; and third, the use of the tax revenue in such a manner that, taking into consideration the effects of both the tax and the use of the tax revenue, aggregate money demand for commodities remains unchanged.

I doubt that Vickrey's proposal, "Averaging of Incomes for Income-Tax Purposes," has a great deal of merit. The brackets are now so wide that in many instances the difference in the tax for most people would not be very great under conditions of normal incidence. For those in the theater or in baseball there might be some merit in it. But for others there is little to be said in favor of such a complicated scheme.

Another article I like is Carl Shoup's "The Incidence of the Corporation Income Tax: Capital Structure and Turnover Rates." He points out that if the tax rate increased from 38 per cent to, say, 88 per cent and if practically everyone expected the new rate to remain in force indefinitely, it seems unlikely that the leading firms in most industries would keep the prices of their products unchanged in face of so drastic a diminution of profits. The tax would provide the occasion for simultaneous, if not concerted, increases in the prices of the products of each of the leading companies, and the smaller concerns would presumably follow. Suppose, on the other hand, that the increase in tax rate was only from 38 per cent to 39 per cent. It seems unlikely that any of the firms would be impelled to give the change more than a passing thought with respect to the price structure for its products.

If management attempts to increase profits before tax in an effort to maintain unchanged the amount of profit available to the common stockholder after taxes, the preceding conclusions apply. The entry of preferred dividends into the analysis does not change the formula. For instance, if preferred dividends equal 62 per cent of taxable income, and if a corporation tax is introduced at the rate of 38 per cent, this tax, amounting to 100 per cent on the common stock profit, can be recouped by an increase of taxable income of 61.3 per cent.

The turnover ratio—the number of times the property used in the business is turned over in sales during the period—becomes the more important, in determining the percentage increase in sales needed to recoup the tax, as less of the operating profit is paid out in interest or rental. Thus, with a turnover ratio of 2, sales must be increased by 1.53 per cent; while with a turnover ratio of only 1, the necessary sales increase, to recoup the tax, is 3.06.

The articles range over the field of equity and welfare. Incidence and incentive effects are also discussed. Included in this area are concepts, methodology and general analysis; also taxes on income and profits, taxes on products and sales, taxes on land and property, and finally incentive effects. A classified bibliography of articles on the economics of taxation is also included.

The articles are well selected. There are few who would deny that the choice has been well made.

PAUL J. STRAYER

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## International Economics

*International Trade and Economic Growth—Studies in Pure Theory.* By HARRY G. JOHNSON. Cambridge: Harvard University Press, 1958. Pp. 204. $4.00.

This book is a collection of eight essays, almost all previously published in journals, organized in three parts: comparative cost theory, the relation of trade to economic growth, and the balance of payments. The accent is heavily theoretical, although policy overtones are present. Given the broad scope and wealth of ideas passing through these 200 pages, the reviewer can do little more than touch on some highlights and must leave out much of interest.

The essay on optimal tariffs under retaliation brings together a number of interesting propositions on this subject: (1) In a two-country, two-commodity world, a country may well place itself in a better trading position with a tariff than under free trade, despite retaliation by its partner. (2) It is not true that successive retaliations will lead to the disappearance of trade altogether; for "some trade is always better than no trade" for each of the countries. More specifically, when one country increases its tariff, its new tariff-modified offer curve is not necessarily less elastic than the old offer curve even though its position is shifted in a trade-reducing direction. Consequently, its trading partner may not find it profitable to raise its own tariff and thus reduce trade further. (3) The position of equilibrium reached after a succession of tariff retaliations may differ, according to which country first increased its tariff. A country does not necessarily come out better by being the first to raise tariffs; the opposite may just as well be true. (4) For a certain class of offer curves (in the form of a power function, and shifting without change in the exponent so that elasticity is unchanged in shifting), the exporter of "luxury" (high price-elasticity) goods will more often than not be in a better position to gain from tariff raises than the exporter of "necessity" (low price-elasticity) goods.

The reviewer has some misgivings about points (2) and (4). As to the former, it seems quite conceivable that for each country each successive offer curve corresponding to a higher tariff might also be less elastic than its predecessor. In that event the process of successive adjustments by each would lead to ever-higher tariffs, which would bring trade closer and closer to zero. It would seem that Johnson has shown the positive-trade outcome to be just as possible as the zero-trade outcome, without having excluded the latter as he believes he has done.

On point (4), Johnson has shown much ingenuity in demonstrating a situation which contradicts our expectations. I believe that his case is even more special than he himself makes it. In brief, his offer curves for one country all have the same elasticities whether shifted by a high or a low tariff. His offer curves shift horizontally or vertically according to the magnitudes of his shift parameters, which reflect the luxury or necessity character of his imports and exports. For different price elasticities and different shift-parameters values he then charts boundary values which separate areas of gain from areas of loss, for each country, resulting from the tariff retaliation game. My objection to this procedure is its implication of complete independence between direction of shift and elasticity of the offer curve. Surely, if a country is willing to buy the same quantity of an import, no matter how high its own tariff on that import, any of the set of offer curves itself will be highly inelastic. His figure 7 (p. 52) does not make it possible to pick out such a case because his elasticity magnitudes begin at unity (owing to the use of logarithms). While Johnson may not yet have given us a usable answer to a complex problem, his approach may at least point a way for future attacks on this question.

The theme of Part II is the relation of growth of output to a country's equilibrium terms of trade, and alternatively to its balance of trade when terms of trade are not permitted to vary. The range of possibilities is quite large, depending on whether the country concerned is industrial or agricultural,

whether growth is due to population growth, to capital accumulation, or to technological change; and whether international mobility of factors is permitted. However, a few points can be mentioned briefly:

1. When countries are incompletely specialized, technical progress in all countries and economic sectors at equal rates will favor the industrial countries and disadvantage the agrarian countries. Output would gain equally in both the industrial and agricultural sectors of each country and thus be "trade-neutral"; while consumption of food would rise more slowly than consumption of manufactures, thus being "anti-trade-biased" in the manufacturing, and "pro-trade-biased" in the agricultural country. The latter must therefore gradually worsen its terms of trade in order to stay in balance. One implication of this is that agricultural countries might find it profitable to push industrialization in order to protect their terms of trade.

2. If capital accumulation were the cause of expansion, the agricultural countries would benefit more strongly than their industrial opposites. Since manufacturing is capital-intensive in comparison to agriculture, manufacturing would expand at constant prices and agriculture would contract. This in turn causes a fall of manufactured prices and a rise of food prices.

3. Population growth will have a different effect on equilibrium terms of trade according as returns in agriculture diminish only slightly or fall sharply. In the former case, the increase in supply of labor relatively to capital will cheapen food and encourage its output at the expense of manufactures in both countries, and the terms of trade will move against the agricultural exporter. With strongly diminishing returns in agriculture, population growth raises the price of food through the growth of demand and moves the terms of trade in favor of the agricultural country.

When one sees the number of possibilities under even the drastic simplifications made here, he may despair of making predictions in any practical situation. The theorist has not yet given the policy-maker a very handy set of tools; but he has shown that rule-of-thumb attacks on international economic problems will often come out with the wrong answers.

Part III deals with elaborations on the transfer problem, both under classical and under Keynesian assumptions. Tariffs and transport costs are introduced into the classical transfer problem, and it is shown that any presumption against the possibility of effecting a unilateral transfer entirely in goods is strengthened (in the usual language, the marginal import propensities of the two countries concerned must add up to a number larger than just unity). The transfer problem is also treated under Keynesian assumptions, and ingenious analogies are drawn between the traditional income transfer and the transfer aspects of exchange-rate adjustment, import restriction, and export subsidy.

This sampling should convince the potential reader of the worthwhileness of these Johnson essays. He will find much highly-skilled argument and will have his stamina tested to the utmost. At times he may question the use of some of the many refinements and qualifications, especially in Chapter V; but this is a minor quibble with the over-all excellence of these essays.

Franz Gehrels

*Indiana University*

*Economic Theory and Western European Integration.* By TIBOR SCITOVSKY. Stanford: Stanford University Press, 1958. Pp. 154. $3.50.

Professor Scitovsky spent the year 1953-54 in research on European integration under contract to the Office of Naval Research. His report, and three more specialized articles which grew out of it, make up this short book. The date of the research and of the report on it, together with the very general level at which the argument is pitched (despite the author's initial intention of collecting statistics), make the volume of little direct use to anyone interested in the economics of the specific form which European economic cooperation has now assumed. But it is the most carefully worked out and seriously thought-provoking statement available of what may be termed the "American analogy" approach to the economic effects of European integration—the view that the main prospective benefits lie in increased internal competition rather than in increased international division of labor, qualified by the reservation that full enjoyment of these benefits requires more positive steps towards integration than the mere removal of trade barriers. As such, the book is especially interesting both for some new developments of theory about the effects of competition, and as an example of the contribution that theory can make to the clarification of complex current issues of economic policy.

Half the book consists of the report on "The Probable Effects of Economic Integration in Western Europe." The analysis is divided into two parts: the effects of economic union on the welfare of the members considered as a self-sufficient group, and its effects on world welfare and the distribution thereof between members and the rest. This division, of course, prejudges the question of main source of benefit; Scitovsky's views on this question are examined below.

In the first part, Scitovsky sees integration as tending to raise average labor productivity in three main ways. The first is through the effects of competition in forcing marginal firms to improve their methods and in encouraging the adoption of mass production; here Scitovsky is particularly interesting on the high-profit-margin policy of European businessmen. The second is through the effects of competition in promoting reallocation of production from the less to the more efficient firms within national industries; here Scitovsky places useful emphasis on the range of costs within the same country, and makes the interesting point that competition may force governments to reduce protection to small inefficient firms in order to strengthen the domestic market base of the larger, more efficient firms. The third is through more efficient investment; Scitovsky lays down very stringent conditions for private investment to be able to reap these benefits, but takes comfort from the fact that much of the relevant investment is suitable for public enterprise which can avoid the difficulties. The first two sources of benefit are regarded as short-run, and the third as long-run, though the treatment is not entirely consistent. This way of breaking up the problem seems to entail some bias towards understatement of possible benefits, since the short-run fixity of capital is held to limit the beneficial effects of increased competition, while the effects on investment are held to be limited by the (clearly short-run) fact that new equipment is only a small part of the total stock. A more helpful way to order the discussion might

have been to distinguish between static effects on the efficiency of resource utilization, and dynamic effects on the rate of growth, separating short-run and long-run effects in each case.

The second part of the analysis, "Economic Relations with the Outside World," begins with a useful exposition in Marshallian terms of what Scitovsky, presumably for emotive rather than descriptive reasons, calls "the orthodox argument" that the gains from a customs union depend on the balance between trade creation and trade diversion. This exposition is defective in some respects—trade diversion is not (as implied p. 55) conditional on changes in marginal costs resulting from increases in intra-union trade, and diversion of both imports and exports should be deducted from the increase in intra-union trade to arrive at the amount of trade creation, thus simplifying the formula on p. 60—and the discussion would have benefited by more explicit recognition of the general equilibrium nature of the problem. The theory is illustrated by a very interesting calculation based on Verdoorn's estimates of the effects of European union on trade and exchange rates, which shows a specialization gain of $74 million and a terms-of-trade gain of $465 million to members (in 1952 dollars), matched by equal losses to the outside world. These negligible figures lead to the conclusion that the gains from competition discussed in the first part are the important ones. This part concludes with a brief but suggestive discussion, following Hicks, of the welfare effects of cost reductions in domestic, exporting, and import-competing industries.

The main conclusion of Scitovsky's report is that the gains from increased international specialization are relatively insignificant as compared with the gains from increased internal competition that would result from European integration. This is a concrete quantitative statement; if valid, it is important both as a statement about the effects of European integration and as a demonstration of the service economic theory can do to economic policy. It is therefore necessary to ask whether Scitovsky is justified in claiming that he has established it. The answer is that he is not. What he has done is to make a careful list of qualitative improvements which more intense competition might bring about, without quantifying them in any way; produce some evidence that the gains from further international specialization are likely to be small relative to national income; and pass a judgment that the unquantified gains will be substantially greater than the quantified gains.

The evidence is itself somewhat unsatisfactory—the fact that the share of intra-European in total O.E.E.C. foreign trade "has only [*sic*] risen from 42 per cent in 1938 to 47 per cent in 1956" does not prove that trade liberalization has had very little effect in increasing intra-European trade, and the Verdoorn calculations are far too aggregative to be acceptable as a basis for a conclusive calculation of benefits—but even if it were much better, acceptance of the conclusion would still depend on confidence in Scitovsky's ability to judge the quantitative importance of a variety of qualitative possibilities. In casting the argument in this form, Scitovsky (who is an exceptionally painstaking theorist) exemplifies one of the serious weaknesses of the economic theorist confronted by a policy problem—the tendency to jump from a subtle

analysis of causal relations to a simple statement of quantitative significance without any intervening stage of calculation.

The remaining essays in the book have all appeared in print, and require only brief comment. "The Theory of the Balance of Payments and the Problem of a Common European Currency" uses the former to establish conditions for the latter to function; the chief requirement Scitovsky finds is an all-European employment and public works policy, carried out by a supranational authority, aimed at stabilizing employment and prices and co-ordinating and financing public works on an all-European basis. This is a tall order, and suggests two questions: why does nobody love floating exchange rates? and why is it always assumed that freer trade will worsen balance-of-payments problems rather than mitigate their causes? This essay finishes with a provocative note on the concept of adequate reserves. "Economies of Scale, Competition, and European Integration," reprinted from this *Review*, breaks new ground in relating decisions about quality of equipment to nature and growth of markets, though it is not certain that stagnation (as contrasted with lag behind the United States) is the European characteristic needing explanation. "The Doctrine of Comparative Advantage and the European Coal and Steel Community" is a rather clumsy attempt to deal with the problem of integration by sectors, which nevertheless makes some worth-while theoretical points.

HARRY G. JOHNSON

*University of Manchester*

*Common Sense About the Common Market: Germany and Britain in Post-War Europe*. By E. STRAUSS. New York: Rinehart and Co., 1958. Pp. 164. $3.50.

The heroine of the French play, *Maya*, was all things to all men. Although the Common Market may not be all things to all international trade students and statesmen, to E. Strauss, head of the Economics and Intelligence Department of the British Board of Trade, it is an instrument of power politics promoted by West Germany to assist in its efforts to dominate Europe. The postulate upon which this conclusion, in part, apparently rests is that under conditions of free trade, the strongest nation eventually dominates its weaker partners. The Common Market is a free-trade area; West Germany is Europe's strongest nation; therefore, under the Common Market, West Germany will eventually dominate the continent.

This conclusion is supported by the author's historical interpretation of the *Zollverein*—Prussia's effort to dominate Germany, as well as by *Mitteleuropa*, Friedrich Nauman's plan for the economic union between Germany and Austria and the abortive *Anschluss* between Austria and Germany—both efforts by Germany to regain the position which she held before the first world war.

The work starts with an exposition of free trade, multilateralism and the Common Market principle, goes on, in the second chapter, to trace the history of the German Common Market idea and to show its role in power politics. The third chapter sketches, in a brief but meaty exposition, Germany's economic reassertion showing the forces and events which German statesmen

were able to utilize to attain their goals. A chapter on Germany's place in Europe which discusses the question of the Ruhr and outlines the European Coal and Steel Community, completes the background for the author's development of his thesis.

The history of the negotiations which led to the elaboration of the Treaty of Rome is followed by an analysis of the Treaty itself; its principles, organization, policy, institutions and general dispositions. The author's main contention is developed in a chapter dealing with the "Treaty and the Future" showing the economic implications of the Common Market and the choices facing "Europe at the Cross Roads."

Strauss does not feel that the Common Market is the best solution of Europe's economic problems and suggests that, like its predecessor customs unions, it will not prove viable. As an alternative, the author, after explaining Britain's reluctance to join the Common Market but willingness to join the Free Trade Area under certain conditions, suggests an extension of the imperial-preference tariff system to most of the countries of Europe. A preference tariff system would promote trade without exposing weaker nations to the corrosive forces of free competition.

This reviewer feels that the economic effects of the Common Market cannot be determined on the basis of the summary, albeit often profound and sophisticated, analysis which the author provides. Analysis of the type which Tibor Scitovsky gives in his recent *Economic Theory and Western European Integration* furnishes a more solid basis for forecasting the future of the Common Market.

Finally, we should note that the Common Market is but one group of institutions of the European Economic Community which includes many other programs looking towards the integration of Europe. Although the Community may have many shortcomings, notably its neglect of labor and business-cycle policies, as Strauss points out, it is an important step forward in man's efforts to improve his lot.

Its future cannot easily be predicted at present, for much will depend on the foresight and statesmanship of its government. The creators of the European Economic Community have already given ample proof of their intelligence, statesmanship and ability to compromise. If they can continue along this ascending path, Europe's and the free world's future may well be considerably brighter.

MAX J. WASSERMAN

*University of Kentucky*

*Tariffs, Markets and Economic Progress.* By RONALD B. MACPHERSON. Toronto: Copp Clark Publishing Co., 1958. Pp. xi, 91. $3.50.

In the literature on international trade a distinction is made between arguments for tariffs which are obvious nonsense and those which are more sophisticated; in contrast with most tariff arguments emanating from the business community, this book falls into the sophisticated category. It is particularly interesting because MacPherson tries to meet and slay the economist on the latter's own ground and with the economist's apparatus. The main thesis is

"that a young nation, such as Canada, may maximize economic welfare by employing tariffs to direct productive resources into more highly-productive industries, and thereby (a) foster the expansion in markets needed to permit the economies of large-scale production and/or (b) increase purchasing power sufficiently to enable consumers to pay higher prices and still increase living standards." The book is mainly a development of this thesis, together with an attempt to establish a rule for finding the appropriate candidates for more and less tariff protection.

The main underpinning to MacPherson's thesis is the assumption of decreasing costs in industrial employment such as the chemical, electronic and automobile industries. The decreasing cost idea is used in much the same way as it was by F. D. Graham in his famous 1923 article on protection. Consider a small young country, such as Canada, producing some industrial products but with a comparative advantage and specialization in, and export of, primary products. Treat the industrial products as subject to decreasing costs provided that a high enough rate of production can be obtained, while the primary products are produced under increasing cost conditions. Impose a tariff on the import of industrial products. It is alleged that an advance in technology will be applied in the industrial production. (The other, more advanced, country in the two-country model is assumed to be already using advanced large-scale technology, and therefore production conditions do not change or change relatively less there.) The encouragement of industrial production will raise real income (even if measured in terms of the protected good); this result depends on the assumption that a rather large decrease in costs is brought about by the expansion of industrial production due to the tariff, a decrease which would not otherwise have taken place. As did Graham, MacPherson at some points assumes that modern technology consists of large indivisible units, so that if production of a decreasing cost industry were subsequently curtailed, diseconomies would arise.

While MacPherson criticises the economists' discussions of tariffs, he gives little evidence of a careful reading of that literature, particularly of the discussions of the decreasing cost case by Graham, F. H. Knight, Jacob Viner, Gottfried Haberler and J. H. Williams. If he had understood that literature the book would not have been characterized by such sloppy analysis. At times MacPherson's decreasing costs appear to be a historically generated phenomenon, and at other times they are a characteristic of an existing technology. No distinction is made between internal and external economies. As Haberler has shown, if the decreasing costs are internal to the firm and industry, then they would be completely realized or else monopoly would result, and all of this irrespective of the tariff; monopoly conditions rather than the absence of protection would be responsible for unexploited economies. If the economies are external to the industry, then MacPherson's case is simply an infant-industry or an infant-economy case for tariffs, and the practical empirical questions must be answered as to the locus and importance of such economies and the extent to which tariffs are the best way to improve economic welfare in such a situation. Nowhere does the book tackle any of these practical questions.

MacPherson also devises a rule to ascertain the maximum tariff protection to be given to any industry. The tariff will shift resources to the protected (A) industry, from other (B) industries. If this is a sensible shift in MacPherson's terms, there will be an increase in "the purchasing power" of the country raising the tariff; in the context, this means real national income. The increase in purchasing power is defined (subject to two small modifications) as the "value added" by 1 unit of production in industry A minus the "value added" which is lost by the diversion of resources from their previous use, B. In MacPherson's terms, the efficiency of industries is measured by the value added per man per unit of time; thus the increase in purchasing power will only be a positive number if the average net product per man-hour is greater in the A industries than in the B industries. The maximum permissible tariff rate (if the tariff is not to be detrimental to the country) is the ratio of the increase in purchasing power to the (duty-free) cost of the competitive imports. Any increase in tariffs up to this maximum is said to be beneficial to the Canadian economy.

The search for a "scientific" approach to tariffs has been a long one and it would be helpful if such a formula were an appropriate guide either to the level of a nation's tariffs or to the structuring of a tariff. Unfortunately this is not so. The formula would be complete nonsense if the selling price of the A products was simply the duty-paid landed price of imports of competing products, for then the value added in the A industry would depend on the amount of the tariff and could not be used to determine the appropriate tariff. This is the old fallacy of tariffs to equalize costs of production but it is not the error which MacPherson usually makes; he treats the price of the B products which are given protection as mainly determined by internal demand and supply considerations, but this is true only when tariffs are prohibitive.

Even if one had the information which the author introduces symbolically into his formula, would it give the correct indications of the appropriate height or structure of a tariff? In general, industries which are expected to give high value added per man would get tariff protection; the higher the value added per man, the higher the tariff protection; and in each case the maximum appropriate tariff is a prohibitive one. However, a higher than average value added in an industry may be due to a number of influences: a high capital intensity of production, an intensive use of natural resources, monopoly conditions, and special experience in the development and the use of technology. If the higher than average value added per man-hour is due to high capital intensity, it is inappropriate to direct resources into such an indutry as if capital were costless. Similarly it is nonsense to encourage monopoly. The connection between more tariff protection and the development and use of technological knowledge is by no means clear. To sum up, the formula is more of a hindrance than a help to the structuring of the tariff.

The book is annoying and misleading on many minor points.

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## Industrial Organization; Government and Business; Industry Studies

*Market Power: Size and Shape Under the Sherman Act.* By G. E. HALE and R. D. HALE. Boston: Little, Brown and Co., 1958. Pp. xix, 522. $17.50.

This volume, third in editor S. C. Oppenheim's "Trade Regulation Series,"[1] has among its purposes "a study of the law of market power prevailing in the United States in the middle of the twentieth century" (p. 3), the discovery of "an underlying rationale for the law's attitude toward monopoly" (p. 10), and the introduction of practicing antitrust attorneys "to the outlines of economic thinking about monopoly" (p. 14). In pursuit of these purposes the authors successively examine the several theories concerning "horizontal size" (a somewhat misleading term by which they mean the *proportion* of a specific commodity or service supplied by a single firm), the problems raised by vertical integration, product diversification, geographic dispersion, "wealth" (absolute size), and attempts to monopolize. These examinations are followed by a survey of remedies afforded litigants in antitrust proceedings, and two interesting appendices.[2]

There can be little doubt that this volume provides a most comprehensive and systematic survey of both antitrust case materials and legal-economic comments thereon. The footnote materials alone are worth, to scholars in this area, the not insubstantial price of admission. Nor can one fail to admire the ability of the authors to integrate the legal and economic aspects of the myriad policy issues in this field.

Having thus saluted both the authors' scholarship and their analytic prowess, the reviewer finds it awkward to express doubt about the soundness of the conclusions set forth. However, a possible explanation lies in the authors' statement that their conclusion "makes no pretension to the rationality which we have sought to infuse into the analysis of preceding chapters" (p. 404). Briefly, the authors favor abandonment of Section 2 (but not Section 1) of the Sherman Act and enactment of legislation limiting the over-all size of business enterprise. By thus abandoning what they characterize as efforts to limit monopoly, and replacing them with efforts to limit "pure size," the authors declare that several advantages would be gained. Succinctly, these are: (1) "reduce the public prejudice against a free-enterprise system and make possible the reduction of intervention and its burdens"; (2) reduce the role played by "partisan politics"; and (3) "bring into the open the criterion which courts are actually applying in the guise of enforcing the antitrust laws" (p. 404). The authors are not unaware of the possible diseconomies which may result from their suggestion. However, they feel that "on balance an over-all limitation upon size of business enterprise would afford a greater degree of freedom and opportunity to our citizens at a lower cost in terms of living standards than can be achieved under existing legislation." Rejection of their

[1] The earlier volumes were G. P. Lamb and S. S. Kittelle, *Trade Association Law and Practice*, Boston 1956, and W. L. Fugate, *Foreign Commerce and the Antitrust Laws*, Boston 1958.

[2] "Market Imperfections: Enforcement of the Antitrust Laws in a Friction-Afflicted Economy," and "Monopoly in Motion: Dynamic Economics in Antitrust Enforcement."

proposals would leave us, in the authors' view, with a choice between public-utility-type regulation and continuation of our efforts, under existing statutes, to destroy monopoly. As between these alternatives, the Hales prefer the latter to "the rigidity and [sic] *veniality* of an interventionist state" (p. 406).

One can agree with the authors' willingness to gamble on diseconomies—it is by now the accepted view that the goal of Section 2 of the Sherman Act is to preserve a competitive system in the hope of realizing "such social and political ideals as the diffusion of private power and maximum opportunities for individual self-expression"[3]—and remain in disagreement with their conclusions. First, the view that "rational limitation upon monopoly is impossible," set forth (p. 404) as being "the whole argument of this book," is one which can be supported only by the kind of industry-by-industry analysis of the impact of antitrust decisions recently attempted by Whitney.[4] In the reviewer's judgment, no analysis of the legal case materials in this field can support such pessimism. Second, both the desirability and the possibility of removing the size and monopoly problems from the arena of partisan politics are to be doubted. Government (including judicial) policy vis-à-vis the corporate sector is and must be, at bottom, based on an assessment of prevailing public will. The "pro-business" decisions arrived at by the Supreme Court in Section 2 cases in the 1920's were not based on any novel judicial interpretations, but on an assessment of the facts of business life in terms of prevailing public attitudes. The "size is no offense" doctrine, credited with saving U. S. Steel from a Section 2 conviction in the 1920's, had not prevented the Court from recognizing public antipathy to the oil and tobacco trusts in 1911. It was broad satisfaction with the functioning of big business which permitted a majority of the justices to wink at business tactics which an earlier generation had found, and a later generation would again find, reprehensible.

Finally, the authors' out-of-hand rejection of an extension of government regulation and administration to industries now privately regulated and administered represents a lapse from the critical approach characterizing the greater part of the book. If the weaknesses and pitfalls of utility regulation are legion, so too are the dangers of privately administered prices. Might not the "rigidity of a politically responsible regulatory body" be preferable to that of what Berle might term a nonstatist civil service,[5] comprised of managers answerable, at best, only remotely to their stockholders? Or might there not be some middle ground, some "Kefauver-method" of inducing possessors of substantial market power to exercise a form of self-restraint? It is questions such as these that the authors may have had in mind when they closed "with the customary recommendation for further study."

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[3] J. B. Dirlam and A. E. Kahn, *Fair Competition: The Law and Economics of Antitrust Policy*, Ithaca 1954, p. 17. Cf. H. B. Thorelli, *The Federal Antitrust Policy: Origination of an American Tradition*, Baltimore 1955.

[4] S. N. Whitney, *Antitrust Policies: American Experience in Twenty Industries*, New York 1958; but cf. J. W. Markham, "United States Antitrust Policies: How Effective Have They Been?," *South. Econ. Jour.*, July 1959, *36*, 58 ff.

[5] A. A. Berle, *Power Without Property: A New Development in American Political Economy*. New York 1959, pp. 8 and *passim*.

*The Fertilizer Industry: Study of an Imperfect Market.* By JESSE W. MARKHAM. Nashville: Vanderbilt University Press, 1958. Pp. 249. $6.00.

> The preparation of a detailed industry study is a tedious, painful, and often dull process. At best it affords an opportunity to apply broad principles, never to develop them. At worst it can be an outlet for the urge to describe (p. 9).

So begins Professor Markham's preface. His book is a characteristically expert performance, applying the "broad principles" with good judgment and copious statistical documentation. It is also not very exciting, for the "best" rather than the "worst" reason set forth above. I am not convinced that the industry study can aspire no higher.

Appraisal of the fertilizer industry, Markham found, required analysis of two quite distinct aspects—business organization and policy on the one hand and the rationality of consumer choice on the other. Each has been defective. As for the first, the supply of all the major component materials—phosphate rock, sulphur, superphosphates, fixed nitrogen and potash—has been characterized by a high degree of concentration and collusion, domestically and internationally. The result in all cases has been noncompetitive pricing and monopoly profits; drawing heavily on Federal Trade Commission reports, the book tells this story fully and convincingly.

The analysis of other aspects and consequences of the industry's market structure is a good deal less satisfactory. For example, Markham emphasizes several times the vertical integration between phosphate mining, superphosphate manufacture and fertilizer mixing and distribution (in sharp contrast with the production of the other fertilizer materials); but nowhere explains this phenomenon. And I found some of his analysis of its consequences confusing: here and elsewhere the book shows signs of hasty writing. He seems at times to place misguided emphasis on the fictional price at which integrated companies transfer phosphates to their own fertilizer departments (p. 128). He suggests that integration permits these companies to engage in a kind of tie-in selling, but how exactly, and what product is tied to what is never clearly and convincingly spelled out.

Again, Markham contrasts the sharp reduction in costs per unit of nutrient made possible by innovation in the concentrated materials-producing industries with the static record of the more nearly purely competitive fertilizer mixers and distributors (pp. 165-67). In view of the widespread acceptance of the Schumpeterian view that this contrast seems indirectly to support, one wishes he had made a more searching and explicit inquiry into this aspect of the industry's performance. The chapters on sulphur and potash disclose no evidence whatever that monopoly was conducive to innovation. The only references to the subject in the discussion of superphosphates mention that the producers have probably retarded progress, and describe the outstanding innovational and promotional contributions of the Bureau of Soils and T.V.A. Government research and encouragement played an important role also in the development of synthetic nitrogen compounds.

Since the second world war, the prospects for competition in these markets have improved, in part as a result of the vigorous antitrust prosecutions of the 1940's. Unfortunately, since most of his crucial statistics stop in 1951, Mark-

ham is unable to shed much light on the adequacy of these changes, although he evidently regards residual concentration in most of these industries (with the spectacular exception of nitrogen) as both excessive and irremediable under the antitrust laws as presently interpreted.

The author's strongest criticisms and proposed remedies are directed against what he demonstrates—convincingly if his agronomic assertions are correct—to be the most serious defect in the industry's performance, the failure of buyers to demand the more economical concentrated fertilizers. The analysis of this phenomenon is the most interesting and original part of the book. The efforts of government in this area are interestingly described and appraised. They come in for a great deal of apparently justifiable criticism, although one wishes in certain instances the author had explained why these agencies have persisted in error.

One wonders, too, whether Markham is justified in so completely exonerating the structure and policies of the industry of any responsibility for the stubborn irrationality of consumer behavior. His demonstration that the fertilizers sold by cooperatives and profit-making companies are not greatly different in composition does indeed strongly suggest that it is the buyer, not the seller, who is mainly at fault. Still one wonders whether even individual producers of, say, concentrated superphosphates, who are big and few, or all of them together—in an industry that has shown considerable assiduity in collaborating for less laudable objectives—might not be held to account for not doing a better selling job. When cooperatives and governments are so ineffective, however, it is hard to blame private industry—unless one is prepared to have the government take over fertilizer mixing and distribution and give farmers what is good for them whether they like it or not.

ALFRED E. KAHN

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*Airline Price Policy—A Study of Domestic Airline Passenger Fares.* By P. W. CHERINGTON. Boston: Division of Research, Harvard Graduate School of Business Administration, 1958. Pp. xx, 471. $7.50.

In this book, the author presents his findings from a study of factors which influence managements of domestic trunkline carriers in the determination of passenger fares.

The basic information was derived from interviews with responsible airline officials and members of the Civil Aeronautics Board and its staff, and from special economic studies and case decisions dealing with airline passenger fares. The author supports his conclusions by frequent references to leading case decisions and rulings of the Board. Summaries of other pertinent case decisions are given in appendices to some of the chapters. In addition, reliance is placed on historical information to assist in correlating basic changes in airline passenger-fare policies wth changes in economic and political circumstances.

The material is organized in a logical sequence. Following the general introduction, consideration is given first to the cost and market environment in which airline passenger-fare policies are formulated. The discussion is then

focused on the small and large segments of the airline travel market, with special attention given to air coach travel. This is followed by a chapter dealing with passenger fares designed to meet competition and another chapter dealing with fares designed to increase passenger revenues of the carriers.

In the past, business and professional travel has been the mainstay of the passenger traffic of the domestic trunkline carriers. It is the author's position that this segment of the first class travel market will not continue to provide sufficient volumes of traffic to fully support the anticipated growth of commercial air passenger traffic in the United States. It is therefore from the air coach and other first class segments of the airline travel market that the carriers must derive their major support for their rapidly expanding passenger traffic. In stressing the potential of air coach travel, the author does not neglect basic problems associated with this type of traffic, especially those related to its peak characteristics.

The author concludes that there does not exist a separate body of airline passenger-fare policy. Consequently passenger fares have been set largely on an *ad hoc* basis. In this connection, attention is directed to the lack of systematic and scientific cost and market analyses as basis for the formulation of sound passenger fare policies. Managements rely too frequently on the "feel" of the market to identify particular segments of the airline travel market and to judge the relationships of supply and demand in these segments. Furthermore, top-executives give too little attention to these policies. This attitude toward airline pricing policies has developed in spite of the fact that the Board has in general left the initiative in these matters to the managements of the carriers.

Criticism is raised as to the rôle of the Board in setting passenger fares. The author found that its actions were confined largely to situations where there were conflicts between competing carriers or where there existed no discernible pricing policies. This short-run approach is viewed as outdated and inadequate. Because of the increasing need for improved communication between the carriers and the Board and its staff, the suggestion is made that the Board should sponsor round-table discussions of basic industry problems, involving members of the Board and its staff and representatives of the managements of the airline companies. Another suggestion is that the Board should add to its staff an analyst who is thoroughly familiar with the over-all situation in the airline travel market.

The book is a major addition to the literature on airline management. It should prove valuable to the managements of the airline companies in pointing out some of the significant shortcomings of airline passenger-fare policies, and the need for continuous cost analyses and market research in this area. The book sets forth suggestions which merit serious consideration by regulatory authorities. In addition, it provides an incentve for students of economics and business management to undertake research with respect to the pricing policies of airline passenger travel under accelerated conditions of growth which characterize this industry.

J. C. D. BLAINE

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*The Motor Industry.* By GEORGE MAXCY and AUBREY SILBERSTON. New York: Macmillan Co.; London: Allen & Unwin, 1959. Pp. 245. $5.75.

It is a somewhat surprising fact that there exists a paucity of industrial and economic analyses of the motor car industries of the western economies, in view of the importance of these industries in these nations. The present study, one of the series of Cambridge Studies in Industry, serves to fill this gap very well for the automobile industry of Britain.

It contains chapters on history, the structure of the industry, demand, production, costs, economies of scale, competition, profits, capital sources and future prospects. There are also useful appendices on such matters as capital-output ratios, comparative productivity and prices in the British and American automobile industries, and the relative prices of British and other European cars.

We learn that economies of scale differ in several of the operations of the ndustry. The economy of large-scale production most associated with automobile manufacture in the public mind is that of final assembly on a series of moving conveyer lines. This is not the operation, however, which deternines the optimum scale of output. On the basis of optimum scales of approxmately 100,000 units per year in final assembly plants, of 100,000 units for notor block foundry castings per plant year, and of 400,000 to 500,000 engines per line for machining and final motor assembly operations thereon, we are gradually led to the demonstration that the press shop in which the larger body panels and shapes are stamped is the key operation which determines he over-all optimum scale level—in the region of 1,000,000 units per year. Put in terms of the long-period cost curve, unit-costs decrease rapidly in all lepartments as volume increases to 100,000 units per annum. Beyond this point economies continue to be realized for machining and pressing as volume grows, but beyond the half-million mark economies of scale gradually taper off as major pressings remain the principal technical source of such savings. Beyond the million mark economies of scale from technical sources appear o cease. Such optima change slowly with time under the impact of such influences as automation.

No British automobile manufacturer has yet reached the optimum over-all conomic level of output. Only three firms in the world—General Motors, Ford and Chrysler—have arrived at such levels although the Volkswagen firm nay reach it soon. (Ford Motor Company probably achieved it as early as 917, with the production of 834,663 units, assuming that the optimum level was 20 per cent lower in 1917 than in 1957.) We may anticipate, therefore, hat British firms may continue to expand under conditions of decreasing costs while the "Big Three" American firms have few remaining technical economies of scale which remain unexploited.

The British industry had reached a stage of oligopoly by 1955 when the Big Five" (British Motors, Ford, Rootes, Standard and Vauxhall) accounted or over 95 per cent of the industry's car and commercial vehicle output compared with the 88 per cent produced by the "Big Six" in 1947. Oligopoly of he "Big Three" in the American industry has accounted for 90 per cent or

more of American car production almost continuously since the late 'twenties. The merger process which was recognizable in the United States as early as 1908 when W. C. Durant formed General Motors Corporation was significant in Britain by 1929. It has continued in Britain in the postwar years, featuring such horizontal mergers as that of the two largest firms (Austin and Nuffield) to form a single firm (British Motor Corporation) which has produced about 40 per cent of the output of the entire industry through 1955.

Besides the "Big Five," 10 or 12 "specialists" such as Jaguar, Rover and Rolls Royce companies, occupy about 5 per cent of the market with an additional 15 "makes" of cars, and some 20 other firms produce commercial vehicles, many to special order.

There is little price competition in the British motor industry—"quality" competition is the rule, but major model changes are considerably less frequent than in America. The acceleration of automobile obsolescence which characterizes American market practices is less necessary in Britain—the British producers depend less upon the replacement market for new car sales than do their American counterparts. The British industry appears to be less "mature" than that of the United States.

The authors have employed such professional tools as market, price and statistical analysis to provide an important addition to our list of industry studies. Theirs is probably the best economic study of a nation's motor vehicle industry to appear on either side of the Atlantic.

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*Economic Regulation of the Motor Trucking Industry by the States.* By DONALD V. HARPER. Urbana: University of Illinois Press, 1959. Pp. vii, 320. $5.00; paper $4.00.

Since the passage of the Motor Carrier Act in 1935 subjecting interstate motor carriers to federal regulation, state regulation of motor carriers has received very little serious study. The competitive impact of interstate motor carriers on other modes of transportation, and the reexamination of the role of federal regulation have focused attention on natonal aspects rather than state. The difficulties inherent in an analytical study of the widely divergent state requirements which, as the author has pointed out, range from no requirements to very comprehensive ones, have also been deterring factors to a long-needed undertaking of this nature.

This thorough study describes current economic regulation of the motor trucking industry by the states and is a valuable contribution to transportation and regulatory literature. A general examination is made of the regulatory policies in all the states while an intensive study is made of the specific requirements of seven states—California, Illinois, New York, Ohio, Pennsylvania, Washington, and Wisconsin. In certain instances state regulation is compared to federal regulation. The reader secures a fundamental understanding of the essential characteristics of the motor trucking industry through an initial description of the types of carriers, scope of operations, and economic characteristics of the industry. The development of state regulation

is traced with a discussion of the reasons underlying the enactment of state regulatory acts and the legal basis for state regulation.

The factors considered in the selection of the seven states comprising the core group are delineated. The amount of regulatory experience of a state was one of the factors. States were chosen because of differences in state regulation as well as similarities. The amount of trucking in a state, particularly the volume of for-hire trucking, was another element considered as was the availability of information which, in many states, was inadequate. Finally, in general, states were paired in order to allow comparisons with neighboring states. The states chosen appear representative and provide sharp contrasts, which brings out one of the characteristics of state regulation.

The basic statute of each of the core-group states and the respective regulatory body administering the act are examined. A thorough analysis then follows of the three broad types of regulation of for-hire motor carriers: control over entry; regulation of rates; and regulation of service. Methods of regulatory enforcement are also covered. The book is well documented and includes a good bibliography.

Economists find in state regulation of motor truck transportation, as in federal regulation, the extremes in regulation. On the one hand, the free play of economic forces is permitted to influence rates and service of one group of for-hire carriers which have been exempted from regulation, whereas other for-hire carriers are comprehensively regulated as to entry into service, rates, and service. Even within the regulated for-hire carriers, differences in the extent of regulation exist between the classes of carriers. Regular-route common carriers in California, for example, must prove public convenience and necessity. Control over entry is strict for this class of carriers although little or no control is exercised over entry of irregular-route common carriers and contract carriers. These groups, with virtually no entry restrictions except payment of a nominal fee, have in numbers and volume dominated California trucking. On the other hand, the turnover rate among this group of operators has been very high.

One of the significant contributions made by this study is the comparison of regulatory aspects which shows relative uniformity on some phases and lack of uniformity on others. The author suggests that state legislatures and commissions should evaluate the effects of regulation upon carriers and upon the economy of a state. Such investigations would result, in the author's opinion, in a redefinition of the objectives of regulation. This is a timely suggestion. Both the economic environment and the trucking industry have changed considerably since the enactment of many of our state regulatory acts. It is pointed out that the use of a "grandfather" clause in new motor carrier regulatory acts, blanketing in, as it does, all of the carriers operating prior to passage of the new act, can result in overcapacity in the future. Furthermore, clauses of this kind permit continuation of practices prohibited under new legislation, thus negating much of the effectiveness of newly enacted statutes.

The author feels that the advantages of state regulation outweigh the dis-

advantages. Regulation generally improves carriers' financial position through restraint of destructive competition and maintenance of a reasonable level of rates. Shippers too have benefited inasmuch as regulation has tended to improve service, equipment, and facilities and to eliminate discriminatory practices.

CHARLES A. TAFF

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*Railroad Transportation and Public Policy.* By JAMES C. NELSON. Washington: Brookings Institution, 1959. Pp. xiii, 512. $7.50.

The first half of the present volume (Part I) deals with the nature and causes of the ills presently afflicting the railroad industry, while the second half (Part II) considers various remedies for these ills. The opening chapters present an extensive statistical study of the changing distribution of aggregate traffic among the various transport agencies and the diversion from the railroads of traffic in particular commodities and commodity groups. There follows a chapter describing the nature and extent of public promotion of transport facilities which concludes that "federal transport development expenditures have speeded technological innovations, increased the supply of transport, multiplied carrier and service alternatives available to shippers and travelers, and aided the economy during recent wars by supplementing railroad facilities and services" (p. 108). Nelson holds that, while these expenditures have had some unfavorable impact upon the railroads, "subsidized transport competition probably is not the principal source of long-term railroad difficulties" (p. 109). However, he recommends imposition of user charges, including provision for property tax equivalents, on all publicly provided transportation facilities.

More importance is attached to regulatory policies than to public promotional policies as a contributing cause of the present railroad difficulties. Criticism is directed at delays in permitting general rate-level increases, the granting of inadequate general increases, delays in permitting individual rate reductions through extensive use of the rate-suspension power, and, particularly, the policy of maintaining a "fair share" allocation of traffic by means of rate parity, with the consequent inability of the low cost agency to establish rates reflecting its cost advantage. Nelson's program for remedying these weaknesses is, in general, that outlined in the recent Cabinet Committee Report, which he discusses at some length, but the reviewer received the impression that Nelson would go considerably farther than the Cabinet Committee in relaxing regulation and relying upon the operation of competitive forces in transport markets.

A chapter on Effects of Traffic Shifts on the Transport Economy describes the effect of such shifts in creating excess capacity in the railway plant and considers the extent to which these shifts reflect the relative cost advantages of the different agencies of transportation. Based on a comparison of ton miles of freight produced per employee hour, per gallon of fuel consumed, and per dollar of net investment, Nelson finds that "the railroad advantage in ton-mile

productivity in terms of comparable inputs is so large that it seems reasonable to infer that the railroads may be the low-cost agency in relation to trucks for volume shipments for distances in excess of 150 miles" (p. 174). He says that, "Because of the detail involved in unit cost comparisons, inadequate data, and money value fluctuations, relative traffic outputs from given inputs of resources were substituted here for unit costs" (p. 173), although he recognizes that "If data were available for limiting input-output comparisons to identical traffic compositions, the relative productivity results would show cost differences more exactly" (p. 174).

The general conclusion is that failure to levy user charges plus the policy of maintaining rate parity have prevented an allocation of traffic in accordance with the relative costs of the different transport agencies and that "There is enough evidence to support a general judgment that total transport costs would be minimized if more of the aggregate traffic were to move by rail" (p. 186). Although the reviewer recognizes that a full treatment of relative costs would require a separate volume he is of the opinion that the present study would have been strengthened if at least some data had been presented showing comparative unit costs of different transport agencies for various lengths of haul and types of traffic. The final chapter in Part I reviews at length the generally unsatisfactory postwar earnings position of the railroads and its bearing upon their ability to raise new capital.

The steps which railroad managements might take to improve the position of the industry are considered in Part II. These are: (1) efforts to increase operating efficiency, particularly in freight car utilization, (2) reduction or elimination of the passenger deficit, (3) revision of pricing policies, and (4) the undertaking of an extensive program of modernization. These matters are given thorough consideration, although certain important topics receive only brief or incidental attention. These include the problem of the deficit on commutation traffic, the development and pricing of trailer-on-flat-car traffic, and agreed charges. The author states in the preface that consolidation is not considered because of government and carrier inaction and the lack of evidence of economies obtainable thereby.

A valuable feature of the book is the extensive statistical material presented. The volume should be welcomed by students of transportation both as a substantial contribution in its own right and as a point of departure for further studies of the individual topics which it considers.

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*Public Regulation of Business*. By DUDLEY F. PEGRUM. Homewood, Ill.: Richard D. Irwin, Inc., 1959. Pp. xviii, 732. $7.50.

Professor Pegrum states in his preface, "Public regulation of business enterprise may be approached from a number of different points of view and may be dealt with by a number of different disciplines, each with its own particular emphasis and techniques." As an economist, the author is faced with the task of bringing economic analysis to an area where most existing texts emphasize law, history, and political science. The approach used by Pegrum properly

focuses attention on the basic economic problem, efficient allocation and utilization of economic resources.

The reader will be gratified to see regulatory problems and issues—for example, the national transportation problem, the regulation of public utilities—treated simply as partial aspects of the basic economic problem: the kinds and quantities of various economic goods to be produced; the way production of the various economic goods is to be allocated among industries and fields of productive activity; the way the currently available stock of consumer goods is to be distributed among consumers; and the total quantity of each reproducible agent of production which is to be available in the long run.

The author has avoided making the book an encyclopedia of government and business and has confined his attention only to the relevant institutional, organizational and economic considerations essential to the understanding of government intervention in the market. Of the five sections, Part I sets forth the basic institutional background and describes the development of those aspects of modern business enterprise from which current problems of regulation emerge. Besides including the traditional material on concentration and mergers, the author wisely devotes one chapter to a discussion of the scale of enterprises and its relation to the significant aspects of the theory of production. In Part II, attention is centered on the pricing process as related to the efficient allocation and utilization of economic resources. This is the most important part of the book but unfortunately one of the shortest.

Part III deals with the antitrust laws and their administration, and is presented in the traditional legal manner. According to Pegrum, somewhat extensive treatment is accorded to court decisions because there does not seem to be any other way of portraying satisfactorily the present state of regulation and the critical issues of administration in this area. This statement suggests the principal defect of the work; in short, a more complete treatment of effective and workable competition would have been desirable. Many economists, J. M. Clark one of the first, recognizing the imperfections of competition and the many patterns into which it falls, have sought to develop a concept of workable competition which would differentiate between socially acceptable and unacceptable situations. In determining workability, economists such as George W. Stocking examine three factors: industrial structure, business conduct, and economic performance. Although the economic standards of workability have been debated and cannot solely be used to determine the legality of business arrangements under the antitrust statutes, Pegrum should have shown how the principle of workability can be utilized to show the existence of situations which are socially unacceptable. In fairness to the author it should be stated that no existing text has as yet successfully combined the economic standards of workability with the traditional legal approach.

Part IV deals with transportation and public utilities. The author in this part, especially with regard to transportation, shows how economic principles can be applied to public policy. Pegrum deserves high praise for this section, and his ideas merit the attention of students interested in the long-run implications of our present national transportation policy. Part V, a single chapter, sets forth the author's views on the relationship of economic policy to political

and social objectives. The author concludes with the following well-chosen words: "The issue, today, is much more than the merely technical one of devising procedures to deal with specific problems. It is the issue of freedom versus authority. The preservation of freedom demands a reaffirmation of the faith of the American people in their heritage and in the capacity of a free people to solve their political and economic problems without sacrificing that freedom" (p. 705).

Pegrum deserves high praise for this thoughtful and scholarly book, which represents a step in the right direction and should be used as a point of departure for future books in the area.

ERIC SCHENKER

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## Land Economics; Agricultural Economics; Economic Geography; Housing

*International Resources and National Policy*. By OLIN T. MOUZON. New York: Harper & Brothers, 1959. Pp. xiv, 752. $7.50.

With few exceptions, notably Ciriacy-Wantrup's *Resource Conservation*, most studies of resources have been heavily descriptive in content. Furthermore, such studies also frequently treat natural resources exclusively. In large part these generalizations hold for Professor Mouzon's *International Resources and National Policy*. Mouzon presents detailed descriptions of selected individual natural resources, recounting each resource's economic history as well as its current sources of supply and uses. Problems associated with each resource, such as instability of price and production, future prospects for markets, and the quest for new reserves or new substitutes are considered largely in an historical, which is to say descriptive, context.

Most of the book is thus occupied with natural resources. However, the plan of the work is not so restricted. Hence, "man-made facilities" and "human resources" are also introduced in an attempt to extend the scope to include all types of resources. In a further extention of scope beyond that of the typical study of resources Mouzon has attempted to tie the economics of resources geopolitics. Unfortunately the treatment of capital and labor do little more than introduce them to the reader. A disconcerting lack of balance in the over-all study is the inevitable result. The attempt to analyze the relationship between resources and geopolitics gets little further, although it exposes some of the limitations of geopolitics itself in the process.

The introduction of geopolitics reflects the author's interest in national security and the maintenance of national power. This is the dominant concern of the book. Although economic progress and freedom are mentioned as a part of the problem addressed by the book, these other goals for the economy are treated as secondary and in some respects as by-products of the achievement of national security. Why then should attention be confined to international resources? In fact it is not. Mouzon devotes substantial space to some resources important to the nation's security but which are almost exclusively domestic in source and consumption. Thus one of the fifteen chap-

ters is devoted to electric power which is treated in terms of its influence on the internal economy of the United States, except that a page or two is used to tally the production and sale of electric power for various nations. Such an approach may help gauge the national productive potential, and thus have implications for national security, but to regard such a resource as international is to give an Alice in Wonderland meaning to that term. On the other hand because of the focus on security, several natural resources which are distinctly of an international nature do not receive attention. Most important among this group are probably the fishing areas lying within international waters, but migratory resources in general may have similar international aspects.

All of this tends to destroy the unity of the book and leaves the reader in doubt as to why international resources were singled out as of especial importance for study in contrast to resources in general.

Since the book is primarily a text the descriptive material will no doubt provide some information about the economic conditions of natural resources which is new to students. Regarding policy, however, very little new is offered. Many of the policy suggestions are taken directly from the already established policies of the government or from conclusions which are the products of other, earlier studies. Arriving at the same policy conclusions is not of course necessarily undesirable. But the manner of arrival is important and it is this which is open to criticism. There is a gap between descriptive material and policy. The techniques for building policy are largely absent. Thus the process of policy formation itself is lightly treated. Often policies are enunciated with almost no economic analysis by way of support.

In the reviewer's opinion the book is unnecessarily long. It suffers from verbosity, repetition, and from many tediously protracted quotations. Moreover, while numerous footnotes identify these many long quotations, many of the shorter ones (up to four to six lines) appear without recognition being given their authors. Statistics are sometimes introduced without identification of source. Other criticisms mght be offered but they seem needless to establish the fact that the book has decided limitations.

JOHN C. MURDOCK

*University of Missouri*

## Labor Economics

*The A. F. of L. from the Death of Gompers to the Merger.* By PHILIP TAFT. New York: Harper & Brothers, 1959. Pp. xi, 499. $7.50.

This book is the second and concluding volume in Philip Taft's history of the American Federation of Labor. The first, *The A. F. of L. in the Time of Gompers*, ends with Gomper's death in 1924, and the second with the merger of the two federations in 1956. In both volumes Taft has largely limited himself to a painstaking summarization of the correspondence, memoranda, minutes and reports which have accumulated over the years in the A. F. of L.'s national office. In defense of this sharp delimitation of his sources and viewpoint, Taft writes:

. . . I wanted to delineate more sharply the activities of the organization itself, and this could be done more effectively by a narrowed vision.

Given these limits, the second volume is an impressive piece of research. The facts, though nearly overwhelming in bulk, are well organized and clearly presented. Much of the data has not been previously published. The issues discussed are for the most part worthy of attention and the treatment throughout is meticulous, balanced and scholarly. Future students will doubtless regard the book as a landmark in the field.

The second volume, like the first, treats the activities of the A. F. of L. both chronologically and topically. Subjects receiving most attention are the enactment of New Deal labor legislation, the split with the C. I. O., labor policies during the second world war, international developments after the war, Taft-Hartley and other postwar legislational developments of interest to labor and the political activities of the A. F. of L. Considerably less emphasis is placed on such questions as jurisdictional disputes, racketeering, communism and the economic aspects of A. F. of L. policies. Some of the play of personalities shows through in the letters and minutes which are quoted at length; similarly, the discussion of major policy issues reflects obliquely the power drives of union empire builders in various fields. The author, however, hews to the official record throughout and readers who lack background in labor history are apt to get a distorted view of this aspect of the labor movement.

The opening chapters, which deal with the 1924-1930 period, might well have included a discussion of the A. F. of L.'s general position in the U. S. labor movement. Instead, a rather miscellaneous list of topics such as the Federation's position on convict labor, aliens and political prisoners are considered. This is the least satisfactory section of the book.

The chapters dealing with the 1930-40 period take up about a third of the volume and are the most important of the entire study. If the A. F. of L. of the previous decade was largely moribund, it showed amazing recovery powers in the course of the next ten years. The Federation was the leader in the long battle to limit the use of injunctions in labor disputes which culminated in the Norris-LaGuardia Act in 1932. In 1929 it started a campaign to develop a government program of old age benefits, and three years later it renounced its long-standing opposition to unemployment insurance. Once the social security law was passed, the A. F. of L. became extremely active in efforts to liberalize its provisions. The Federation was also active in the movement for governmental protection of the workers' right to organize, first through the NRA and then in connection with the passage of the Wagner Act in 1935. Its part in the enactment of the wages and hours law of 1938, on the other hand, was of comparatively little importance.

The Federation's approach to the other big issue of this period, the split leading to the formation of the C. I. O., was short-sighted and almost wholly negative. The documents summarized in this volume leave no doubt that the principal spokesmen for the A. F. of L. approached the whole issue of industrial unionism in personal power terms. Taft rightly emphasizes the point that, in the very act of suspending the ten unions which had affiliated with the

Committee for Industrial Organization, the Executive Council of the A. F. of L. assumed authority which could be exercised only by the convention. The mild efforts of William Green and the more vigorous attempts by George Harrison to prevent the split seemed foredoomed from the start. Few events of equal importance and complexity can be attributed so completely to the willful obduracy of a small group of narrow-minded men.

In light of these facts, the resurgence of the A. F. of L. in the late 'thirties seems all the more remarkable. Vigorous measures were taken to prevent invasion by the C. I. O. of fields in which A. F. of L. unions were already established. Steps were also taken to extend union organization to related fields. While the methods of organizing were less dramatic than those employed by the C. I. O., the A. F. of L. enjoyed a tremendous advantage in its long-standing relationships with local unions in all parts of the country. After the suspension of the C. I. O. unions in 1936, hardly more than a year passed before the A. F. of L.'s membership was greater than it was before.

During the second world war and after, the Federation's role broadened considerably. Its importance between 1940 and 1945 in the war effort is well known. Taft's discussion of A. F. of L. activities after the cessation of hostilities, however, breaks new ground. Particularly notable is his treatment of the Federation's efforts to re-establish free trade unionism in Europe and its consistent refusal to cooperate with unions from communist-dominated countries. The British Trades Union Congress and the C. I. O. joined with these unions to form the World Federation of Trade Unions in 1945. Subsequent events were to bear out the A. F. of L.'s belief that their decision was a mistake.

Taft gives relatively little attention to the A. F. of L.'s position in the fight over the Taft-Hartley Act and later efforts to amend this law. There is nothing in his account to alter the prevailing view that the Federation showed little flexibility or imagination in dealing with this issue. In pursuing other political objectives, such as in the education and housing fields, the Federation followed more resourceful policies. After 1952, when George Meany assumed the presidency of the A. F. of L., the organization's influence was enhanced further, both in connection with the fight on internal union corruption and in the events leading to the merger with the C. I. O.

Unfortunately, Taft attempts no general assessment of the changing role of the A. F. of L. The clear inference to be drawn from the data he has assembled is that the Federation (now the A. F. of L.-C. I. O.) will play an increasingly important part in labor relations and in American life generally. The reason is not so much that it will assert more authority in areas traditionally left to the constituent unions but that, to an increasing degree, labor issues will fall in the political and international sphere. The evidence assembled in this book leaves little doubt that the political orientation of the American labor movement will increase substantially in the future, and that the A. F. of L.-C. I. O. will play a prominent role in this development.

The factual record set forth in this book throws much light on other aspects of modern unionism, such as the powers of national union officials, the appropriate unit of bargaining, the determination of rival jurisdictional claims and

the public regulation of union practices. The task of relating the data to these issues and of describing the general direction in which the Federation has been moving in recent years is left to the reader. It is to be hoped that Taft will round out his picture of the A. F. of L. in a subsequent study. Meanwhile, he has provided a solid basis on which others can build.

FRANK C. PIERSON

*Swarthmore College*

*Labor and Economic Development,* Edited by WALTER GALENSON. New York: John Wiley; London: Chapman and Hall, 1959. Pp. xiii, 304. $6.75.

This book consists of six essays by different authors, one each on five countries in different states of development (India, C. A. Myers; Japan, R. A. Scalapino; Egypt, F. H. Harbison; French West Africa, E. Berg; the British West Indies, W. H. Knowles), and an introductory chapter by the editor which also serves as a summary. The countries included represent a considerable range in the degree of development, from the very undeveloped to two which are starting on the road to development, and including Japan, which is hardly to be classified any longer as an undeveloped country.

The major theme is the relationship between labor and economic development with greater emphasis put on the influence of economic development on labor and its organization. Essentially, the outline of each essay is the same; the economic and social conditions of the country, a description of the labor force and of management, the growth of unionization and its problems, the nature of management-labor relations, the role of government and its relations to the labor movement. In most cases the union movement receives the greatest emphasis.

These essays make a definite contribution toward our knowledge of the problems of undeveloped and developing countries. Our ability to gain insights into the forces of economic growth depends on accumulating factual knowledge about every phase of economies at different states of development. This book takes one step in advancing our knowledge of one important institutional influence, the labor union. Most of the studies are straightforward factual accounts, and though in several cases in a readers-digest condensation style are admittedly concentrated versions of large works. However, the articles do not suffer as a result and present an amazing amount of material in a small space. If the reader requires further detail, he is aided by copious documentation. Only the article on Japan is involved and difficult to follow due partly to excessive repetition and to the long and involved industrial history of that country.

The reader is of course concerned with what generalizations are possible from any comparative study. These Galenson very admirably provides for the reader in his introduction. Although recognizing exceptions Galenson makes the following generalizations (in expanded form):

1. The problem of the commitment of the industrial labor force is not as great as is usually thought and what resistance exists in the form of the

attraction of the home village is advantageous to growth in lessening urban-overcrowding and in providing a form of social security.

2. The problem of acquiring skills is only transitory and is essentially a managerial problem.

3. Employer-employee relations are often marked, out of necessity, by employer paternalism.

4. In respect to the labor movement unions are sure to emerge; the leadership will rarely be from the working classes; unions will almost always be deeply involved in politics; collective bargaining will be weak, usually because of excess population and unemployment; blanket unionism will be most prevalent; unions will be fractionalized and will lack internal democracy.

5. Problems of acquiring managerial skills exist but training can alleviate this.

6. Government must inevitably play an important role in development and must, therefore, have a great effect on labor through wage policy and social legislation (often advanced but not enforced). It must fit the labor movement into its plans. This government role encourages political action by unions. Conflicts are inevitable, for example between higher wages and more adequate investment funds, higher wages and the budget, more social services and industrial investment; but unions are useful as a channel of worker protest, and thus as a means to avoid more radical movements, and so must be encouraged.

More emphasis could have been put on the role of government in encouraging unions (as it is in the individual studies). It appears doubtful from this sample that unions, in any form resembling a union, could exist without government support and that they would inevitably develop as Galenson suggests. A further generalization might have been added that the exports situation and the balance of payments are powerful limits to economic and social gains of the workers. The authors as a group should be commended for a lucid and interesting presentation of the condition of labor and labor unions in a variety of development situations.

Bruce R. Morris

*University of Massachusetts*

*A History of American Labor*. By Joseph G. Rayback. New York: Macmillan Company, 1959. Pp. vi, 459. $6.00.

It is encouraging that in the midst of the sordid findings of the McClellan Committee which accentuate the exception and make it the rule a number of serious studies of the labor movement have recently come off the press. Such prominent students of labor affairs as Philip Taft, David Saposs, and the late William Leiserson have all written books that help place the American labor movement in proper perspective. The present book on labor history is by the chairman of the departments of history and labor education at Pennsylvania State University.

Professor Rayback starts his story in colonial times. His last paragraph deals with the AFL-CIO's recent expulsion of corrupt unions. In between, he treats all of the major developments affecting the growth of the labor move-

ment as well as the impact American labor has had upon our society. He does not treat unions as if they played their role in a vacuum, but he instead takes care to trace in the background of American economic, social, political and industrial history. Rayback says that he had three major aims in writing this book: first, "to correct an all too general impression that there were no working men in America until some vague period in the nineteenth century when the factory system began and that there was no labor movement in American life until after the Civil War; second, to indicate the means used by labor to solve its problems within the context of American political and economic developments; and third, to show the influence of labor upon American institutions." Rayback should be pleased. He achieves these aims and at the same time writes an entertaining and interesting book.

Unfortunately, it is perhaps inevitable that any single-volume treatment of labor history should suffer from an unreal overemphasis upon the role of the worker in American life. This book is no exception. For example, the author, though he makes some reservations, has the workers play a far more important role during the colonial period than he or anyone else knows they played. He does this by pointing out that some workers belonged to, though they did not lead, such organizations as the Sons of Liberty. He then describes the activities of these organizations in their fight against England as if their actions or ideas necessarily reflected the actions or ideas of most working men. Rayback says in one place: "Workingmen generally recognized that a breach of the nonimportation agreement would produce a flood of British-made goods to the jeopardy of their jobs. In addition laborers had developed ideologies." What evidence does the author have to indicate what most workingmen thought? Even today, when we have better data, we have no real idea what laborers think. What the author did, and what we all frequently do, is assume that because we believe that workingmen should have thought or acted in a certain way, consequently most of them did. We protect ourselves by adding the word "generally." The author does this again when he says that workingmen "generally" supported wage and price controls during the Revolutionary War. I presume that in both examples the author placed the word "generally" there to protect himself from evidence to the contrary, but my question is more basic. How does he know that it was even "generally" true? It would indeed be a brave writer who after much research wrote "I do not really know but I think or have a hunch that . . ." Rayback would have been on sounder ground if he had done this more frequently but I wonder how many publishers, or learned journals, would accept such a manuscript.

Nonetheless, Raybeck's book is a worthwhile addition to the very small collection of one-volume labor histories now available. As such, it ought to be seriously considered as a textbook or supplementary reading in a labor or economic history course. Anyone can read it and as a result have a better understanding of the important role played by labor in American society of yesterday and today.

Albert A. Blum

*The American University*

*The Crisis of American Labor.* By SIDNEY LENS. New York: Sagamore Press, 1959. Pp. 318. $6.00.

*Labor, USA.* By LESTER VELIE. New York: Harper & Brothers, 1959. Pp. xv, 318. $4.95.

Here are two very candid "inside" looks at the labor union, its philosophy and practice, in the United States today. One, *Labor, USA*, in purple prose, is the report by a roving editor of *Reader's Digest*. The other, by a local union leader in Chicago, is equally colorful but far more analytical. Together they summarize what the layman can or should know about the McClellan hearings. They are disturbing reading and are apparently intended to be. The academic economist will find, perhaps not to his surprise, that neither book is really an exposé and that the recommendations each author makes are neither new nor radical. Even Velie, whose use of words is certainly designed to shock his readers, is found finally to suggest relatively minor changes in the Taft-Hartley Act.

Lens is disturbed about the meaning of business unionism. He argues that the materialism in labor organization has led to undemocratic unions, self-serving leaders and racketeering. He traces the development of business unionism from the early days of the A. F. of L. and finds the roots of today's scandals already well planted by the turn of the century in such unions as the Carpenters and the Mine Workers. The obvious alternative is a philosophy in which mere gain is somehow tuned down. For Lens, then, Reuther is more of a hero than Hoffa. Interestingly he finds, as does Velie, that Hoffa is being forced more and more to rely on such "egg-heads" as Harold Gibbons and Eddie Cheyfitz and that this is a good augury for the Teamsters.

Of particular value in both books are the vignettes of labor leaders in action. Velie gives us shorts on Hoffa, Reuther, Dubinsky, MacDonald, Meany, Gompers, Lewis, and A. Philip Randolph. Lens emphasizes the contrast between Reuther and Hoffa, who are the "symbols of the conflict in the house of labor today . . . each operating at opposite poles of union philosophy." Each is a seeker after power, Hoffa for its own sake, Reuther to serve some socially (or should we say socialistically?) defined end. Hoffa is the epitome of business unionism, which Lens calls "the victory of materialism." He is the proper heir of Gompers. Reuther is a revolutionary or, better, a liberal. "For Walter Reuther the labor movement was the natural outlet of a socialist philosophy, the finest goal he could achieve. For Jimmy Hoffa, it was a field he stumbled into by accident, unrelated to his personal *Weltanschauung*—assuming that he ever had any" (Lens, p. 138).

Lens finds in the "business agent's position such a reservoir of power that he could, if freed from checks and balances, achieve a status far beyond his rank-and-file members." The movement away from democratic unionism has led to opportunities to take advantage of this key position and of the "give" in capitalism which makes possible a policy of accommodation with the employers. But if Lens finds the business agent's position the focal point for the betrayal of union interests, Velie regards the union leader as "a man whose power seems neither checked nor balanced." He gets his power not so much

from the rank-and-file as from the government, which also helps him keep it. Velie's answer: the government must make a public servant of the union leader, must put him in a "goldfish bowl." Power must be balanced with responsibility.

My impression is that Velie misses the point, for he never understands the economic and social role of the union despite his massive, though often misinterpreted, research. Lens is clearly the more careful scholar. He sees the only real hope of labor to lie in a search for an ideal "if we are to have a participative rather than a manipulative democracy." Both authors can be read with profit on the plight of the union today. Each in his own light is fair and thorough. Neither is antilabor nor too obviously prolabor. But their usefulness to students of the labor movement is in exposing the basic dilemma in the house of labor. What is the reason for union organization in the United States of 1959?

Charles J. Stokes

*Atlantic Union College*

*American Labor in Midpassage.* Edited by Bert Cochran. New York: Monthly Review Press, 1959. Pp. xi, 196. $3.50.

Eleven writers discuss critically from a socialist point of view the trade union movement of the United States and its prospects. To a significant degree the conclusions reached are explicitly reactions against the generalizations developed by the "Wisconsin School." The title chapter, written by the editor, occupies one-third of the book. Other topics include: the conditions of the working class; cultural exploitation; class war; labor and politics; automation; white collar workers; working women; Negroes; union corruption; books on labor; and the Taft-Hartley decade. Throughout, comments on "stultifying business unionism" have an informed bite. To many readers these observations and the interpretations of recent union developments will be of greater interest than the prognostications.

The loss of contact between radicalism and the trade unions is viewed as unfortunate for both sides. It is when workers are in motion and the ranks are concerned with alternate lines of official policy that democracy thrives. Unless leaders are available, a rank and file group is helpless when confronting an entrenched union administration (pp. 187-88). Further, it is radicalism which provides a higher ethic to insulate unions against corrupt leadership (p. 164). Thus a radical minority provides an essential service for the preservation of membership participation in union government.

The outlook for the future is described in Marxian terms. After pointing out that union membership gains have been made in periods of great upheaval, it is suggested that the next decade may produce a popular need to "gain control of the levers of government to ensure economic growth and to reorient our foreign relationships" (p. 39). The resulting struggle may shake up existing union leadership and "provoke a more ambitious lunge to make government serve the common man's needs" (p. 38).

As to unions, "since no one has yet discovered a new social force . . . which

can usher . . . inevitable social changes into our rudderless industrial society now gone amuck, the conviction must be maintained . . . that the labor movements, each in its own way and time, will rise to the historic needs of our epoch" (p. 64). On the political scene the conviction persists that a labor party, probably modeled after the British Labour Party, is inevitable.

The tone of the book is one of defeat. There is no suggestion of means by which socialists might regain some measure of influence in unions. Limited socialist roles of observer, analyst and critic are implicitly accepted. There is no suggestion of an activist program. Several of the expressions of future expectations sound more like a hope for an awakening of the ethical perceptions of the masses than like Marxism. The socialists appear to accept not only the loss of all ties with the unions, but also with the workingmen of the country.

RUSSELL S. BAUDER

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# NOTES

Members who wish to make suggestions for officers of the American Economic Association for 1960-1961 are invited to place names with James Washington Bell, secretary of the Association, for transmission to the Nominating Committee, which will be appointed by the incoming President.

## Fellowships for Regional Faculty Research Seminars in Economics

The Ford Foundation announces six Regional Research Seminars in Economics to be held in the summer of 1960 for an eight-week period. Participation is open on a competitive basis to faculty members teaching economics or business subjects at institutions of higher learning that do not offer a doctorate in economics. It is the purpose of the program to enhance the effectiveness of teaching and to encourage research studies of significance.

The regions, directors, and subject areas will be the following: I, Northeast, William S. Vickrey, Columbia University, Economic Analysis and Welfare; II, Middle Atlantic, Dorothy S. Brady, University of Pennsylvania, Consumer Economics; III, Southeast, Rendigs Fels, Vanderbilt University, Empirical Applications of Microeconomic Theory; IV, Middle West, Gardner Ackley, University of Michigan, National Income—Problems of Stability and Growth; V, Northwest, Douglass C. North, University of Washington, Economic History and Economic Growth; VI, Southwest, Melvin W. Reder, Stanford University, Labor Economics.

Fellowships provide a stipend of $800, a $200 allowance for living expenses, and travel expenses. Preference will be given to applicants under 50 years of age who have had 3 years or more teaching experience since attending graduate school. Applications may be secured from the seminar director in the applicant's region, and must be submitted before January 15, 1960. Awards will be announced not later than March 1.

## Asia Foundation Grant

A grant of $2,500 has been awarded to the Association by the Asia Foundation to be used for the general purpose of bringing closer association between American and Asiatic economists. Two ways in which these funds may be used are: (1) to enable Asian graduate students and postdoctoral scholars in economics who are in the United States to attend meetings of the Association; (2) to make available memberships in the Association to selected Asian scholars in this country or resident in Asia. Members of the Association who wish to make recommendations of names of Asian scholars to receive either type of award are invited to write the secretary of the Association, Dr. James W. Bell, Northwestern University.

## New Publication

The Joint Council on Economic Education (2 West 46th Street, New York 36) announces the publication of *Abstracts of Original Research,* to be made available to economist subscribers at $5.00 for 10 issues; institutional subscriptions will be $10.00. Among the works for which abstracts have been, or shortly will be, prepared are: *The Labor Force under Changing Income and Employment,* by C. D. Long (abstract by V. H. Jensen); *Anti-Trust Policies: American Experience in Twenty Industries,* by S. N. Whitney (abstract by G. W. Stocking); *Pricing in Big Business,* by Kaplan, Dirlam, and Lanzillotti (abstract by E. G. Nourse); *Federal Budget and Fiscal Policy 1789-1958,* by L. H. Kimmel (abstract by C. L. Harriss); *Financial Intermediaries in the American Economy since 1900,* by R. W. Goldsmith (abstract by W. N. Peach).

## *Announcements*

The first International Conference of Economic History will be held in Stockholm, Sweden, August 17 and 18, 1960. The program will revolve around the subjects (1) Industrialization as a Factor in Economic Growth after 1700 (*rapporteur:* K. E. Berrill, University of Cambridge, England) and (2) Comparative Study of Large-Scale Agricultural Enterprise since the Middle Ages (*rapporteur:* F. Lütge, University of Munich, Germany). Applications for registration should be sent not later than May 31, 1960 to: International Conference of Economic History, Riddargatan 11B, Stockholm O, Sweden. Applicants should state whether they are also registering for the main Congress of Historical Sciences.

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As an outgrowth of three biennial conferences, the first of which was held in 1955, a regional organization, the Pennsylvania Conference of Economists, has been formed. Officers for the current biennium are: Howard M. Teaf, Jr., president; David P. Eastburn, Winthrop Everett, vice presidents; David H. McKinley, secretary; Ronald McCrady, treasurer. The executive committee is made up of the officers and Richard A. Easterlin, Albert L. Gray, Jr., Harold Hinkelman, and John W. May.

## *Deaths*

Guy Freutel, Federal Reserve Bank of St. Louis, August 29, 1959.

Laurence R. Gray, University of Arizona, July 7, 1959.

Jack N. Ockerlander, April 19, 1959.

Selig Perlman, University of Wisconsin, August 13, 1959.

Lloyd P. Rice, May 10, 1958.

Sumner H. Slichter, Harvard University, September 27, 1959.

John K. Towles, May 2, 1959.

Fred D. Wish, Jr., December 28, 1958.

## *Retirements*

Reid S. Fulton, The City College, New York, June 1959.

F. R. E. Mauldon, University of Western Australia.

Hans Staudinger, The New School of Social Research, June 1959.

W. H. Steiner, Brooklyn College, September 1959.

Charles H. Waterfall, University of South Carolina, August 1959.

## *Visiting Foreign Scholars*

Keith O. Campbell, University of Sydney: visiting professor, University of Illinois.

Michael J. Farrell, Cambridge University: visiting associate professor, Carnegie Institute of Technology, autumn semester.

Frank H. Hahn, University of Birmingham: visiting professor of economics, University of California, Berkeley.

T. W. Hutchison, University of Birmingham: distinguished visiting scholar at the Thomas Jefferson Center for Studies in Political Economy, University of Virginia, spring term, 1960.

Nicholas Kaldor, King's College, Cambridge: visiting research professor of economics, University of California, Berkeley.

A. M. Kerr, University of Western Australia: visiting professor of economics, University of Illinois, 1960.

Abu N. M. Mahmood, Dacca University, Pakistan: lecturer in economics, Montana State University, 1959-1960.

Bertil Ohlin, University of Stockholm: distinguished visiting scholar, the Thomas Jefferson Center for Studies in Political Economy, University of Virginia, fall semester, 1959-1960.

Afife Sayin, Institute of Economics and Sociology, University of Istanbul: visiting fellow, New York State School of Industrial and Labor Relations, Cornell University.

Brinley Thomas, University of Wales: George A. Miller distinguished professorship, University of Illinois.

### *Promotions*

Leon Applebaum: assistant professor of economics, University of Wisconsin—Milwaukee.

Wallace N. Atherton: assistant professor of economics, Michigan State University.

L. J. Benninger: professor of accounting, University of Florida.

Harry G. Brainard: professor of economics, Michigan State University.

Robert Campbell: associate professor of economics, University of Oregon.

P. William Capatch: assistant professor of accounting, School of Business Administration, University of Pittsburgh.

Richard E. Caves: associate professor of economics, University of California, Berkeley.

Edward J. Cook: associate professor of economics, Fordham University.

Willard M. Craig: professor, department of business administration, Oregon State College.

Dale Cramer: associate professor of economics, University of Alabama.

Riley S. Dougan: associate professor, department of agricultural economics, Ohio State University.

Edgar S. Dunn, Jr.: professor of economics, University of Florida.

Roland T. Ely: assistant professor of economics, University College, Rutgers, The State University.

Eleanor Emerson: professor, New York State School of Industrial and Labor Relations, Cornell University.

Joseph R. Ewers: assistant professor of business administration, School of Business, Indiana University.

Wytze Gorter: professor of economics, University of California, Los Angeles.

Richard W. Graves: assistant professor of business economics and public policy, School of Business, Indiana University.

Kurt L. Hanslowe: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Donald V. Harper: associate professor, University of Minnesota.

Joseph R. Hartley: associate professor of transportation, School of Business, Indiana University.

Franklyn D. Holzman: professor of economics, University of Washington.

John H. Kareken: associate professor of economics, University of Minnesota; on leave, 1959-1960 with Douglas Subcommittee on Inflation and Economic Growth, the Brookings Institution, and the National Monetary Commission.

H. T. Koplin: associate professor of economics, University of Oregon.

Robert F. Lanzillotti: professor of economics, Washington State University.

Roy L. Lassiter: associate professor of economics, University of Florida.

Lester S. Levy: associate professor of economics, Texas Technological College.

John D. Long: professor of insurance, School of Business, Indiana University.

Ragaei El Mallakh: assistant professor of economics, University of Colorado.

D. D. Martin: associate professor of business economics and public policy, School of Business, Indiana University.

Francis B. McCormick: associate professor, department of agricultural economics, Ohio State University.

Margaret F. McDonald: associate professor, department of agricultural economics, Ohio State University.

Frank Miller: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Hyman P. Minsky: associate professor of economics, University of California, Berkeley.

John H. Mudie: assistant vice-president of the Government Development Bank for Puerto Rico.

John L. O'Donnell: associate professor of accounting and finance administration, School of Business and Public Service, Michigan State University.

Russell O. Olson: professor, department of agricultural economics, Ohio State University.

Harlan Perrins: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Robert F. Risley: professor, New York State School of Industrial and Labor Relations, Cornell University.

Robert Rivers: associate professor of finance and transportation, School of Business Administration, University of Massachusetts.

Ross M. Robertson: professor of business economics and public policy, School of Business, Indiana University.

Henry Rosovsky: associate professor of economics, University of California, Berkeley.

Walter J. Salmon: lecturer in business administration, Graduate School of Business Administration, Harvard University.

Donald H. Sauer: assistant professor of finance, School of Business, Indiana University.

Stuart B. Seaton: professor, department of business administration, Oregon State College.

George Seltzer: professor, University of Minnesota.

George B. Simmons: faculty lecturer in business administration, School of Business, Indiana University.

Jack Stieber: professor, department of economics, and director, Labor and Industrial Relations Center, Michigan State University.

James H. Street: professor of economics, University College, Rutgers, The State University.

Lester B. Strickler: associate professor, department of business administration, Oregon State College.

Howard L. Timms: professor of management, School of Business, Indiana University.

Roswell G. Townsend: professor of economics, Wilson College.

Robert W. Tufts: professor of economics, Oberlin College.

James R. Wason: lecturer in economics, College of General Studies, The George Washington University.

Donald A. Weisner: assistant professor, School of Business Administration, University of Miami.

Jack R. Wentworth: assistant professor of business administration and associate director, Bureau of Business Research, School of Business, Indiana University.

Albert K. Wickesberg: professor, University of Minnesota.

George W. Wilson: associate professor of transportation, School of Business, Indiana University.

L. Zakuta: assistant professor, department of political economy, University of Toronto.

Edward Zane: assistant professor of marketing, School of Business Administration, University of Massachusetts.

Arnold Zellner: associate professor, University of Washington.

H. Jerome Zoffer: associate professor of urban land studies and insurance, School of Business Administration, University of Pittsburgh.

## *Administrative Appointments*

Emile Bouvier, Georgetown University: president, University of Sudbury, Ontario.

Andrew F. Brimmer: executive officer, department of economics, Michigan State University.

Louis F. Buckley: regional director, Bureau of Labor Statistics, U. S. Department of Labor, Middle Atlantic Region.

Lorne D. Cook: acting chairman, department of economics, Pomona College.

G. Heberton Evans, Jr.: dean, faculty of philosophy, Johns Hopkins University; also continues as chairman, department of economics.

Robert A. Gordon: chairman, department of economics, University of California, Berkeley.

Melvin L. Greenhut, Florida State University: associate dean, School of Business, and professor of economics, University of Richmond.

Thomas K. Hitch, Hawaii Employers Council: vice-president and director of economic research, Bishop National Bank of Hawaii.

Philip G. Hudson: head of department of economics, University of Arizona.

William E. Koenker: chairman, department of economics, and director, Bureau of Business and Economic Research, University of North Dakota.

Paul B. Kohberger: acting dean, School of Business Administration, University of Pittsburgh.

Berndt L. Kolker: director of research, University of Kansas City.

Jacob Loft: chairman, department of economics, Brooklyn College.

Shelley M. Mark: director, Economic Research Center, University of Hawaii.

Joseph F. Marsh, Jr., Dartmouth College: president, Concord College, West Virginia.

John W. McConnell, formerly dean, Graduate School: dean, New York State School of Industrial and Labor Relations, Cornell University.

Howard D. Marshall: chairman, department of economics, Vassar College.

Charles N. Millican, Hardin-Simmons University: dean, College of Business Administration, State University of South Florida, Tampa.

Arvella Payne, Jacksonville State College, Alabama: chairman, department of economics, Henderson State Teachers' College, Arkansas.

Henry Rosovsky: vice chairman, department of economics, University of California, Berkeley.

J. Everett Royer: associate dean and professor of accounting, School of Business Administration, University of Miami.

Raymond D. Thomas: head of department of economics and business administration, Drury College.

Albert K. Wickesberg: chairman, department of business administration, University of Minnesota.

Howard A. Zacur: assistant dean and professor of accounting, School of Business Administration, University of Miami.

## *Appointments*

Mark A. Alexander: instructor in industry, School of Business Administration, University of Pittsburgh.

A. Asimakopulos: assistant professor of economics, McGill University.

G. Robert Averitt, Michigan State University: instructor in economics, Carnegie Institute of Technology.

Frank T. Bachmura: Fulbright professor, Escuela de Estudios Economicos Latino-americanos para Graduados, University of Chile.

Stephen J. Barres: professor of management and chairman, Division of Business Administration and Economics, College for Men, University of San Diego.

James W. Beck, University of North Dakota: research economist, International Business Machines, White Plains, N.Y.

William R. Belmont: assistant professor of economics, University of North Dakota.

James Berna, formerly Georgetown University: Institute of Economic Order, India.

Gerhard Biedermann: instructor in economics, University of North Dakota.

Donald R. Booth: instructor in economics and business administration, Chapman College.

Ivor I. Bowen, University of Hull: chair of economics in the University of Western Australia.

Bernard G. Brown: assistant professor of economics, Florida State University.

Phillips H. Brown: member of staff of the Bureau of Business and Economic Research, College of Business Administration, University of Arkansas.

Webster Cash: associate professor of economics, Florida State University.

Robert C. Cauthorn, formerly of Tulane University: assistant professor of economics, Arizona State University.

Gordon K. C. Chen, Ithaca College: assistant professor of business administration, Rutgers University.

A. Keith Collins, University of Melbourne, Australia: assistant professor, New York State School of Industrial and Labor Relations, Cornell University.

Salvatore Comitini: post at Oregon State College, Corvallis.

Eaton H. Conant: faculty lecturer in management, School of Business, Indiana University.

Mary Conlon: special lecturer, department of economics, Hofstra College, fall semester.

L. L. Crum, University of Texas: assistant professor of finance, University of Florida.

William C. Davis: professor of marketing, College of Business and Public Administration, University of Arizona.

E. Devletoglou: lecturer, The City College, New York City.

K. E. Dunlop, Hollins College: East Carolina College, Greenville, N.C.

Harold J. Ecker, Ohio State University: Michigan State University.

Alexander Eckstein, Harvard University: professor of economics, University of Rochester.

Ralph C. Epstein: resigned as chairman of department of economics; appointed research professor of economics, University of Buffalo.

James E. Estes, University of Texas: assistant professor of management, University of Arkansas.

Martin T. Farris: associate professor of economics, Arizona State University.

Philip Faucett, Northwestern University: instructor in economics, University of Illinois.

Max E. Fieser: assistant professor of economics, Arizona State University.

Roger W. Fingado: market analyst, International Business Machines, White Plains, N.Y.

Donald A. Fink, Yale University: research associate, Carnegie Institute of Technology.

Charles S. Franklin: instructor in economics, University of Tennessee.

James M. Fremgen: faculty lecturer in accounting, School of Business, Indiana University.

Lowell E. Gallaway, Colorado State University, Fort Collins: to faculty of San Fernando Valley State College.

Irving J. Goffman, Duke University: assistant professor of economics, University of Florida.

Arthur Goldberger, Stanford University: associate professor of economics, University of Wisconsin.

Paul J. Gordon: associate professor of management, School of Business, Indiana University.

William E. Gordon: associate professor of economics and head of the economics department, Park College.

W. E. Grasham: lecturer in department of political economy, University of Toronto.

Harold R. Hartzler, formerly University of Wisconsin: assistant professor of economics, West Virginia University.

Samuel P. Hayes, director Foundation for Research on Human Behavior, Ann Arbor: professor of economics, University of Michigan, 1959-60.

Harmon H. Haymes: assistant professor of economics, Smith College.

William A. Heffelfinger: staff of Bureau of Business and Economic Research, University of Arkansas.

Robert Hewitt: research associate, New York State School of Industrial and Labor Relations, Cornell University.

Uldis E. Inveiss: to University of Wisconsin-Milwaukee.

Shirley Johnson: lecturer in economics, The City College, New York City.

E. Kanovsky: lecturer in economics, The City College, New York City.

Jacob J. Kaplan, formerly representative to the European Monetary Agreement in Paris: assistant coordinator for program, Office of the Under-Secretary of State, Washington, D.C.

William M. Keane: assistant professor of accounting, College of Business Administration, University of Florida.

Milo Kimball: lecturer in business finance and investments, University of Delaware, 1958-59.

Leo P. Kloos, Western Reserve University: lecturer in management, University of Miami, School of Business Administration.

Robert E. L. Knight: assistant professor of economics, University of New Mexico, 1959-60.

Charles Kretzschmar, Bureau of the Census: assistant professor, Commerce Division, Ferris Institute.

Sherman Krupp: assistant professor of economics, Florida State University.

David T. Lapkin: associate professor of economics, School of Business Administration, University of North Carolina.

Tack S. Lee: instructor in economics, Illinois Institute of Technology.

Eugene Lerner: assistant professor, The City College, New York, N.Y.

Cyril C. Ling: faculty lecturer in management, School of Business, Indiana University.

Elmer P. Lotshaw, Washington University; market economist, Pet Milk Company, St. Louis.

George Macesich: assistant professor of economics, Florida State University.

John W. Manning: professor of finance, School of Business, University of Louisville.

Peter Max, Cornell University: research associate, Carnegie Institute of Technology.

J. N. McLeod: lecturer, department of political economy, University of Toronto.

Taylor W. Meloan, Indiana University: professor of marketing, University of Southern California.

Janet K. Messing: instructor, department of economics, Hunter College.

David N. Milstein, Rutgers University: research associate, Resources for the Future.

Jacob A. Mincer: visiting assistant professor of economics, Columbia University 1959-60.

Daniel C. Morgan, Jr.: assistant professor of economics, University of Tennessee.

Dean Morse: lecturer, The City College, New York, N.Y.

Clark E. Myers, retired from Ohio University: director, Management Development Institute (IMEDE), Lausanne, Switzerland.

Wladimir Naleszkiewicz: assistant professor and acting head of economics department, St. Benedict's College, Atchison, Kansas.

Robert P. Nelson: assistant professor of accounting, department of business administration, Oregon State College.

Sylvia Ostry: assistant professor of economics, McGill University.

Pranas Padalis, formerly of Voice of America: associate professor of economics, Arizona State University.

Braxton Patterson, Michigan State University: instructor, department of economics, University of Illinois.

Frances Perkins, former Secretary of Labor: visiting lecturer, New York State School of Industrial and Labor Relations, Cornell University.

Leroy Qualls, University of Florida: member of staff, Florida State budget director.

C. Hock Quan, of University of Kentucky Bureau of Business Research: professor, Carthage College.

Sher J. Rana: instructor, University of Alaska College, Alaska.

George F. Rohrlich, formerly Department of Labor: principal member, Social Security Division, International Labor Office, Geneva, Switzerland.

Milton R. Russell: instructor in economics, Iowa State University (Ames).

Roy J. Sampson, Texas Technological College: assistant professor of transportation, School of Business Administration, University of Oregon.

Robert L. Sandmeyer: instructor in economics, Oklahoma State University.

G. T. Schwenning: visiting lecturer in industrial management, Duke University, 1959-60.

Samuel N. Seidman: instructor in economics, Douglass College, Rutgers University.

Harvey Shapiro: instructor in economics, department of economics, Iowa State University (Ames).

Edwin B. Shultz, formerly with Tennessee Valley Authority: visiting professor, New York State School of Industrial and Labor Relations, Cornell University.

Jacob Shumelda: assistant professor of economics, University of North Dakota.

Gerald Sirkin: assistant professor, The City College, New York, N.Y.

Per Sjogren: assistant professor of finance, department of business administration, Oregon State College.

Jack W. Skeels, Michigan State University: to staff, Michigan Wayne State Institute for Labor and Industrial Relations and department of economics, Wayne State University.

Fred Slavick, formerly State University of Iowa: visiting associate professor, New York State School of Industrial and Labor Relations, Cornell University.

Karl U. Smith: Ford Foundation visiting professor of business administration, School of Business, Indiana University.

Stephen P. Sobotka: research associate, Graduate School of Business, University of Chicago.

Edwin O. Stene: Ford Foundation visiting professor of business administration, School of Business, Indiana University.

Robert M. Stern: instructor in economics, Columbia College.

Charles F. Stewart: visiting associate professor of business administration, School of Business, Indiana University.

Paul Sultan, University of Southern California: professor of economics, Claremont Graduate School.

Norman Sun, Park College: chair in economics, International Christian University, Japan.

Donald F. Swanson, Bellarmine College, Louisville, Ky.: assistant professor of economics, School of Business Administration, University of South Carolina.

Shanti S. Tangri: lecturer in economics and social sciences, University of California, Berkeley.

Arlene Theuer, formerly Cleveland Metropolitan Survey staff: research associate, University of Kentucky Bureau of Business Research.

Louis Vargha, Michigan State University: research associate, University of Kentucky Bureau of Business Research.

Franklin V. Walker, University of Southern California: assistant professor, department of economics, Pomona College.

Benjamin N. Ward, Jr.: assistant professor of economics, University of California, Berkeley.

John D. Wells, University of Texas: assistant professor of economics and statistics, University of Florida.

Samuel Z. Westerfield: visiting professor of business administration, Graduate School of Business Administration, Harvard University.

Franklin S. Williams: professor of marketing, College of Business Administration, University of Arkansas.

Donald N. Winch: lecturer in economics, University of California, Berkeley.

J. N. Wolfe, University of Toronto: visiting professor, Purdue University.

Walter Woodworth, University of Michigan: first Bailey memorial professor of money, banking and finance, University of Illinois.

Frederick Zeller, University of North Dakota: instructor, Ohio State University.

### *Leaves for Special Appointments and Assignments*

Floyd A. Bond, Pomona College: director of business-education division, Committee for Economic Development.

Kenneth E. Boulding, University of Michigan; visiting professor of economics, University College of the West Indies 1959-60.

Robert W. Bradbury, University of Florida: Fulbright lecturer, University of Paraguay, fall term.

Louis K. Brandt, University of Mississippi: to teach in the University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Ralph Bristol, Rand Corporation: visiting assistant professor, University of Washington, autumn quarter.

James E. Chace, University of Florida: visiting professor of business administration, University of Arizona, 1959-60.

Hollis B. Chenery, Stanford University: advisor on economic planning to government of Israel under U.N. Technical Assistance Program, fall 1959.

Rudolph Corvini, New York State School of Industrial and Labor Relations: research staff, Socony Mobil Oil Co.

Donald E. Cullen, formerly Cornell University: visiting associate professor, department of economics, University of California, Los Angeles.

Harold W. Davey, Iowa State University (Ames): School of Advanced International Studies of the Johns Hopkins University, Bologna, Italy.

Tom E. Davis, University of Chicago: visiting professor, department of economics, Cornell University, fall term.

Malcolm Davisson, University of California, Berkeley: visiting professor, University of Washington, winter and spring quarters 1960.

Bruce Edwards, University of Illinois: to teach in University of Nommensen, Medan, Indonesia, two years.

R. B. Eutsler, University of Florida: lecturer, University of Indonesia, Djakarta, Indonesia.

Joseph W. Garbarino, University of California: visiting professor, New York State School of Industrial and Labor Relations, Cornell University.

Richard K. Gaumnitz, University of Minnesota: consultant on management education, European Productivity Agency, Paris, France.

Hourmouzis G. Georgiadis, Cornell University: research fellow, Brookings Institution, 1959-60.

James A. Gherity, Jr., Michigan State University: assistant professor of economics, MSU-Oakland, 1959-60.

Zvi Griliches, University of Chicago: research associate, National Bureau of Economic Research, 1959-60.

Robert S. Herman, New York State Division of the Budget: visiting professor in public administration, Indian Institute of Public Administration.

Malcolm W. Hoag, Rand Corporation: member of faculty, National War College, Washington, D.C., 1959-60.

T. Edward Hollander, Duquesne University: technical assistant to director of professional development, American Institute of Certified Public Accountants, 1959-61.

Vernon H. Jensen, New York State School of Industrial and Labor Relations: lecturer, University of Leeds, England.

Frederick C. Joerg, Duke University: lecturer, Institut pour l'Etude des Methodes de Direction de l'Entreprise (IMEDE), Lausanne, Switzerland.

Donald L. Kemmerer, University of Illinois: Fulbright lecturer, University of Montpelier, France.

Milton R. Konvitz, New York State School of Industrial and Labor Relations: consultant to the government of Liberia on revision of the labor code; also to study at the Institute for Advanced Study, Princeton.

Harvey J. Levin, Hofstra College: Brookings National Research professorship for 1959-60.

Ben W. Lewis, Oberlin College: consultant to the Economic Development Board, Kingdom of Jordan, fall semester.

Clayton P. Libeau, North Carolina State College: University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Carl C. Malone, Iowa State University (Ames): to spend fall in India as consultant to the Ford Foundation in connection with India's food production program.

Harry E. McCallister, State College of Washington: research associate, National Bureau of Economic Research, 1959-60.

David Ott, University of Maryland: research fellow, Brookings Institution, 1959-60.

Peter Palmer, Long Beach State College: Fulbright appointment, University College of Rhodesia and Nyasaland, Salisbury, Southern Rhodesia, 1959-60.

Robert N. Schweitzer, University of California, Berkeley: research fellow in economics, Brookings Institution, 1959-60.

Frederick J. Seubert, University of Oregon: member of an economic advisory group in Korea set up by the International Cooperation Administration, Government of Korea and University of Oregon.

George A. Spiva, University of Tennessee: teaching appointment in the University of Wisconsin-Ford Foundation project at Gadjah Mada University, Jogjakarta, Indonesia.

Marshall D. Wattles, University of Oregon: adviser to economic development council in Korea, 1959-60.

Willis D. Weatherford, Swarthmore College: adviser on community development to government of Malaya for U.N. Technical Assistance Administration.

Samuel Z. Westerfield, Atlanta University: visiting professor of business administration, Graduate School of Business Administration, Harvard University, 1959-60.

Nathaniel Wollman, University of New Mexico: on research project, Resources for the Future, Inc., Washington, D.C.

## *Resignations*

David S. Carlson: School of Business Administration, University of Pittsburgh.

Samuel B. Chase: department of economics, University of Illinois.

Roy Elliott: department of economics, Wayne State University.

Charles J. Grayson, Jr.: Graduate School of Business Administration, Harvard University.

Tyler F. Haygood: School of Business, University of Louisville.

Fred W. Kniffin: School of Business, Indiana University.

Michael Levy: department of economics, Columbia College.

Nelle P. Lewis: Bureau of Business Research, University of Kentucky.

John McCalley: The City College, New York, N.Y.

Bruce D. McSparrin, Jr.: School of Business, Indiana University.

Taylor W. Meloan: School of Business, Indiana University.

James T. S. Porterfield: Graduate School of Business Administration, Harvard University.

Henry N. Sanborn: department of business and economics, Illinois Institute of Technology.

Henry K. Shearer: Bureau of Business and Economic Research, Montana State University.

Richard H. Slavin: School of Business Administration, University of Pittsburgh.

T. H. Smith: department of economics, School of Industrial Management, Purdue University.

Donald M. Soule: department of economics, University of Illinois.

## *Miscellaneous*

William H. Nicholls, Vanderbilt University: president elect of American Farm Economic Association to serve in 1960-61.

J. S. Raj, director Asian department, International Monetary Fund: has returned to India for permanent residence.